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Neil Smelser

NEIL SMELSER: DISTINGUISHED SOCIOLOGIST, UNIVERSITY PROFESSOR AND
SERVANT TO THE PUBLIC

Interviews conducted by
Jess McIntosh and Lisa Rubens
in 2011-2012

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Berkeley — The culture of suspicion in academia — Loyalty to Berkeley while ready to leave — Planning on becoming Director of the Center for Advanced Studies in the Behavioral Sciences (CASBS) — Intellectual work during the late 1980s and early 1990s — The state of sociology during this period of time — Richard Jessor and the Colorado Conference — Economics as Catholicism, Sociology as Protestantism — The International Sociological Association (ISA) and politics of international sociology — Cold War politics and "Third World sociology" — Invitation from James Coleman to contribute to *Rationality and Society* — Critiquing Rational Choice Theory — Exchange with Gary Becker — The "Chicago School" — Argument against Becker receiving the Nobel Prize — Relationship with Kai Erikson — Talk at Yale on "Sociology as Science, Humanism, and Art" — Presaging his Simmel Lectures — "Centrifugality," "conflict," and "accommodation" in the field of sociology — Election to the National Academy — Dispute over Talcott Parsons' rejection from the National Academy

[Audio File 32]

547

History of The Center for Advanced Studies in the Behavioral Sciences (CASBS) — Invitation from Gardner Lindzey to join the Board of Trustees — Chairing a subcommittee of the board — Being passed over for Director in 1988 — Thoughts on the director's position — Becoming chair of the board — Conflict over the Ronald Reagan Library — Responsibilities as chair — Becoming Director in 1993 — The purpose of CASBS — How it differs from a university — Smelser's aspirations as Director — Criteria for selecting fellows — Innovations made as Director — Psychiatric support for fellows — His first-year international initiative — Funding and Fundraising — Locating and selecting scholars — Special programs while Director — Informal project groups — Socializing with the fellows — Initiative on cultural trauma with Jeffrey Alexander — Producing *Diversity and its Discontents* and *Cultural Trauma and Collective Identity* — Simmel Lectures at Humboldt University — Framing *Problematics of Sociology* — Issues of macro, meso and micro levels of analysis

Interview #17 August 22, 2011

[Audio File 33]

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Thoughts on advising history dissertations — Reflections on historian William Sewell — More on *Problematics of Sociology*: role of affect and emotion; trust and allegiance — Talcott Parsons' essay on the international system, solidarity, and allegiance — The "global" level of analysis — Critical goals of *Problematics* — A suicide at the Center — The "California speech" on the dark side of life in a utopia — Methods of selecting fellows for the Center — Special project on cultural trauma — *Cultural Trauma and Collective Identity* — Adding an essay on 9/11 — Other special projects — Funding problems at CASBS — Merging CASBS with Stanford — Relationship with trustee and Citibank CEO John Reed — Reed's views on CASBS funding — Debate over moving CASBS out of Palo Alto — Reed's background in social and behavioral sciences — Stanford's sociology department — Reflections on Martin Lipset — Advising Drew Faust and others on the Radcliffe Institute

[Audio File 34]

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More on advising the Radcliffe Institute — Managing the staff at CASBS — Social duties as CASBS director — Sharin's contributions — Decision to resign as Director — Coming back to teach at Berkeley — Plans for future projects — Work on terrorism after 9/11 — Teaching in the postdoctoral program in the School of Public Health — Outside involvements while at CASBS: Guggenheim, American Sociological Association, the National Academy — Relationship with other centers for advanced study — Producing the *International Encyclopedia of Social and Behavioral Sciences*: Working with Elsevier press; conference on *Encyclopedia's* viability; working with Paul Baltes on organization of the encyclopedia — Consulting advisors and selecting section editors — Encyclopedia funding — Gender issues in the selection of contributors — Emphasizing quality over thematic coherence — Emphasis on theme in previous encyclopedias — Addition of philosophy to the Encyclopedia — Conflict with Jurgen Habermas

Interview #18 August 26, 2011

[Audio File 35]

603

Revising textbook *Sociology* — Translation into Russian — A pirated version in Mongolian — Gardner Lindzey's presence at the Center—Lindzey's relationship with the *Encyclopedia* — More on working with Paul Baltes on— Sociobiology — Reception of the *Encyclopedia* — Use of the *Encyclopedia* — Compensation and controversy with Elsevier — Editing the entries — Crises and conflict with some of the authors — Role of Nelson Polsby and Margaret (Meg) Conkey — Debate over admitting biographies of living figures to the *Encyclopedia* — Thoughts on Robert Merton and Pierre Bourdieu

[Audio File 36]

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Thoughts on Clifford Geertz — History in the American Sociological Association — Role as President of the ASA — Conflict in the ASA over the *American Sociological Review* — Appeasing the feminist caucus — Goals as president — Internationalization — Presidential address and the concept of "ambivalence" — The influence of Christine Williams — "The Politics of Ambivalence" — Rational choice as a foil — Ambivalence and economics of regret — Values and ambivalence — Sociology and community — A career focused more on the idea of conflict than stability — Reception of the presidential address — Erik Erikson and Kai Erikson — Producing *The Social Edges of Psychoanalysis* — Essay on Erik Erikson — Teaching with Robert Wallerstein — Relationship with Jim Clark of UC Press — Remembering Reinhard Bendix

Interview #19 August 29, 2011

[Audio File 37]

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Reflections on French sociology: Michel Crozier and Alan Touraine — Dinner with Touraine and Fernando Henrique Cardoso — On Raymond Boudon — The "Columbia School" of sociology — Recruited by Merton to attend Columbia graduate school — Influence of Max Weber, Emile Durkheim, Karl Marx — Return to Berkeley, 2001 — Work as a member of the National Research Council's Committee on Behavioral and Social Sciences and Education (CBASSE) — John Holdren — Changing CBASSE to the Division on Behavioral and Social Sciences and Education (DBASSE) — Significance of this change — Moving into the core of the Academy — Chair of DBASSE — The functions of the National Academy — Objectivity and politics in the Academy — Marijuana and global warming — Joining the Guggenheim Foundation as sociology reader, then as member of central Committee on Selection — Conflicts within the Committee on Selection — Working with historian Natalie Davis — Application process for the Guggenheim Fellowship — Role of foundations in academia — Working with author Jean Strauss

[Audio File 38]

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Work on the governing board of the Social Science Research Council (SSRC) — Craig Calhoun — Frederic Wakeman — The German-American Academic Council, Helmut Kohl, and Bill Clinton — Fritz Stern — Further thoughts on directing the Center — Ambivalence about administration — Adjusting to life back in Berkeley after leaving the Center — September 11th, 2001 — Trip to Seattle and Mount St. Helens — The National Research Council (NRC) panel on "Making the Nation Safer" — Tom Schelling and deterrence theory — Chair of NRC's committee on understanding terrorism from a behavioral and social science point of view — Funding from Defense Advanced Research Projects Agency (DARPA) to study deterrence of terrorism — Receptivity to the social aspects of terrorism in the National Academy — Jerry Bremer and opposition within the Academy to social explanations of terrorism — Lewis Branscombe's mediation of conflict between Smelser and Bremer — *Making the Nation Safer* — Meeting with Tony Tether, head of DARPA — Committee members Ira Lapidus and Gene Hammel — Thoughts on terrorism and reactions — Writing on terrorism for the National Academies Press — Lack of funding at the press — *The Faces of Terrorism: Social and Psychological Dimensions* picked up by Princeton University Press — Reception of *The Faces of Terrorism* — Thoughts on the present state of U.S. security — The Patriot Act and UC Berkeley

Interview # 20: September 14, 2011

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Response to *The Faces of Terrorism* — The human side of terrorism — Sociology's engagement or lack thereof with terrorism — Advising the FBI on the "Unabomber," Theodore Kaczynski — Giving backgrounds of former students to government agencies

— Homeland security and social science — Emergence of centers of social science devoted to security and terrorism issues — On personal anecdotes and autobiography in his own analytical works — Working with John Reed — Leisure reading and interest in the arts and fiction — *America Becoming: Racial Trends and Consequences* — On Bill Clinton's approach to racial issues — National Academy of Sciences conference on race — Relationships with William Julius (Bill) Wilson and Faith Mitchell — Previous work on race, riots, and lynching — Multiculturalism, affirmative action and "The Politics of Ambivalence" — Working with UC Berkeley administrator John Cummins

[Audio File 40]

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A shift in the goals of affirmative action: from occupational advancement to cultural respect — Postmodernism and humanism — UC's transition from Special Policy One (SP1) on Affirmative Action to SP2 — Richard Scheffler and the Robert Wood Johnson Foundation postdoctoral program — Teaching in the program — Teaching at West Virginia and Illinois State, to be near to his daughter — Family relations — The SARS epidemic and UCB's Committee on Surprises — Essay on the analysis of a surprise — Collecting the essays for *Reflections on The University of California* — "Post-retirement episodes:" Teaching abroad: Taranto, Italy in 2004; the Institute for Social Sciences in Vienna in 2006; returning to Taranto in 2009; heading to Bern in 2011

Interview # 21: September 29, 2011

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Early thought on *The Odyssey Experience* during the Education Abroad Program — Collaborating with Katherine Trow — Lack of time while at the Center for Advanced Study in the Behavioral Sciences — Impact of Victor Turner's work on pilgrimages — Interruption of the work on terrorism — James Billington and the Kluge Fellowship at the Library of Congress — Arrangements with Lynne Withey for publication at UC Press — Withey's emphasis on biography — The "self" that persists through odyssey experiences — Relationship to structuralism and post-structuralism debates — Emphasis upon the "typical" — The elements of the "human condition" — Mortality, immortality, and the denial of death — Relationship between notion of trauma and notion of odyssey — Reception of *The Odyssey Experience* — Emphasis upon methodology explicit in early career, implicit in later — Essay on Erik Erikson — Other essays from the 1990s — What led to *Reflections on the University of California* — Selection for the Kerr lectures — Historiography of the university — The "Pangloss Cassandra complex" and extreme reactions to change — Rise of non-tenured faculty — Outline of Smelser's Kerr lectures — Interest in drama and acting; father's history in the theater

[Audio File 42]

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Role of theater in family life — Involvement with Berkeley Drama Section and Town and Gown club — The satisfaction of performing — More on interest in fiction — Further Reflections on topics — History of the Berkeley's sociology department — Beginnings of the department — Divisions during the Free Speech Movement —

Receiving the title of University Professor — Department rancor subsides in the 1980s — Relationship with department as emeritus professor — Teaching at the School of Public Health in 2002 — Ongoing personal relationships — Departmental drift away from the more coherent "Berkeley School of Sociology" — Intellectual conflict with Michael Buroway — Potentially writing the history of the department — Working on *Usable Social Science* with John Reed — Background of relationship with Reed — Meeting with Reed and "all-star social scientists": Alan Kruger, Steve Stigler, Dick Scott, James Peacock, Rob Jervis, Susan Fiske — Working with Reed and the Russell Sage Foundation — Research for *Usable* — Hiring assistants — Feedback from Reed — Including Reed as a co-author — Finding a publisher and choosing UC Press — Reviews of the manuscript — Critiquing rational choice and managerial handbooks — Finding a middle ground for applications of social science — Time spent in Washington, DC, as Kluge Fellow — A former student, Mary Waters, replacing him on the Guggenheim committee — Reflecting on Harry Kreisler and the Conversations with History project

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Interview History

Lisa Rubens

The following twenty interviews were conducted in Neil Smelser's home in Berkeley, California, between January and August, 2011. Jess McIntosh, a doctoral candidate in U.S. intellectual history at UC Berkeley, joined me in conducting most of the interviews and took the lead questioning Dr. Smelser's work with Talcott Parsons as well as other seminal thinkers in the field of sociology. In certain ways this oral history was an intense as well as thoroughly enjoyable collaboration among the three of us. For all his stature—as an intellect, writer, professor, psychoanalyst, and administrative leader—Neil Smelser is extraordinarily accessible—open, humorous, down to earth and self-reflective. Each interview was informed by our disciplined research about the specifics and context of Smelser's life and then structured in discussion to frame a topic and time period. But the dialogic and unrehearsed, spontaneous character of the interviews is radiant. As Smelser explains in this "Afterword," this oral history affords an uncontrived and unmonitored account of his life and times.

Smelser is a very fluid thinker and raconteur. Elaborate sentences unwind, moving from the particularity of a subject to over-arching construct and analyses. His narrative follows an historical outline but occasionally returns to reflections on an earlier experience or projects his wrestling with an intellectual problem to a later resolution. His enthusiasm for life, including what he describes as his "interdisciplinary impulse," as well as his reckoning with hard times, is palpable. And animating all his work is an effort to understand and explain human behavior and experience to himself and his many publics. In this oral history we are privy to how his intellect and psyche evolve and mature. He revels in, and writes *suis generis* about, his personal odysseys. But in addition, these interviews in many ways constitute a history of the key people and critical developments in the social sciences since the mid-20th century.

Whether Smelser is traveling in the learned circles of the American Academy of Arts and Social Sciences or a departmental poker game, his personal, psychological, and sociological insights make for extraordinarily colorful and informative reading. A glance at the table of contents of this oral history shows an exceptional range of activity, experience, thought, and achievement: as a cub reporter at the Arizona Republic, as a revered professor at the University of California, as the convener of national commissions and the director of the Center for Advanced Study in the Behavioral Sciences, to name a few of Smelser's many endeavors. But the running outline that is the table of contents does not adequately reveal the richness and complexity of the discussion. Above all, Smelser's contribution to academic and public life is incomparable.

"Neil Smelser: Distinguished Sociologist, University Professor and Servant to the Public," is part of ROHO's series on University of California history. This interview was generously funded by a grant from the Carnegie Corporation of New York.

January 2013

Foreword

Jeffrey C. Alexander

Readers of Neil Smelser's oral history will encounter an extraordinary narrative of public citizenship and academic scholarship. His story spans the entire second half of the 20th century, and it is still going strong. It recounts intellectual and civic accomplishment of the highest order, displaying the best face of the American nation during an historical period when it strove, and sometimes failed, to set scientific and civic standards for the rest of the world.

What unfolds in these compelling pages of transcribed, informal interviews is a panorama of a strenuous intellectual life well lived. An exceptionally brilliant student, upon whom fortune smiled, became a man not only of great accomplishment but of good conscience and good works. He gave back to his profession and community even more than they had earlier given to him.

In the first place, Neil Smelser was a remarkably acute sociological theorist. Time and again, with razor sharp precision, he succeeded in dissecting historical, contemporary, and analytical problems that hitherto had been treated in murky and confusing ways. During his first year of graduate school, Smelser coauthored *Economy and Society* (1956), a major work of theoretical innovation with Talcott Parsons, the towering figure of mid-century sociology. Recounted here for the first time in detail, Smelser's analytical contribution to that joint project triggered a fundamental advance in functionalist theorizing, an idea about societal interchanges that continues to be influential to this day. In the Ph.D. thesis that soon followed, *Social Change in the Industrial Revolution* (1959), Smelser created a new approach to class conflict and historical change, anticipating future research on family and gender in a book that immediately became a contemporary, if controversial classic. Just three years later, Smelser's *Theory of Collective Behavior* (1962) appeared, a gigantically ambitious, systematic theory of social movements and cultural change that played a central role in defining the field for decades to come. One year after came his pioneering *Sociology of Economic Life* (1963), a subtle and precocious essay that adumbrated the future sub-discipline of economic sociology.

In less than a decade, and still two years short of his 35th birthday, Smelser had already published a life's work of radically new sociological theory. To be sure, in the years that followed there were many more works to come. Among them, *Comparative Methods in the Social Sciences* (1976) remains a uniquely theoretical investigation of methodology, exemplary for its rigorous, counter-intuitive logic; *The Social Edges of Psychoanalysis* (1999) explosively expanded the boundaries of sociology and psychology; *The Odyssey Experience* (2009) explained how institutions can be designed to release people from organizational constraints, allowing personal creativity to reorganize the parameters of cultural life.

Yet, even as he continued and deepened his personal intellectual journey, Smelser began to invest in – to give back to — the institutions that nurtured this exemplary academic career. One path led to ever deeper participation in the University of California, to this day the world's finest public institution of higher education. In Berkeley's times of troubles in the 1960s, Smelser served as a counselor to a series of university leaders, warning against repressive measures and

arguing for negotiation and mutual understanding. For a decade, he also helped lead the Berkeley faculty association. He chaired a major task force reconsidering Berkeley's school of education. In another time of trial, he chaired the Blue Ribbon Commission investigating the university's involvement in Intercollegiate Athletics. He represented the system-wide (nine campus) faculty for two years on the California Board of Regents. He became a Special Advisor to the University of California president, a floating trouble shooter engaged in long-term planning. Another pathway beyond academic writing led Smelser into the heart of the social scientific community, from being the youngest ever editor of the *American Sociological Review* to chairing boards for the Social Science Research Council, the Guggenheim Foundation, and the National Academy of Sciences, and to becoming the extremely successful Director for seven years of the Center for Advanced Studies in the Behavioral Sciences in Palo Alto.

These admirably wide-ranging extra-curricular activities remind us that citizenship is not just about participating in politics; it is also, and often even more importantly, about sustaining civil institutions. For a half century, Neil Smelser devoted himself to maintaining the most central institutions of academic life. At one point in the pages that follow Smelser recalls his seventh grade teacher pulling him aside to tell him, "you should become a diplomat." As he became an ever more powerful figure on the American (and international) academic scene, Smelser was indeed considered an honest broker, committed to deep principles both political and intellectual but even-handed, a person who not only saw but responded to the other's point of view. It was precisely this virtue, however, that allowed Smelser to move from diplomat to leader, becoming a dominant figure who dispensed great intellectual power in a sagacious, fair-minded way.

Some part of Neil Smelser's civic accomplishments were due to his cool head, his clarity of mind, and his immense, supra-disciplinary knowledge and sophistication. As a University of California leader once remarked, he was simply "non-intimidatable"! His achievements as an academic citizen were due also to his unusually synthetic turn of mind, a distrust of extremes that fed an ambition to find the golden mean. But if he was almost always disinterested, he was never uninterested. For Smelser, the politics of the academy was an ethical pursuit, the kind of morally driven and pragmatically responsible calling that Weber called "politics as a vocation."

In six decades of intellectual and civil prowess, Neil Smelser received virtually every honor, yet he eschewed bragging rights and seemed effortlessly to sustain a modest and approachable, if also high-minded, demeanor. In a time when academic stars flitted from one university and department to another, Smelser spent his entire academic career in one place. He recounts how it was that, during that time of troubles when he began to project his activities to audiences beyond department and discipline, he experienced for the first time, not simply appreciation for the University of California, but a deep love for it, an intense desire to serve and protect it from harm. Those who had the good fortune to work closely with Neil felt this love, but it was also experienced much more broadly. If Neil has been a true son of Berkeley, he has also been a profoundly significant member of society at large.

I first met Neil 43 years ago, in September, 1969, when I entered the sociology Ph.D. program at Berkeley and sat down with some 80 other graduate students to hear his weekly lectures in sociological theory, required of the department's first year students but attended by others from all over the social sciences. It was a difficult time. I had just come from SDS at Harvard and burned with all sorts of social indignation. I was taken aback by Neil's decidedly cool analytical

style, how he responded with brilliant detachment even as my Marxist friends and I peppered him with our critiques of the classical and modern sociological tradition. In later years, as my political fury ebbed and my disciplinary interests grew, I came to feel that a privileged opportunity to learn from a master had passed me by. So I took Neil's first year course all over again, this time as an advanced student, and determined never to let another opportunity to learn from him slip by. I asked him to co-chair my thesis committee; ten years later, we collaborated on three years of German-American theory conferences; ten years after that, during his directorship of the Center in Palo Alto, we organized two years of conferences and a third year of intensive disciplinary collaboration. We have co-edited two books and co-written one. My theoretical thinking about society has been deeply affected by Neil's. It has been an extraordinary privilege to learn from him these many years.

New Haven, Connecticut
August 2012

Interview #1 March 1, 2011

[Begin Audio File 1]

01-00:00:00

Rubens: Good morning, Neil.

01-00:00:01

Smelser: Good morning.

01-00:00:02

Rubens: It's the 1st of March 2011. Jess McIntosh and I are sitting in your home and we're beginning this odyssey, this journey of interviewing you about your life, and work. As a sociologist you've written that you strive to move from the biographical or the particular to the more objective and general. So we'll follow that strategy when we can. Tell us where you were born and where you were in the family construct and then we'll back up and do some family history.

01-00:00:41

Smelser: Yes. I was born in Kahoka, Missouri, a town of 2,000, in 1930, July 22nd, middle of the summer. My parents had moved to Phoenix one year before but my mother wanted to have me in her own home.

01-00:01:02

Rubens: In her childhood home?

01-00:01:03

Smelser: In her childhood home. In fact, it was on a farm and I was born in the same bed as my brother was born and in the same bed that my mother was born, which I think are highly unusual historical circumstances. There were several features of my birth that were always called to my attention when I was a boy and became a kind of focus of a certain feeling of specialness. One is that I weighed ten and a half pounds and my mother weighed ninety-five or a hundred at the time I was born. That was an unusual circumstance. I had flaming red hair and I had two teeth at birth, which is extremely rare and my parents always called attention to this. So that's it. I'm unusual. The teeth did not last, by the way. They were a source of inflammation and pain for me, I was told, and they had to be clipped out at age two weeks. So I went through the first six years of my life with soup dribbling, with no teeth in the bottom two spaces. Later teeth came in normally and that was that. But nonetheless, my baby pictures always showed those two teeth gone.

01-00:02:22

Rubens: And your mother liked talking about this? I guess you were her biggest baby.

01-00:02:26

Smelser: Yes. She said that I wasn't her most painful birth, though both my brothers weighed somewhat less than I.

01-00:02:32

Rubens: Where were you in the family structure?

01-00:02:34

Smelser:

I had an older brother, Bill, who was almost exactly six years older than I. He was born on July 14th, Bastille Day, and then I had a younger brother, Philip, who was three and a half years younger than I. My mother had a miscarriage between Bill and me, and this was a girl. This fed into childhood fantasies, you might imagine. Suppose I had been that girl. You know the way children elaborate on these things. It didn't prey on my mind but it was a very interesting kind of source of fantasy.

01-00:03:18

Rubens:

Well, it sounds like your mother talked to you about your birth, her pregnancies, and early years.

01-00:03:23

Smelser:

Some, some. Not in great detail. I knew about her family.

01-00:03:33

Rubens:

What was your mother's name?

01-00:03:34

Smelser:

My mother's name was Susie Marie Hess. Her parents' names were Hess, obviously, and Mohr, M-O-H-R, suggesting the German background. Her family had come to the United States, according to reports from her, in 1848 at a period of upheaval in Germany. They were in a Protestant minority in Bavaria and suffered some religious persecution and left and settled, along with basically neighbors of their own sort, in and around that little town of Kahoka. Kahoka was all German until World War I. Spoke German. Church services in German. A Lutheran/Protestant congregation. And the members were named Hammel and Hummel and Mohr—it was like a bit of Germany transformed into that community. They gave up German in World War I as a result of community and political pressure of not teaching German in schools and not having church services in German. So my grandparents did not speak German but my mother was exposed to a lot of German when she was young. She studied it again after her retirement.

01-00:04:53

Rubens:

They were farmers?

01-00:04:55

Smelser:

Yes. It was 120 acres. I daresay it was a historical part of the land granting in the United States. I thought that my grandparents lived very modestly on this farm. They grew wheat and corn and had livestock and gardening. I didn't think they were poor. I thought they were modest. But in retrospect, I think they were well off as farmers go. They had a car in those days, which was rare, I think. My father's parents were on a similar kind of farm in Missouri. Lived in a little town called Paris. It was my father's birthplace and he grew up there. I remember we drove there one time. He showed me his school and other places.

Kahoka was the focal point of my life because my paternal grandparents were older. My paternal grandfather died when I was six or seven years old. They lived in a little town in a less exciting sort of spot than a farm, for a kid anyway, so that my identification was more with the Kahoka [history] than with my father's place.

01-00:06:20

Rubens:

So your paternal grandfather was a farmer, even though he lived in town?

01-00:06:21

Smelser:

Well, yes. He lost his land and they moved into town. I can tell you about that a little bit, too. My mother and my father each had several siblings, but they were the only ones who went to college. They met at the same college in Kirksville, Missouri, not far from Kahoka, which is now Truman University in that little town. And I once established contact with one of the faculty members on that campus and I told him in that correspondence that my mother and father had fallen in love at Kirksville State Teacher's College. And he said, "Well, there's not too much else to do in Kirksville at the teacher's college." I thought that was very funny.

01-00:07:13

Rubens:

Just to embroider your family background a little bit more. On your father's side, could you trace when the first immigrants—?

01-00:07:20

Smelser:

Yes. On my father's side—the name Smelser is German. His father's descendents were almost all German. The mother was Irish, Scotch-Irish they say. Now, her name was Kendall before she was married. I know more about my grandfather's side. They were Pennsylvania Dutch. They came in 1820s as far best as I can determine and lived in Pennsylvania, then migrated to a farm in Missouri. My grandmother's origins were southern Kentucky, and I'm not certain about circumstances of their marriage and how they met. But most of the family background has been of German origin.

01-00:08:09

Rubens:

A bit unusual that your mother went to college.

01-00:08:16

Smelser:

Very.

01-00:08:18

Rubens:

Do you know how that came about?

01-00:08:19

Smelser:

No. No, I don't. I don't know where that inspiration came from. Really quite remarkable. She must have been obviously a bright young woman. She sang and she was very much interested in music and she somehow or other educated herself very well in the humanities. A lot of it was self-education. At age sixteen she was actually hired to be a teacher in this little country school outside of Kahoka. I once went to it with her. Where she taught all the grades

from kindergarten through eighth grade, perhaps. And she then went away to college. It was very interesting. She taught, later on, English and Latin and word study. Very literate. A very literate person. And I rather get the idea that she one time dreamed about a musical career but nothing ever came of it, though she was a talented pianist and she encouraged all of us in music. And that's the origin of my great—

01-00:09:29

Smelser:

Yes, Piano in the house. My brother took violin lessons. I first took piano lessons and then later violin myself but gave up when I went to college.

01-00:09:38

Rubens:

Well, then let's turn to your nuclear family, your immediate family. You said that your father had moved to Phoenix so he had secured his job in a community college before you were born.

01-00:09:51

Smelser:

Yes. He had gotten a master's degree in drama, dramatic arts at the University of Iowa just after they were married. He was unemployed—

01-00:10:03

Rubens:

I meant to ask when were they married?

01-00:10:06

Smelser:

Well, about 1922. My mother was nineteen and my father was twenty-four. He was five years older than she. He had gotten a master's degree at Iowa, but, however, was unemployed. This was in the Depression. He worked and he worked on a hand to mouth basis as a gas station attendant around a small town in northern Missouri and then by some fluke he got wind of this job in Phoenix and got it. So they went out there in 1929, the year before I was born. He stayed there his whole life and it was a great boon, I'm sure, because of the times.

01-00:10:50

Rubens:

It will begin to really grow during World War II.

01-00:10:56

Smelser:

Phoenix was a town of 30,000 when I went there. It was basically a village. During my youth we lived on East 14th Street, meaning fourteen blocks from Central Avenue and we were outside the city limits. Only one mile from the center but we were still outside the city limits. But it has, of course, ballooned. The big growth came after World War II and industry began to come in there. Irrigation was plentiful. Air conditioning became routine at that time and so a lot of retired people and so on moved out to Phoenix. So it's a very different place now than originally.

01-00:11:37

Rubens:

When did your mother start teaching? Was she at home during her children's early years?

01-00:11:43

Smelser:

She was at home during—basically up until the time I was, say, in seventh or eighth grade. She substituted. She was on call to do teaching but was never away that much as I recall. And then she started teaching at a private boy's school outside of Phoenix because she couldn't get a job in the public school system because of nepotism rules, because of my dad. The community college was in the district and you couldn't hire husband and wife, or kin, in the same system. So she taught in this private school for a while. Then the rules relaxed and she taught in public high school in Phoenix. That was about the time I went away to college that she moved into the public school system.

01-00:12:35

Rubens:

Let's talk about the Smelser household. Three young children, well spaced well enough. Educated parents.

01-00:12:49

Smelser:

Well, I would have to describe my childhood as being—I'd use the word normal and the fact that my parents stayed together and were basically harmonious. It was not a household filled with conflict. My mother, in kind of a traditional way, tended to be very quiet. She was a quiet and long suffering person and quite clearly had this traditional feature of my father being the authority.

01-00:13:19

Rubens:

Were they affectionate with each other?

01-00:13:20

Smelser:

Not exceptionally but I felt they were happy together. Yes. They called each other endearing names all the time and it was quite clear that they were made for each other. Except on very rare occasions of an argument, I never sensed that there was much negative tension between them. So I saw my parents, in my child's eye view, as a quite serene household. And they cared for the kids. I think they liked the boys a lot and they made every effort to treat us all as equal. That was part of the family culture. It's always the boys, right. And, of course, that created problems for each of us, I suppose, in that we wanted to be special. But their idea about raising the children was that they're all equal and they're all positive about it. They gave us a lot of positive feedback.

01-00:14:22

Rubens:

How did you boys get along when they were young? I know that Bill becomes very—

01-00:14:26

Smelser:

I got along with Bill, you might say, absolutely beautifully. He was six years older than I, so he was a bit of an uncle. The uglier sorts of sibling rivalry were not there. They were subdued though I later came to recognize that they were there. But I don't think that I ever had a fight with Bill. He was just my guide. I was very faithful and loyal to him. I have described my relationship as being his lieutenant. He was the captain and I was his lieutenant. He was

always extremely solicitous and encouraging of me in that when I was in first grade he was in seventh grade and we went to the same school. Every lunch he would come eat with me.

He was sort of a saint really. And he encouraged me, especially in sporting activities and in learning things. Even when he was in the army and came home, he was studying advanced math and I was in high school at the time, we would talk math. He was just very helpful and encouraging. I'll give you one incident. We were on a local little touch football team at Garfield School where we both went to school. It was a WPA team, government sponsored, and I was the center. It was a six man team and I was the center and six years younger. He was the quarterback and leader of the team. And the center is usually sort of a non-entity in six man football. You just center the ball and there you are. But he made a point of signaling me out, giving me a chance to catch a pass from time to time, making sure that I was in the action and doing important things. I even knew it at the time.

01-00:16:19

Rubens: It's unusual with that big a gap in years that he would have that kind of nurturing—

01-00:16:23

Smelser: Well, yes. It's very interesting. He was an extremely well-behaved and very good boy and I continuously felt I wasn't of the same cloth as he. I wasn't good enough. Again, in comparison with him, that was kind of a haunting sense, that I was not—

01-00:16:42

Rubens: In all these dimensions in terms of—

01-00:16:44

Smelser: Mostly kindness. I never thought he was smarter than I but I always thought he was kinder than I. In that sense, I saw his link to my mother, because she was a kindly person, as well. So I saw them as the same and I saw myself as a little bit more troubled and possibly not as well behaved.

01-00:17:07

Rubens: Did you feel more threatened by your younger brother?

01-00:17:08

Smelser: Yes. My younger brother Philip was born when I was three and a half years old. I was made famous in the family by reporting at one time when he—he came home from the hospital when he was just an infant. My mother had a temporary nurse to help out with him. I was told that I said—that I accused the nurse of wanting to steal him, which was a sort of thinly disguised projection. I was always teased about this. Philip and I have always had a good relationship. When he was little there was a lot of conflict. I'm quite sure I resented him. And also, I made an effort, because of my identification with Bill, to become his captain and have him be my lieutenant. Well, he didn't

want to go along with that, so we didn't have that recapitulation of the older and younger brother. We were closer and there was a lot—and Bill and I often ganged up on him, teasing mostly. There wasn't any physical conflict but there was a lot of verbal conflict and I think we were not terribly nice to Philip on some occasions. But now I have an excellent and beautiful relationship with Philip and have had ever since we were—80 percent of our lives we have had a very good and affectionate relationship. And we visit them and they come up here.

01-00:18:46
Rubens:

So your mother didn't seem to particularly have extra affinity, love for the baby? She—

01-00:18:53
Smelser:

I probably suspected that but I couldn't find any evidence of it. I didn't associate it with being personally rejected. I just think I didn't like this little character who'd come into my life.

01-00:19:06
Rubens:

Right. So tell us just a little bit about how the family structure worked. Did you have help in the house? Did your mother manage the household?

01-00:19:17
Smelser:

It was a fairly traditional division of responsibility. My mother did the cooking and she did the housecleaning. We weren't well enough off. We were modest income. Steady but modest income from my father's salary, so we didn't have any family help to speak of, except when she substituted we would bring in a person to take care of us after school, that kind of thing. But notably it was a pretty self-sufficient household without other people in it. It was a family core and that was pretty much it.

One interesting thing is they enrolled Bill and me, and later Philip, into doing family chores at a very early age. I remember Bill was the dishwasher and I was the dish dryer. We each got—we got paid for it—two and a half cents per meal. So four meals, we each earned a dime and then that was in lieu of any allowance. We never had an unearned allowance. It was always doing something. I got an allowance for drying dishes, allowance for mowing the lawn, for diligent practice of the piano. It went into my allowance. But it was always a very small allowance. We had this little institution, Philip and I particularly, that we called nickel day. One day a week we were given a nickel to spend as we wished. We'd always go and buy a bottle of RC Cola or something like that.

01-00:20:57
Rubens:

I was going to ask what you were saving the—

01-00:20:59
Smelser:

Candy. Candy at the local stores.

My parents encouraged us academically but not in a frantic way. There was a very important transition when I was in seventh grade. But my early years in school, I was a superior student. Got very good grades and I was—

01-00:21:29

Rubens: Did you like going to school?

01-00:21:31

Smelser: Yes, yes. I felt school was normal. I liked it. I liked my teachers and I had a good, good grade record. But I wasn't especially motivated. I didn't have this drive particularly. I just sort of did it and I—

01-00:21:47

Rubens: It came easily.

01-00:21:50

Smelser: It came easily. My learning was easy. I felt I was on top of the material. I wasn't bored but I wasn't—I didn't pick myself out as being special, as a smart kid. My parents were very happy with my grades. I remember, for example, in say fourth or fifth grade, I received a C in geography. A three they called it in that school. My father took me aside. He said, "You can do better." He didn't punish me. They were obviously on top of it and they were proud of a good academic performance. It was very much part of the family culture, as you would expect with two teachers in it. They were tied to the school and they liked the fact that their kids were identifying with the school.

We all ended up teaching. My older brother, who was a clinical psychologist, taught at the University of California in the social welfare school for most of his career on a part-time basis. Not tenured. An instructor. He was a practicing clinical psychologist. And Philip is a teacher. He went in the footsteps of my father. Taught up until last year philosophy in a community college in Glendale, Arizona. So it was a very academic family and—

01-00:23:22

Rubens: Were there books in the house and journals?

01-00:23:24

Smelser: All the time. Every time I got bored I found some book being handed to me as a way of accommodating the boredom. They weren't compulsive about it. In other words, I didn't ever feel it was a high pressure atmosphere to perform academically. But they definitely had that in mind and they looked at my report cards. And when I was in high school and I was in speech contests and other activities, my parents took a keen interest, especially if I would do well in it. They were always involved. And I'd say strongly supportive rather than punitive in supporting my own schooling.

01-00:24:16

Rubens: I think I read that several of your summer vacations were passed in different towns where your parents had actually gone to summer school.

01-00:24:26

Smelser:

Well, yes. Every other summer, my parents would—we'd get in the car and we would drive back to Missouri.

01-00:24:33

Rubens:

Do you remember what kind of car?

01-00:24:36

Smelser:

The one I remember best, there was an old Lafayette, 1937 Lafayette. There was a 1934 Chevrolet. The maximum speed they ever drove was thirty-five miles an hour. And this was a long trip. I think it took us five days to get back to Missouri. We would go to Kahoka and Vandalia, where my grandparents on my father's side lived at the beginning and at the end.

Then in the middle of the summer, say for six weeks, my parents would go to a university. They went to Columbia, University of Missouri, University of Iowa, Minnesota, Baton Rouge when I was tiny. I don't remember that. But we would go there and live and they would take summer school courses themselves. That's how my father shifted from teaching drama into teaching philosophy. He just went and reeducated himself. It helped their salaries that they'd do summer work. The school had a reward system for people who did continuing education, as we now call it, so they were helping their own careers but at the same time studying different things. And so I remember those trips with the greatest sentimentality, both visiting my grandparents and being on that farm, particularly, and living in these different towns all the time. My brothers and I would always form a little gang among ourselves, right, assuming we weren't going to have time to make friends with neighbors and that sort of thing. So we became a little tribe and sort of made our way in these new areas. We'd go to the railroad tracks. We'd climb around the cars and generally sort of get into mischief. But I always felt very—kind of in a little tiny solidarity unit with my brothers in these trips.

01-00:26:36

Rubens:

So even with the younger one?

01-00:26:40

Smelser:

Oh yes. At that time my younger brother was fully involved. Those were not competitive times. We didn't fight. I think my older brother was a kind of policeman.

01-00:26:53

Rubens:

So I'm wondering if we're ready to talk about when your brother went off to the Army?

01-00:27:02

Smelser:

It was a critical point in my life. It was when I was in seventh grade. I went to Garfield School, which was kindergarten through sixth. I went to Emerson School, which was also the subsequent one, when I was in first grade because my parents lived one year in that district and then moved to Garfield.

And so I was in Garfield School for about five years. Then I was scheduled to go to Whittier School, which was in my district, seventh and eighth grade. And eighth grade was the end of the primary school and then you had four years of high school. That was the system. I was so identified with Bill that I more or less set up a personal demand that I go to Emerson even though I wasn't in the district and it required a special effort on my parents' part to get me into Emerson. It wasn't that much further away but it was not in my district. But we prevailed, so I went to Emerson and sort of relived those two years with the same teachers as Bill had had. He volunteered for the service in 1942 when I was twelve years old and just in that seventh, eighth grade level.

01-00:28:20

Rubens: Do you remember Pearl Harbor?

01-00:28:23

Smelser: Oh, yes. Yes. I remember it because I was in a movie with Bill and when we came out everybody was milling around and excited and we didn't know what had happened. No, I remember that very vividly, that particular aspect of it. And I remember Roosevelt's speech which they broadcast in the schoolroom when I was in—I believe I was in sixth grade when that happened. So yes, I remember that very vividly. I followed the news in the war pretty—even as a kid pretty closely.

01-00:28:51

Rubens: Well, I interrupted you. I didn't mean to. I want to come back to that. So he volunteered for the army?

01-00:28:59

Smelser: He volunteered to go in the service. He went in for basic training. Came up to Fort Ord here down near Monterey for his basic training. Nearly died. He got meningitis. By some absolute blessed event sulfa drugs had just been released and were being used. He came up here, was hospitalized in Cowell Hospital on the Berkeley campus when he was in the Army and the sulfa drugs saved his life. That, of course, was an extremely meaningful moment in my life to know how close to death he came.

But I went to Emerson School and I suddenly, and this has to do with his departure, and I was now the oldest boy in the family. I suddenly underwent a transformation in which I became—I just remembered I just became absolutely ambitious and engaged in my work and the sort of sense or the feeling that I was going to move ahead in life. That's exactly when it happened. And I now have reinterpreted that many times in different settings as being a manifestation of my own competition. It was at the same moment I said to my mother, "I don't want to be a piano student. I want to be a violin student." And so I switched at that moment when Bill went away.

01-00:30:25

Rubens: Bill had played the violin.

01-00:30:27

Smelser:

Bill was the violin. It was his instrument and mine was the piano. I said, “No, I want to play the violin.” So she says, “Fine.” I switched and played violin all during high school, all during those years and through high school. And my academic performance shot. It was always good but this became, I suppose, a small obsession on my part to get all ones and my teachers—I was singled out. I was already in eighth grade. They had something equivalent to a valedictorian at the end of the primary school and I was the speaker at the graduation even in eighth grade. And my teachers, on a couple or three occasions, would call me aside and compliment me and encourage me and so on. So it began to take on a new and more driven quality about it. One incident you should know about was my seventh grade teacher called me outside the classroom one day after the lunch period and she said, “You ought to be a diplomat when you grow up.” I was totally stunned by this, her diagnosis that I should be a diplomat, and I don’t know what part of my behavior she was reacting to because I certainly didn’t see myself as a diplomat. I just saw myself as a kid and so I have been forever curious why she thought that was the case.

01-00:31:55

Rubens:

I do want to ask you about your self-esteem then. One of the things that you end up writing about and feel very strongly about later in your career is the idea of self-esteem.

01-00:32:09

Smelser:

Well, my self-esteem certainly took a boost at that time. And I’d have to say that in general I’ve never really had a depressive or self-blaming streak that’s very strong. Where my self-esteem was a little flawed was always in relation to Bill because I always thought of him as a better person and I always also saw him as, in some respects, probably the favorite of my mother because he was so much like her. That he had this monopoly, if you will, or inside track for my mother’s affection. Then this all changed when he left—

01-00:32:54

Rubens:

There’s room for you to—

01-00:32:57

Smelser:

In a way I said to myself, not then but in retrospect, kind of said, “Well, I’ll occupy this little throne now myself.” But my big claim to fame at that time was academic performance. I think Bill had established himself as the good boy and he had that cornered pretty well, but I chose academic excellence as the way to assert my specialness in the family setting. And, of course, what better could you do in a family like that? I got rewarded plenty for choosing that route.

01-00:33:32

Rubens:

A couple more questions just about the household at that period. You mentioned that the family moved.

01-00:33:38

Smelser: Yes.

01-00:33:39

Rubens: Did this represent a sort of upward mobility in social standing or location of where they lived?

01-00:33:45

Smelser: Well, my earliest memories go to a house on East Moreland Street. We called it the \$25 house because that was the rent. They were not owners there. There was one other house I never remember. But I was there from age three to five and I have some vivid memories of that house. Then they moved for one year up not far from Emerson School. That was when I was in first grade. Then they bought a house in 1937. They had enough to buy a house. They built a house in the neighborhood where I lived from seventh grade and almost through high school we lived in that house where they built. It was on the edge of town and across from us was a completely vacant lot. That only got built up subsequently. So we were a little bit in the country almost, not really but on the edge of town at that time. That took me up through the middle of junior year in high school.

Then my father, in a sentimental moment—mostly my father, it was, decided he would like to have a house with some land and some livestock. It was his farm thing coming back. Very sentimental. So we bought a house out in the west side of town, which had a couple of acres and it had a cow, chickens, turkeys and so on and some place to grow a little something. And I remember very vividly, I was at a park playing ping-pong with a high school friend, and my dad ferreted me out. I don't know how he found out that I was there. But he came and got me. They were moving that day and he said, "You've got to come home. You've got to come help us. Get in here." From that day forward he designated me as taking care of the cow. I learned how to milk. I was the official slaughterer of chickens. We sold chickens sometimes to neighbors and to friends and I was designated to do that. And so the last year and a half before I went to college we lived on this little farm.

01-00:35:55

Rubens: And the family was able to manage it? Did you have to have help?

01-00:35:58

Smelser: Nope, nope. We did the whole thing. It was a challenge. I was really very, very busy because it was a lot further away from high school where I went.

01-00:36:06

Rubens: How did you get to school?

01-00:36:07

Smelser: Bicycle. Our last house was about three miles from the high school, so I would bicycle in every morning.

01-00:36:18

Rubens: So Bill wasn't a part of that?

01-00:36:22

Smelser: He was away. Bill was away from the time I was in seventh grade. He came back to visit a lot, of course, in summers and so on. But the years from twelve to age eighteen, I was the oldest kid in the house. I have to say, just as an additional part of it, Philip was not a—Philip was a sick child. He had stunted growth in his right leg and he had to miss a whole year in school by staying in bed with a weight on his leg to pull it out, to treat it. He always had a limp. In seventh grade, he missed a lot of—most of the year. He graduated by special examination. He had rheumatic fever and he had to live at home, be at home in bed for much of that year. So this was a little bit of a blight on my life to have Philip being sick a lot and I'm sure I had very ambivalent attitudes toward it. But that was a feature that was an important part of those years for me.

01-00:37:32

Rubens: What about religion?

01-00:37:35

Smelser: My parents grew up in a small community. My mother was still somewhat religious. My father I don't think was brought up very strongly in religion and he actually became quite antagonistic to organized religion. The religion of that community was Lutheran, German Evangelical, actually, we'll call it. And we would always go to church when we'd go to Missouri because my aunt was the organist. My mother's sister was the organist in the church. And my mother put us into Sunday school in a church in Phoenix called Grace Lutheran, which was the closest she could find to what her German Evangelical counterpart was. I attended Sunday school and I would have to say that in the first grade, through first grade, my experience of it was very indifferent. I was bored. It wasn't an engaging experience for me at all. Then the church began to work on my mother to do volunteer work and get in there and involve herself and so on. Too many demands on her time, so she dropped out. And that was pretty much the end of it as far as my religious involvement was concerned. I've never been a religious person. I occasionally went to church with high school girlfriends, always with some muffled resentment that I wasn't doing what I—it was a foreign experience and I was doing it for them and not because I had any interest. So religion was not a very big part of my life. I've given you the whole thing.

My parents were not literally religious and my mother got influenced very much by my father's anti-religious attitudes. I would have to say that they were, from the standpoint of character, extremely faithful to representations of the Protestant mentality in terms of hard work, discipline, no excesses in life. When I hear Garrison Keillor talking about the Lutheran mentality I really think it's rich because my parents sort of had that kind of—and they really believed in hard work and that aspect of the religion. If you go with Max

Weber, they were pure Protestants. My parents always tied my allowance to doing something. And then my father in particular was very interested that I learn the value of money and the value of work. So when we went back to Columbia in Missouri I must have been seven or eight years old. We would go out in the country and we would pick blackberries and gooseberries and he'd have me go sell them. Had to carry them around in a little wagon to sell to neighbors.

01-00:41:05

Rubens: Bill, too, during—

01-00:41:06

Smelser: Bill was in on it. The whole family went out and picked them and then Bill and I sold them. I don't remember how involved he was. I certainly remember my involvement. Then at the beginning of World War II, just before Bill went into the service, my dad and he worked part-time, the way many people did in the summers and so on, at a cantaloupe packing shed to earn, supplement their income. There were labor shortages, so jobs everywhere. And they had a practice at this cantaloupe packing shed of giving the overripe cantaloupes, called culls, to the employees because they couldn't ship them. They were too ripe to be ship but they were, of course, the best cantaloupe in the lot but they couldn't ship them. So the culls they gave to my dad. He'd bring them home. Then he'd send Philip and me around the neighborhood with these cantaloupes, selling them for a nickel a piece, six for a quarter, and there was my work, right. And I kept all the money that I earned in this work.

And in seventh grade, by some means or others, I got a job. I broke the law because I was too young. I wasn't fourteen yet and the state labor laws wouldn't let kids under fourteen work. But I lied and worked four days a week after school in seventh grade, 4:00 to 7:00. And then later I worked on Friday afternoon from 4:00 to 7:00 and twelve hours on Saturday, breaking another law that you couldn't work a kid more than a certain number of hours a day. But it was all kind of winked at.

01-00:42:49

Rubens: Now, where were you working?

01-00:42:50

Smelser: I got a job in a grocery store packing groceries.

01-00:42:55

Rubens: Now, had Bill had that job before?

01-00:42:46

Smelser: Yes, he had. Not in the same market but in the same—well, the same system. Bayless Markets. And I had one up in a different part of town. But I packed groceries and then after some months the manager, I don't know whether he spotted some kind of talent or just had an opening or what, he put me in charge of stocking vegetables. So I became an expert in recognizing

vegetables and how to trim them and how to fix them and judge their quality and that sort of thing. So I worked for several years—I worked all through high school in that capacity. Usually Saturdays, all day Saturdays. As a supplement to that, when I was in eighth grade, the local milkman said he wanted to hire me to help him deliver on Sunday morning because he wanted it a little easier, right, when he was making the Sunday-morning run. So I worked for a dollar for several hours of delivering milk on that day. And every cent that I earned was mine. So I built up, I guess for a kid of that age, substantial—I saved almost all of it.

01-00:44:05

Rubens: Did you have plans for that money?

01-00:44:07

Smelser: I saved almost all of it. War bonds, a bank account, whatever. I certainly was not a spendthrift boy. I basically didn't have anything I wanted to buy.

01-00:44:21

Rubens: You weren't buying ham radios or model airplanes?

01-00:44:23

Smelser: No. No, no. I really didn't spend the money. I must have saved 90 percent of it. I'm not sure what led me to that decision but I had a nest of money all through college. Didn't even touch it in college. I only used it when I went abroad and really needed it in my studies abroad.

Now the last part of this work history. In high school I was very involved in journalism. I became the sports editor of the high school newspaper as a junior, and a general editor when I was a senior in high school and then I also was the editor of the yearbook. So I was involved in journalism and I got a job at the *Arizona Times* as a proofreader, when I was in senior year in high school. And then the *Arizona Times* went out of business in 1948. That was about the time I went to college. I came back and got a job in the *Arizona Republic*, first as a proofreader to earn money to supplement my fellowship at college and then I was put on—I became a reporter. The editors thought I'd be okay as a reporter—and I filled in for everybody who was on vacation. So I was on the federal beat, I was on the police beat and I was on feature stories and this, that and the other thing for the last couple of years in college, which is a—I'll come to it later. It was a very important era in my life and I think I learned as much from that as almost anything. Has a lot to do with my writing.

01-00:46:10

Rubens: I was going to ask you if you remember writing stories in high school. When you say associate editor, were you putting together—

01-00:46:17

Smelser: Oh, yes. I had a column, a sports column and then a general column, which I made a lot of—did a lot of political commentary. Every time the *Coyote*—it was called the *Coyote Journal*—came out I had an editor's column that I

wrote there. I didn't do much reporting; that was mainly in the commercial paper.

01-00:46:36

Rubens:

Talk a little bit more about your social life in high school and what the relationship between being involved with the newspaper and the yearbook - did it give you a certain status or—

01-00:46:49

Smelser:

Well, as I say, I graduated with big honors even in grade school. And then when I went to high school, my behavior didn't change at all. I said it would make sense for me to learn how to type. It would be a good thing for me to learn how to type. So at the end of my grade school I went to a summer school at Phoenix Union High School and took typing. It met from 6:00 in the morning until 9:00 in the morning because of the hot weather. Summer. And I would bicycle there every morning, 6:00 to 9:00, learn how to type. Became a famous fast typist at age thirteen. And, of course, that has served me ever since. And the class was all girls. All secretaries in preparation whose parents thought this was going to be a skill you need. I was the only boy in the class, as I remember, and would bicycle home at nine o'clock and have the rest of the day to myself. So in high school I was your quintessential straight A student. There just wasn't a subject in which I didn't just knock it out. And this was all that ongoing ambition still unfolding. I didn't have any idea what I was going to do afterward but I was just motivated to do well, and did, and, of course, the teachers took notice of it.

01-00:48:22

Rubens:

Were there teachers that were particularly outstanding to you?

01-00:48:25

Smelser:

Oh, yes, yes.

01-00:48:26

Rubens:

Oh, I was going to ask you also, the social makeup of that school.

01-00:48:29

Smelser:

Oh, the social makeup of the school. It was one of two high schools in Phoenix at the time. It was called Phoenix Union High School and it was in the center of town. There was a north Phoenix High School that was out not far from the country club, so it got the wealthier students in the community. But we still lived in the district of Phoenix Union High School. It was a rainbow except for blacks because Arizona education, Phoenix education, didn't desegregate until 1952, two years before the Supreme Court decision. But I was not in high school at that time. There was a segregated high school, George Washington Carver High School. But we had a heavy Mexican population even then in Phoenix. I think it was probably fifteen or twenty percent of the students were Mexicans. And I had a lot of acquaintances and friends in high school there.

But let me go back a little bit and tell you a little bit about my youth. My social life and my youth and then I'll get into the high school, if I may. This happened to be mostly Bill and me because Philip was a little bit too young during much of this period. In 1937, when I was seven years old, we moved to that house on East Moreland Street.

01-00:49:55

Rubens: That your father built.

01-00:49:57

Smelser: That we built. That neighborhood was populated, from the standpoint of youth, mostly of boys. It just happened as an accident to be boys. And these boys would get together to play and to fool around and sometimes to commit some mischief. And we took special interest in new houses that were being built because we'd go and invade them after the workers left and climb around in the attics. Occasionally would do a little desecration. A kind of gang involved in this. Not in the sense of urban gangs that we know these days. This was just a group of boys of which Bill was the leader. He was older than any of them and he was the unequivocal leader and I was his lieutenant. But it was mostly a social gang.

01-00:50:50

Rubens: Did you do sports or—?

01-00:50:52

Smelser: We played sports and games. But then we dug a cave in a vacant lot, a very elaborate cave that would hold ten or twelve people and had a couple of tunnels and a separate exit and so on. We all did it with knives and forks. But we would gather there at night and we would build a fire and we would roast potatoes and we'd sit around and talk and do things. We didn't engage in any war or anything else with other gangs. It wasn't that kind of gang. We just played together. Mostly sports I'd say would be it. And I have very happy memories of that. Kind of solid group. We moved there when I was in second grade and we lived there. So it would cover the years really up to high school for me. And I kind of outgrew it and left it when I went away to Emerson School. But there was an intensive period of several years in which this gang played a very big role in my life. And we were left free to roam. My parents were very permissive. They kind of liked the idea. They said, "You have to come home at a certain time," and we'd occasionally get called to come home. My parents knew where I was. It was just across the street from where we lived. But they were very permissive. It was one of these kinds of times in the history of the country where people didn't lock their houses and their supervision of—they sort of saw Bill as being my supervisor and so they didn't worry about us.

And we didn't get into deep trouble. We would occasionally get caught kind of throwing clods and things around houses under construction. The contractor once caught me doing it and he reported me to my father. We'd

been throwing the clods, dirt, into the house and messing up the plaster, the plastered walls. And my dad just took me right over there and made me clean every bit of it off. That was about the depth of which we got—It was children, boys' mischief, and the police never involved. So we were, you might say, a true gang but a somewhat innocent gang.

01-00:53:28

Rubens:

Were these kids of the same social class and ethnicity?

01-00:53:33

Smelser:

This may be more significant than I'm indicating but a lot of the—several of the kids, maybe a third of them, fathers were policeman. I think they liked living it up. I assume they had more authoritarian households than I did. But those kids clearly liked the somewhat devil may care attitude that this gang displayed.

01-00:53:58

Rubens:

And primarily these were kids you also knew at school?

01-00:54:02

Smelser:

Yes. There was a couple who were just my age. Others were a little bit more scattered. No girls. No girls. I had no interest in girls.

01-00:54:15

Rubens:

Let's switch tapes and take a break.

[Begin Audio File 2]

02-00:00:04

Rubens:

I think you wanted to talk a little bit about—

02-00:00:12

Smelser:

The working. Yes. I talked a great deal about how my parents, especially my father, had a very rewarding posture toward my earning money and working and learning the value of labor. But when I was working for the grocery store in eighth grade and working four afternoons a week from 4:00 to 7:00, my employer apparently had expanded opportunities and asked me to come in Friday from 4:00 to 7:00 and then work twelve hours a day on Saturday. I went home and told my folks about this. My dad just took me aside and he said, "You can't do that." I don't know whether he was interested that I would interfere with my school life or I'd just get exhausted or what. But he said—

02-00:00:57

Rubens:

There were your chores, as well, at the family farm.

02-00:01:01

Smelser:

Well, this was a little before that time. Though I continued to work fifteen hours a week even while on that farm.

He simply pulled me aside, said, “No, you can’t do that.” He said, “That’s too much. You can’t be every day until 7:00 and then twelve hours a day on Saturday and then go out delivering milk on Sunday. You’re hitting the ball too hard.” I remember his words absolutely precisely. And he forbade my taking that extra time. He liked the idea I was working and earning money but he didn’t want me to overdo it. In a way, I have that discussion with him very fondly in my mind, that he’d sort of examined the situation, said, “Well, just—” In a way it went against his values because he liked the idea I was working but at the same time, don’t overdo it.

02-00:01:56

Rubens:

He had a sense of limits and appreciated that you should have this full life of friends, school, and work.

02-00:02:04

Smelser:

He was a drama teacher and he directed plays and he would have me come out and help him build the sets all during the young years of my life, from age seven to fourteen, and I would go to all these plays. I got a great love of drama because he directed them. Most of them were over my head. He chose plays that were not for kids. That was a great period of bonding with my father, to go out and work with him on these sets. I’ve carried a love of drama for my whole life.

02-00:02:41

Rubens:

Plus you learned some skills. Were you good with a hammer and—

02-00:02:44

Smelser:

Oh, yes. He taught me how to be sort of a carpenter. I had carpenter skills. And, in fact, between my senior year in college and going off to England I worked as a carpenter’s apprentice in Washington, DC for the summer just to earn money. The foreman wanted me to become a carpenter, I was good enough, and I told him, “No, I want to go to school.” This is before going to Oxford. He sort of said, “Why do you want to go there for? I’ve offered you this apprenticeship.” [laughter] It was his values. So anyway, I wanted that as a footnote to my work history.

02-00:03:26

Rubens:

Also, this is during the whole years of the Depression. In ’37, when your father built the house, there was another big downturn in the economy.

02-00:03:34

Smelser:

It was a low period in the economy. He had a guaranteed income. I remember his salary was \$3,500 a year. But it was a steady income. We lived modestly. But for some reason they were able to finance the construction of this house. It wasn’t a fancy house. I’ve gone back to see it. Every time I go to Phoenix, Philip and I go around the neighborhoods where we lived and they’re all intact. It wasn’t a huge house. All three boys shared one bedroom. And so that’s to give you an idea that it wasn’t big. My father had a study and they had a bedroom and then there was the usual living room, dining room. We all

had the one—we called it the back bedroom in the house. So it was, again, modest but nonetheless it was for the times comfortable.

02-00:04:24

Rubens:

And do you kind of measure that there were indices of having a little bit more comfort in the house?

02-00:04:34

Smelser:

Not truly. The house was a fairly disciplined household when it came to spending money. It was good. We could afford these trips to the Midwest and they were steady. Was there anything that appeared that to me looked like it was anything like luxury? No. They bought modest cars for the family. I didn't feel I was in any kind of affluent setting. I didn't feel wanting but I didn't—and my mother always would economize on food. I definitely remember that. I was aware there were tough times as I became more socially conscious but I wouldn't say I had a wanting childhood in any respect.

02-00:05:25

Rubens:

I meant to have you elaborate some on what you wrote about in high school.

02-00:05:42

Smelser:

Well, in my sports column I would venture opinions about local sports in high school and then about the sports scene, mostly in the western United States. I once wrote an opinion article after Illinois had whipped USC in a Rose Bowl game or something like that. I wrote the fact that Middle West football is really superior to West Coast football. And one of my physical education teachers was a Stanford graduate. He really reamed me out for [laughter] saying that. Irresponsible. And my general column was more political. I commented on the state of the nation. I wrote articles about Truman and his policies. And the faculty member who oversaw the paper, he was a little sensitive that I was getting into the touchy areas by writing about politics—but I didn't feel inhibited. He let me do it. So I guess it was like an editorial column more than anything else.

02-00:06:36

Rubens:

Did you engage in sports yourself?

02-00:06:37

Smelser:

Yes. As a kid in school I played all the sports. Basketball, football. In grade school I was always involved in the school sports. In high school I was asked by my coach to go out for the track team as a quarter miler and I was very good in gymnastics. In fact, I think I am still the state record holder for the rope climb. Twenty foot. Fast Speeds. And so I enjoyed the gymnastics a lot. He asked me to go out for the track team and I worked out for a while but then came on a school play that I was strongly encouraged to do. So I had to weigh it out. Do I want to go into the spring in drama or do I want to work out for the track team? So I chose the drama route with great encouragement from my father, of course.

02-00:07:39

Rubens:

I can imagine. Do you remember the plays?

02-00:07:42

Smelser:

I acted as one of the characters in *Lost Horizon* by James Hilton and then a cheap detective play, one that I can't remember the title of. I had minor interest. We'll get into the extracurricular activities in high school in a moment. Friends. I started dating girls when I was in seventh grade and mostly the usual pattern of parties and going out. I went out to some movies and things of that sort and dances. So then in high school I more or less dated pretty much continuously through high school. Never exclusively except toward the end. I was steady with a girl for about a year and a half at the end of high school but that died a natural death when I went away to college. I always had a network of friends. I say I wasn't a—

02-00:08:36

Rubens:

Ladies man or—?

02-00:08:37

Smelser:

Well, I never felt as though I was either unattractive or obnoxious. No wall flower. I was always involved in the social action, especially dances and parties and that sort of thing. So I guess you can call it a kind of average involvement. I did not have a clique of friends. I kind of gravitated toward different groups, of which I was kind of a partial member. Some sports minded boys, for example. I hung around a little bit with a strange clique of mostly girls who thought they were very special from a social class point of view, though I never felt I was—in fact, I even felt alienated from that even though I had friends and most of them were pretty smart. I had another network of friends of the really smart students, maybe a half a dozen of us, who were obviously the competitors for academic performance. We'd show up on prizes and things of that sort. I was in speech contests, I was in journalism, I was in drama and these all netted me little networks of friends. A few special friends. A guy I double dated with almost two years in high school and some other special friends that I felt special continuity with. So I was pretty social. Not excessively social. I often considered myself, from the standpoint of introspection, to lean more toward being a loner than a groupie. Even though I did have a lot of social involvements, my own picture was I was an autonomous kid who was making his way through and never subordinated to or a conformist with high school norms.

02-00:10:36

Rubens:

You had that sense of yourself then?

02-00:10:38

Smelser:

Yes. I was kind of tired of the high school faddism. Impatient with it. Not tired of it but impatient with it. I thought what are these kids doing? So in that sense I felt myself kind of self—

02-00:10:52

Rubens:

Do you mean that vis-à-vis music that they liked or having more—

02-00:10:55

Smelser:

Music, clothes, use of language, slang that the kids used. They always have their own language and so on. I never got into all that stuff. I just considered it kind of silly. Not that I was superior or anything but I just didn't have the taste for it. And so I felt myself more than anything else a kind of self-directed kid that was involved socially but not dependent on it very much. Because I was working part-time all during—I was working and a lot of extracurricular activities and involvement and so I—

02-00:11:31

Rubens:

What about reading, you said your parents always gave you books to read when you were bored. Do you remember reading novels or—

02-00:11:35

Smelser:

Yes. Yes, I did. My parents got me into reading novels and plays early, even seventh, eighth grade and through high school. And I was kind of precocious in this regard. I read really advanced novels before I went away to college. *War and Peace*, Dostoyevsky. They were always available and I kind of knew about these things. My father and I talked philosophy a lot. He was teaching philosophy at the time. When I went away to Harvard I discovered myself very well prepared in subjects, unlikely subjects, largely because of my relations with my father and my brother Bill. And all these things all—

02-00:12:19

Rubens:

The classic philosophers? The western—

02-00:12:22

Smelser:

Yes. Spinoza. I was a great fan of Spinoza. My father was a great devotee of Dewey so he had me talk about and read Dewey. He introduced me to advanced drama. Naturally you'd expect him to be interested in Ibsen and Shaw. He got me interested in that, even in high school. So you could kind of see the bookishness. It was novels and drama that I was exposed to.

02-00:12:50

Rubens:

What about movies? What kind of movies do you remember?

02-00:12:55

Smelser:

Oh, the whole array. I was a complete devotee of westerns when I was young; my brothers and I would always go on Saturday afternoons to see these. My family would go to the movies sometimes. That was a way of celebration. Going to the movies was a rare occasion and going out to eat in a restaurant was a rare occasion. But we just followed the usual run of movies. I remember a lot from my childhood. We occasionally rent them now and see them. Not an addict by any means, but went fairly regularly.

02-00:13:31

Rubens: Do you think we did enough on your social life? I have to just review my notes a little.

02-00:13:39

Smelser: I'm satisfied that I kind of got to the essence of it. I'm not sure at the moment if I know what to add. I guess maybe I should just add one thing. You had asked me about my teachers. I had a special relationship with many teachers in high school, both because they spotted me as a superior student and because, one way or another, I always felt—Bill had gone through the same sequence. They all knew me as Bill's brother. And so that was already an introduction. And some of them knew my parents as teachers in the community. So I always felt kind of a—probably a more than usual identification with and closeness to teachers, just because they were—and I had some of them who were really, really good.

02-00:14:28

Rubens: Do you remember the subjects?

02-00:14:29

Smelser: English mostly. English and writing and speech. I took all this verbal stuff. I went through advanced math and took physics and chemistry and everything else but I was really much more drawn to the writing and expressive subjects. And, of course, that's—

02-00:14:47

Rubens: And then you actually wrote by working on the newspaper.

02-00:14:50

Smelser: I wrote a lot, working in a newspaper and that was—I guess maybe I should comment on that as being a real advantage to an academic. I sort of feel that every academic should have had some kind of experience of this sort where you have to write directly short sentences, know what's important, get it there and elaborate it later if necessary. But there was a real kind of art to that. And you had to do it quickly. So I was often assigned stories on which you had to just go get it and write it and do it. I never recognized this at the time but my father had thought of becoming a journalist in his own youth and I sort of thought that about myself, too. However, in high school I got to know all these—I was always younger than all the other reporters because they were full-time. They were career reporters. And I seem to remember picking up this definite sense—I even articulated it to myself. That this is a culture of cynicism and I'm not—

02-00:15:55

Rubens: That the journalists represented?

02-00:15:57

Smelser: Journalists. It's a culture of cynicism. It's just not in my spirit to be so destructive. Must have been an accident. They're not all that way. But it was my perception of my own newspaper at the time. And so one of my mentors

and friends at the newspaper encouraged me a lot to stay in journalism. He said, "When you go to Harvard you've got to go out for the *Harvard Crimson*. You're just a natural. You've got to do it." That was, of course, a route for a lot of journalists, to write for a famous college newspaper. But the reason I basically didn't do it was because it was so time consuming. These kids were spending all of their time at the *Crimson* and they were neglecting their academics. That was why I didn't do it.

02-00:16:46

Rubens:

Okay. Well, we'll be talking about Harvard soon enough. But now I'd like to turn a little bit to your sense of what the politics of your family were. And I don't mean the social dynamics. I mean literally you've mentioned a couple of times about your father having very strong opinions.

02-00:17:00

Smelser:

Yes. He was a man who was very passionate about his attitudes toward politics, toward the rights and wrongs of this world and sensitive to social class. Not that he was socially mobile but he had these longstanding resentments against business people, lawyers, doctors.

02-00:17:25

Rubens:

His father had lost his farm.

02-00:17:27

Smelser:

I was going to say that. He was an agrarian radical out in the Midwest. Standard radical. Out of the mold. He was anti-eastern, anti-money, anti-capitalist. What happened is my grandfather had owned this farm and was paying mortgages on it. Just near the end of the payoff time, when he could have been an outright owner, he missed a couple of payments. They took the land and he was kicked off of it, forfeiting all of his value. My father really never forgot that. He spoke with bitterness and passion about the whole thing. This tailored his attitude toward the capitalist system. Of course, the best thing that ever happened to him in his whole life was the election of Franklin D. Roosevelt. He was an ardent New Dealer and Roosevelt walked on water. He hated Republicans. I got barraged with all of this all during my childhood and much of it stayed with me. There was a touch of bigotry in his hatreds. Not the standard racial bigotry that we think of because he was always a civil rights person and encouraged liberal attitudes on all fronts. But I never inherited this bitterness about doctors and lawyers. He thought they were all criminals.

02-00:18:58

Rubens:

All of them.

02-00:18:58

Smelser:

Businessmen. They just were worthless and they were bleeding the country dry and they were enemies. He had this habit of writing humorous but hostile letters toward companies about their advertising policies, about how they lied. He'd just shoot out these letters to the world that were consistent with this anti-commercial and anti-corporate sensibility. So I grew up absorbing that.

And my mother was pretty much quietly in agreement with all these. She wasn't active politically. She didn't speak up very much but she—

02-00:19:39

Rubens: I assume she voted.

02-00:19:40

Smelser: Oh, yeah. They always voted but she didn't challenge my father at all. If she happened to believe differently from him I would never have known it because there were never any political arguments in my house. My father pretty much was a kind of political dictator. Dominated the politics. What I didn't like was this bitterness and his hostility. It made me nervous about my father. I sort of had a feeling a little bit there was a bit of a volcano inside him. This was one of my points of—a frail point in my relationship with my father. I didn't challenge my father very much but I sort of vowed that I was going to be more moderate in my opinions through life than he was. But I never fought him.

But that wasn't the whole story about my dad. He was affectionate, caring. He told humorous stories all the time, jokes and anecdotes about his childhood. He had a tremendous sense of humor. His hero was Mark Twain, as is mine. And that's all through my parents and their—of course, that's Mark Twain country where I was born and where my parents lived and they loved him. I have the complete works of Mark Twain on my shelf in there. That's the one thing I wanted when my mother died and my brothers said, "Okay, fine." I don't want to over portray him as an ogre, a monster, even though he had this particular political passion that he inflicted pretty much on the family. But at the same time there was this humane side. He played. He played sports with us and he took us places especially and he was encouraging. So he was a complex man but he had this core of bitterness, political and religious and social class bitterness that I felt myself—I distanced myself from that side of his personality.

02-00:21:54

Rubens: What about Fireside Chats? Do you have images of your family listening to the radio?

02-00:21:59

Smelser: Yes. I knew about Roosevelt, of course, all the time because my father was always talking about him. But I guess my independently developing political consciousness didn't begin until about World War II, when I was eleven or twelve years old. And I followed the war news very, very—

02-00:22:23

Rubens: Yes, you mentioned that earlier. Was your brother sent overseas?

02-00:22:28

Smelser: Yes. He was picked out because he was academically strong and scored high on the army's tests and so on. He was not put in a combat battalion but put in

for training, a meteorologist. And he went to Oregon, he went to Fort Ord, he went to Harvard. He was a big influence on my choosing Harvard because Harvard engaged in a lot of education of troops in World War II and he was there living on the Harvard campus for four or five or six months. And he went overseas. He was stationed in Iran as a weatherman with the service and then he worked for a year for TWA as a weatherman before he came back and finished up his college and graduate school.

02-00:23:20

Rubens:

Was he writing letters back home?

02-00:23:32

Smelser:

Well, I kept close relations with my brother. I was extremely proud of him. When he came back on furlough I took him back to Emerson School with me. So he and I would go visit his teachers and my teachers. So I kept in touch with him and he would write all the time to the family and sometimes to me and he'd bring back gifts and things when he would come home. So I was still very, very attached to him.

02-00:24:01

Rubens:

And would you have tracked the war even if he had not been there?

02-00:24:05

Smelser:

Oh, no. It didn't have to do with him that I tracked the war. I got very much interested in the European war and the Russian, the Finnish War. Even before World War II, I was a great fan of the Finns and was following the news accounts. After Pearl Harbor I was interested in everything and would read the paper avidly. So I got politically rather conscious but it was tied specifically to the war. Domestic politics didn't play very big a role in my life, though I was very conscious of rationing and the things that the war brought.

02-00:24:40

Rubens:

And do you remember the dropping of the bomb on—

02-00:24:46

Smelser:

Vividly. I was at work. In the spirit of what the public mood was at the time, I did not suffer a sense of disgust at the dropping of the bomb. I fully shared the fact that Japan was our enemy. I didn't cheer but I just read it with great interest. I didn't have any cosmic feelings about was it going to change the world or feelings against Truman. As a matter of fact, a couple of years later in high school, when a person working in the cashier's office —when I was in there paying a bill or something— spoke out viciously against Truman for dropping the bomb I said, "What?" Because there was so much consensus that this was the thing to have done at the time that I didn't get beyond it.

02-00:25:35

Rubens:

Right. Because this was supposed to end the war and thus kill less people if it had dragged on.

02-00:25:37

Smelser:

Yes. The war's over. I remember hearing about the end of the war when I was at work, so I went downtown and watched everybody dancing in the streets and so on. I very vividly remember the end of the war. And it was just after the bomb exploded that the war ended.

02-00:25:59

Rubens:

There was what a race war or a fight in Phoenix. Apparently one of the black regiments that was stationed there—

02-00:26:10

Smelser:

Oh, yes. Fort Huachuca down south of Phoenix. We actually went to visit that when we were on a recent trip to Arizona. That was where the blacks were trained. That was quite a ways from Phoenix. That was down near Bisbee. But I knew about it. And we had prisoner of war camps around Phoenix and I knew about those, though I never saw any of them. But a lot of the German prisoners of war were put in detainment near Phoenix.

02-00:26:40

Rubens:

And also in Arizona were two of the Japanese internment camps.

02-00:26:45

Smelser:

Yes, out in Poston, which was south of Kingman and then another one. I remember in high school—

02-00:26:52

Rubens:

The Gila River.

02-00:26:54

Smelser:

I remember in high school one day just like *that* every Japanese kid disappeared and nobody seemed to notice. It was really a strange thing. And I didn't say, "Where are they? What's going on?" I had no political consciousness of all this. They just weren't there. I was friends with a couple of them and they just didn't show up. They were obviously sent away to these camps. I always reminisced. One of the great defenses that Germans used after World War II is, "We didn't know what was going on." It was a standard defense. And here I was in this little situation. I didn't know what was going on. I didn't have any clues. Where were they going? What were they doing? Where were they put? Were they killed? You had no sense of anything.

02-00:27:44

Rubens:

So you had mentioned earlier, and I wanted to come back to it. You said your father had liberal attitudes.

02-00:27:48

Smelser:

Yes.

02-00:27:50

Rubens:

And supported civil rights. Do you remember him commenting on these events also?

02-00:27:55

Smelser:

Oh, yes, yes. As a matter of fact, when I was a freshman in high school I entered a public speaking contest, a school wide public speaking contest and there were eight finalists in it. And my speech, much inspired by my father, was about racial discrimination. I remember being very comfortable in speaking about that, but at the same time I also sort of had the feeling, barely articulated feeling, that I was maybe too much influenced by my father in this; that I had not developed this on my own. He didn't write it for me but he obviously articulated the attitudes that I built into this speech. Interestingly, the judges of this speech were absolutely totally split down the middle. I got two first place votes and three last place votes, obviously reflecting the political attitudes. One of the judges actually came up to me afterwards and scolded me, one of the right wing ones, for talking on this subject.

02-00:29:04

Rubens:

And were you talking about integration?

02-00:29:07

Smelser:

It started off with an incident where I had personally observed a black being kicked off a bus because he wouldn't sit where the conductor told him to sit.

02-00:29:17

Rubens:

Really? In Phoenix?

02-00:29:18

Smelser:

In Phoenix. Oh, yes. It was a racist town. It had a long southern influence, even at that time. And that was the beginning of my speech. It said, "Why was this done?" And so then I built on a few general ideas. And I actually still think it was a pretty good speech. But it split the judges and this guy just came up and said, "You ought not to be talking like that." I remember I didn't get intimidated by that but it made a mark on me.

02-00:29:52

Rubens:

And what about the UN. Did you follow the—just because you become so involved in international area studies and have—

02-00:30:02

Smelser:

Well, you see, all of the UN's work was being done when I was a senior in high school. I did not get glued to the process by any means. However, my father was a member of a local chapter of a group called the United World Federalists, which was a group that believed in world government. A liberal group. It was very active at the end of the war and into my early college years. My dad very much influenced me. As a matter of fact, in my freshman year in college, I joined a Harvard chapter of this, but it kind of faded away in my life. It was one of those organizations that I think I continued on out of deference to my father but it didn't resonate exactly. I thought it was kind of an unrealistic point of view that was being propagated and I didn't quite understand how this could be realized and so I drifted away from that point of view early in my college years.

02-00:31:16

Rubens: So the family had never gone to the West Coast or the East Coast. I was thinking about the formation of the UN in San Francisco.

02-00:31:23

Smelser: Never. My dad, another negative attitude he had was toward California. California was full of these thieves who are taking Arizona's water, its power and wealth. That was not an uncommon attitude in Phoenix. We sort of felt like a third world or in a colony of the southern California affluence and so on. So we didn't go to California very much. Not at all. I think the first time I was in California was when I was in high school and I was on a debate team and that was kind of it.

02-00:31:59

Rubens: Well, that was a big deal. So you had a traveling debate team?

02-00:32:04

Smelser: Yes. I was also on the debate team. That was another one of my extracurricular activities. In the junior year we debated a lot locally. But we took one trip to California, debated at the Compton High School, in Southern California, and Bakersfield, and then spent a long weekend in Los Angeles, which was a very memorable moment on my part. I went to the opera for the first time in my life. It was the first time I was away from home and a high school teacher went along as a chaperone for us. But no, California didn't figure much in my life when I was young. It oriented more in the Midwest.

02-00:32:52

Rubens: So I think we've almost gotten up to where you're thinking about where you're going to college and before we move on—is there anything you want to ask right now, Jess?

02-00:33:08

Jess: Well, sure. I've just noticed that as the conversation has proceeded we've gotten to a point where we're beginning to talk about you, for instance, going to Los Angeles or shooting out of Phoenix for brief periods.

02-00:33:21

Smelser: Yes.

02-00:33:22

Jess: Or even during your summer travels. And I'm curious if your perspective on your upbringing and Phoenix itself and being part of this third world colony, as you described it, if that perspective changes as you begin to kind of get a broader sense of the outside world, both the United States through your travels and the world in general through your readings and through your father.

02-00:33:51

Smelser: Oh, yes. Despite my father's prejudices, I had a link with the cosmopolitan world, mostly through reading and through their consciousness. My mother was a fan of Roman history and she talked a lot about that. She taught English

literature. She was a great fan of Shakespeare. I got exposed to Shakespeare through her more than my father. My father was forever quoting Shakespeare at the dinner table. Usually humorous parts of Shakespeare. And so there was an opening there. And then I had this early skepticism about my father's prejudices. So that did not inhibit me from thinking more complicated thoughts about the country, about the different regions of the country. I gave up my Phoenix oriented world pretty much immediately when I went to college, because I was so far away and in such a different culture. I had a California attitude, as well.

02-00:34:57

Rubens:

Of course, your brother had settled out here.

02-00:34:58

Smelser:

Especially when Bill came here. I wasn't in deep prejudice but this is what I inherited from my father but I began distancing myself from what I subsequently—what I described as his parochialism— pretty easily. Without much conflict with him, by the way. I was a kind of quiet rebel in that regard.

02-00:35:18

Jess:

I don't want to get too ahead of ourselves because we're going to talk about your arrival to Harvard next time, but when I read your biographical writings, it's interesting because you're talking about actually clinging to this parochial identity at Harvard.

02-00:35:38

Smelser:

One aspect of it only, and that was I did not like that precious cultural attitudes that I got at Harvard, that I witnessed at Harvard. It was the most special place in the world and nobody else counted. Elitism was really the—

02-00:35:57

Rubens:

The fact that you'd entered the elite. That's—

02-00:35:59

Smelser:

The elitism was what really nagged at me. And nope, I was never treated better in my life than I was at Harvard. Teachers, friends and so on. There was nothing wrong. I even got invited to come into the clubs when I was at—one of the waiting clubs they call them, just before you got into the final clubs. And when I got invited by my roommates to join the club, I said no. It just wasn't in my blood. It's like later on I was asked to join the Bohemian Club. I said, "No, it doesn't fit me." So this aspect of my father's attitude, his egalitarianism, his anti-elitism, really hung on to me and in some respects it diminished my capacity to enjoy all of Harvard culture, and later all of Oxford culture because they're both elitist institutions and I just kept myself distant and I think that was a kind of—much as I broke from my father in many regards, I did not break with him in regard to this egalitarian strain that I still feel very strongly about.

02-00:37:02

Jess: We're going to come back to this next time.

02-00:37:03

Smelser: I would like to talk about my decision to go to Harvard.

02-00:37:11

Rubens: I just have a few more questions about your youth and childhood and family values. I wondered just one more thing about your mother. You said that the church had enlisted her for awhile.

02-00:37:20

Smelser: Yes.

02-00:37:21

Rubens: But did she participate in PTA or any women's clubs?

02-00:37:28

Smelser: The two most vivid memberships I remember from my parents are that there was a club in Phoenix called the Missouri Club. It was people who had grown up from Missouri and migrated to Phoenix and they'd formed a little organization. A lot of towns and cities and states do that, particularly this growing community in which practically nobody was born in it. They all came from outside. They had potluck dinners and so it was a strictly social club and my parents, still feeling their own identity with their home state, their own origins. So they went there quite a bit and we went when I was—they stopped going when I became a little older. But mostly they belonged, during my grade school years, to the Missouri Club.

There was also a book club. Once again, a kind of part of their precocious cultural presence in a town that was basically very provincial and growing. This was made up mostly of teachers in the community college where my dad taught and they both—

02-00:38:28

Rubens: But your mom participated?

02-00:38:29

Smelser: It was a couples club and they all read the same book and they all—and they would host this and she was a famous member of it because of a special type of muffins or buns that she cooked that we inherited. We make those from time to time. They're totally delicious. I'm not sure where she got the recipe. But everyone wanted to meet at our house all the time because she would make these buns. They were active members in that. They read political books. They read academic books and novels. And that was continuous during my whole childhood, that book club meeting.

They were not very active in the neighborhood politics. My father was a leader in the American Federation of Teachers group there and he was kind of—insofar as he was locally known, he was kind of regarded as kind of a

radical in the Phoenix politics. Some barber broadcasted around that this man's a communist. And I remember when I was subsequently cleared, had a top level clearance thing—we'll come to that in my later work with the regents and so on. I was interviewed by the FBI and they spent a portion of that interview, maybe up to a quarter or a third on my father's politics. "What did he do?" Of course I answered what do I know about my father's politics.

02-00:40:04

Rubens:

But you knew he was a union man and that he believed in unions.

02-00:40:06

Smelser:

His favorite union was -he's in AFT, and he was outspoken. He would jawbone a lot with neighbors and friends about his politics. He wasn't shy. I don't know how the FBI would track down this information, most of which was misinformation as far as I could determine. But nonetheless, if you want a kind of a sense of where he was politically, that was another symptom.

02-00:40:37

Rubens:

But I don't get this feeling, or is this true, that he was drawn to socialism or to communism.

02-00:40:47

Smelser:

No, no. I think he had an appreciation of the work of Karl Marx. I do. I've written about it and so on—

02-00:41:02

Rubens:

You taught his works.

02-00:41:03

Smelser:

Yes. And then taught it and did not have a rejective attitude toward Marxism. Mixed, always a mixed attitude. But these—

02-00:41:12

Rubens:

I just wondered if you were aware of it or—

02-00:41:14

Smelser:

He would not have wanted to vote for Norman Thomas. He didn't have those socialist leanings. He was just an ardent liberal Midwestern agrarian radical Democrat. So in that sense he was in the system but had very strong feelings.

02-00:41:30

Rubens:

Of course, you always knew you were going to go to college. What about Bill. In '42 he joins the Army. Had he already been—

02-00:41:41

Smelser:

Yes. He was just entering Phoenix Community College where my father taught. He was only there for I think a few months and then he went into the service.

02-00:41:56

Rubens: Did the fact that the war had broken out, did that shape why he went to Phoenix? Do you think there were any other possibilities?

02-00:42:03

Smelser: I'm not sure why. I've never been aware why he went to Phoenix College rather than somewhere else. There's localism.

02-00:42:12

Rubens: May have had to do with finances, as well? Or what—

02-00:42:15

Smelser: Maybe. That's a blank. I draw a blank on why he went to college where he did at the beginning. By the time I went to Harvard he had come to Berkeley. This was after the war, '48. He was at Berkeley already finishing his undergraduate and doing graduate work in philosophy and psychology at Berkeley. So that also was a link. That's one of my main motives for coming to Berkeley. Really was that Bill was here later on.

02-00:42:43

Rubens: You mentioned that he was stationed at Harvard.

02-00:42:47

Smelser: He took math courses and things that you teach meteorologists and he lived in Dunster House and he wrote me these letters about the Charles River. He described it very vividly. It wasn't a typical undergraduate experience. It was this wartime instruction that was part of his military training. But it was kind of memorable for him and he spoke of it kind of fondly and he gave me an idea. You mentioned I knew I was already going to go to college. I guess that's right but it was not uppermost in my mind. I didn't write my future trajectory very vividly during my high school years. I don't remember plotting that I was going to apply to here and to here and so on. And when it came to it, I applied to the University of Arizona in Tucson and Harvard.

02-00:43:45

Rubens: Those were the only two?

02-00:43:46

Smelser: Only places. I had a last minute application to Yale just as a kind of afterthought. And that was it. And I didn't have a magical aura about Harvard. It was all about Bill. I knew it was a world famous university and it would have been great to go there and so on but it wasn't in my culture very much. It wasn't in my life perspective.

02-00:44:12

Rubens: Your parents weren't suggesting certain places? They may have wanted you nearby, since Bill was gone.

02-00:44:15

Smelser:

No, I didn't have this sense that my parents were driving me in any way. They liked the idea I was going to go to college. They assumed I was. I was number one in my high school class. It was just all natural that—

02-00:44:29

Rubens:

Were there any counselors that made suggestions?

02-00:44:34

Smelser:

Yes. Two high school teachers. Two high school teachers took me under their wing and said, "You really have to try to get into someplace good." I'm not sure that I listened to them that carefully. But there was some assistance from my high school counselors. It was not decisive but it was there. But mainly Bill. And then when I sent in my papers, Harvard had just introduced a program called the national scholarships. It was their way of diversifying at the time, meaning they were going to get more people outside of New England and they were going to get more people from public schools than they had. It was midway in that transition. When I went to Harvard, it was half public schools, half private schools and they had already come a long way from the private domination. And it was half Massachusetts and half non-Massachusetts. But this national scholarship was fifty scholars from around the country that they got special scholarship funds for. It was national. It wasn't deliberately public but they got smart public school kids to come, boys. Somehow or other the admissions committee got notice of my application and commissioned a local guy, one of their local alums, to come and interview me. He was interested in my possibility of getting a national scholarship, which totally paid your way to Harvard. So I got interviewed by him. I'm still looking around and still thinking. I wasn't frantic and I just wasn't quite certain. But I didn't exactly have a systematic approach to here are the colleges I want. Not very much calculation. I just picked these places.

And it was a very funny incident when I got admitted. This was someday in spring or something like that. And the letter came admitting me to Harvard when I wasn't home. My parents couldn't stand the curiosity, so they opened the letter, and it said, "You're admitted to Harvard. You got a national scholarship." And so what they did—it was a curious, curious event. They hid the letter from me when I came home. Instead they gave me a little note. It said, "Please look under this." Go to the note. It said, "Please look under that." Another note, "Please look. Please find. Look in your shoes. Look all around." So they had sent me on this crazy wild goose chase around the house. The last was the letter from Harvard. I thought it was kind of a joke. It was kind of slightly bittersweet. There was a little punitiveness, a little frustration that they imposed on me about this as well. And, of course, it was a happy ending to it but they'd given me a little needle, a little—

02-00:47:31

Rubens:

You think that's what it was?

02-00:47:32

Smelser:

That's what I sort of felt like. Deep down my father was so proud of me for getting into Harvard. My mother, too. They just were delighted that I had scored this great success, which was decisive in my life. But my dad still had this eastern thing. He didn't want to lose me to that culture. He was pretty circumspect in revealing any opposition he might have had or any shortcomings or any reservations he might have about my going to Harvard because he mainly was joined in the congratulatory mood, which is—he felt that, too. But he had this idea that I might get seduced or spoiled into this odious eastern culture. And he didn't have that objection to Oxford. That was just in a foreign country, even though it was as elitist an institution as Harvard. He didn't like the idea that I was maybe going to get spoiled or ruined a little bit. Soured. Lose out on these convictions that he felt so strongly about. We never fought about it.

02-00:48:56

Rubens:

And there was no question, then, of course. You had a fellowship, off you were going to go. The only other choice was Arizona. So there was no—

02-00:49:03

Smelser:

Yale admitted me at the last moment. This was afterthought. But by that time I had made up my mind and had got the fellowship and so on. My parents had to spend \$35 a month during my entire college career. That was it. Can you think of such an advantage for a kid from the sticks to get that kind of opportunity?

02-00:49:28

Rubens:

No. And your brother had not made suggestions to you about where you would go?

02-00:49:32

Smelser:

Only because he'd been there and showed this enthusiasm. He gave me no advice. I don't know why I didn't apply to the University of California. It just didn't cross my mind that I would do that, because he was there at the time. My younger brother, Philip, came to Cal. He's got an undergraduate degree from Cal. But no. That wasn't a realistic thought in my mind. When I look back on it, it was kind of a very weird transition because of how non-calculating I was and how I didn't really think it through. My ambitions hadn't crystallized in that form. I knew it would be an advantage but I didn't—

02-00:50:14

Rubens:

What about your friends? I was kind of interested in the college bound rate of your high school and—

02-00:50:22

Smelser:

My high school, half these kids—they had a dropout rate of at least a third. It was, as I say, completely heterogeneous and it had a low rate of people going away to college. There was one guy from North Phoenix High School and me

who went to Harvard that year. As far as I know, the only two of us. We rode the train back together. He was traumatized by it and didn't stay.

02-00:50:55

Rubens: Traumatized in the sense of—

02-00:50:56

Smelser: Oh, he's just away from home. He was sort of a frail character. He was a mathematician. Boy genius mathematician and he didn't fit in socially at all and got depressed. He was sleeping all the time. He didn't perform very well. And he dropped out.

02-00:51:12

Rubens: What about some of your close friends? Where did they go to college?

02-00:51:16

Smelser: Arizona mostly. Arizona and Arizona State and some to a local community college. It was a locally oriented high school. I think I capitalized on its academic culture through its good teachers more than anything else. The atmosphere in the school was not intellectual. It was not. And I think I inhibited my shining academic record sort of unconsciously for that reason. I didn't go around all the time with the smart students and I didn't, I believe did not carry an attitude of superiority to most of my classmates. It just wasn't part of the picture. I was sort of a friendly young man.

I dated a heterogeneous group of girls, including a couple of Hispanic girls. Most of my dating was not deep or serious but there were a couple of Mexican girls. We called them Mexican at the time. And didn't hang around with especially intellectual girls or wealthy. It was a mix I'd say. I don't know. Maybe I had fifteen or so what you might call girlfriends, meaning anything from a couple or three dates to more extended. But that was sort of the pattern.

02-00:53:00

Rubens: So it wasn't one kind that you were attracted to?

02-00:53:02

Smelser: No. No, no. It was a mix.

02-00:53:04

Rubens: At one point you said one of your homes was out near the country club. I forget what you said about what the relationship was to the country club. But I wondered if you had any associations with some of the elite of Phoenix, if there were some people who did have money or came from the—

02-00:53:28

Smelser: Most of the family friends were teachers. I don't think it went much beyond that. It was in that subcommunity of kind of cultured types but certainly not rich types. The grocery store that I worked in was right across the street from the country club. And my customers were—a lot of them were very, very wealthy. I was a very social young kid there. When I was packaging the

groceries and carrying them to their car I engaged in a lot of talk with the customers. I kind of opened up socially in a way that I wasn't in high school and would talk and joke and I actually developed some charm in that relationship with the customers. This, of course, helped me out by getting tips but I wasn't calculating.

02-00:54:18

Rubens:

Just what I was going to ask you.

02-00:54:21

Smelser:

I wasn't calculating about the tips. One thing that happened in that workplace that you have to know. It says a lot about me at the time. This was in World War II and we had shortages. A lot of foods were rationed. Meat and sugar and canned goods and so on. And a lot of others were short but not rationed. Toilet paper, paper products in general and cigarettes. Cigarettes were being manufactured for the troops and there was a shortage of supply and they were very competed for by customers. And I was designated by the manager of the store to be the cigarette boy. What we did was—we would wrap up these cigarettes, two packages, in paper—so they wouldn't be recognized as cigarettes. They were just in a little package. And we put them in the back of the store and I was designated to recognize the customers that were good and to supply them with two packs of cigarettes. The cashiers knew those cigarettes from the way they were wrapped and would just charge them but no one ever saw the cigarettes. And sometimes the cashiers would signal me to go get cigarettes. And the thing that was, to my point, in retrospect maybe the crowning point of this: A few customers offered to bribe me if I would give them more cigarettes. Fifty cents or something like that. And without thinking at all, I simply declined, that I wouldn't do it. Just seemed wrong. I guess it's family values, whatever. And never told the manager that they tried to do it. I never told anybody that they tried to do it. I just didn't comply. I went to give them their two packages of cigarettes. And what interested me, that I can't quite ever figure out, is why the manager would have chosen me. He must have seen something. Trustworthy kid who didn't cause him trouble. And he's already picked me out for this—

02-00:56:41

Rubens:

But worked hard, got along with his good customers, as well.

02-00:56:43

Smelser:

Yes, I got along with the customers. The customers liked me. I just lived it up with them. And, actually, I'm quite sentimental about those years in the grocery store. Something new came out in me.

02-00:56:57

Rubens:

Interesting. Was it some kind of association with the upper crust?

02-00:57:04

Smelser:

Of course it never occurred to me at that time. I knew that it was a rich neighborhood but I didn't know the incomes of the people who came and that

dimension wasn't there and I certainly didn't feel that these people deserved special respect or anything like that. It was a kind of an egalitarian view toward everything. But what changed in me was my sociability and my humor. There was just a lot of humor I had. And I would even tease customers. This diplomat image kind of actually developed. This was after the teacher told me I should be a diplomat. But it was that friendly, cooperative, joking and a little bit teasing relationship that I developed with a lot of customers. To my mind, it was a part of my growth, I suppose, but I didn't feel it in the same way in my high school setting or neighborhood setting.

02-00:57:55

Rubens: Or it didn't come out.

02-00:57:56

Smelser: There's something special about that, working in that store.

02-00:57:59

Rubens: Did you smoke?

02-00:58:03

Smelser: Never.

02-00:58:03

Rubens: Or your parents?

02-00:58:03

Smelser: Caster beans. When I was a little kid, we had caster beans growing in—a caster bean is kind of like a little tree that has hollow stems. And you can cut off the stems and you can light one end of it and it can be like a cigarette. I don't think it's toxic and I found it—Kids tried it. We'd go sit outside in a vacant lot and we'd try it. I didn't smoke very much of it. I didn't like it, actually. There's another reason I didn't smoke, is my dad smoked. A pipe, cigarettes. My mother was forever grousing about how dirty, what a dirty habit it was. She didn't have any health reasons because it wasn't a health matter at that time. But she didn't like cleaning up after it. So she was grousing all the time. I think maybe there was just a little competitive thing with my dad, that I decided I was first never going to get—he had a bit of a paunch. I said, "I'm never going to get fat and I'm never going to smoke." So even though I had opportunities in college, I always—

02-00:59:21

Rubens: Did Bill smoke?

02-00:59:23

Smelser: Let's see. Did Bill smoke? He smoked a pipe. That's right. Yes. And Philip smoked until he actually got a stroke and then he gave up cold turkey. But I was the son who didn't smoke. Of course, my mother never smoked.

02-00:59:37

Rubens: And what about drink? Just in terms of ceremonial or special occasion? Was there liquor in the household?

02-00:59:48

Jess: Excuse me, but we're coming down to two minutes left on the tape.

02-00:59:49

Rubens: I didn't ask where the name Neil comes from. Who were you named for?

02-00:59:53

Smelser: I don't know. My middle name is Joseph. My father's name. And he had people in the ancestry named Joseph. But Neil, I don't know where that came from. Bill was named after William Taylor Coleridge. William Taylor Smelser. And Philip was named—I don't know why but his middle name is my uncle's name, Sidney. But Neil, I don't know where it came from.

02-01:00:12

Rubens: So let's end for today and we'll begin with your first days at Harvard next time.

Interview #2 March 8, 2011

[Begin Audio File 3]

03-00:00:01

McIntosh: So here we are on March 8th, interview two with Neil Smelser. And Neil, last time we were here we ended off just on the cusp of you going out to Harvard from Arizona. There are still a few loose ends, though, that I was hoping we could cover, revolving around the question of support or competition that you felt having gotten into Harvard, both in your family and among your peers in Arizona. Getting a scholarship is no small feat and it can often engender a lot of jealousy, as well. Did you feel any competitive energies from your family or your friends?

03-00:00:49

Smelser: Well, I think I mentioned some of the ambivalence my father had about the East Coast last time and that was in the family. I know my mother was ambivalent about having her son that far away and only coming home periodically. It was the big break for me from the family. I was away from home from then on. So I wouldn't call it competition. It was a mixture. There was a thread of it a little bit perhaps, but it was the kind of usual tragedy that comes with the empty nest, it's always a theme there, proud as they were of my going.

Schoolmates? I was known in high school. This is, in fact, one of the little tensions in my life. The word brain doesn't have a positive connotation in high school, especially one that was totally rainbow, as I said. But I also kind of maybe adapted myself to be friendly and didn't pull rank on anybody. Also was not a snob in any way and so on. So I kind of engineered that kind of typical conflict. One of the things that now strikes me as not necessarily being competitive but probably was. One of my advisors was one of those who encouraged me to go to Harvard and when I got in he said to me, "Neil, you're going to be in a big pond back there. Expect to be a B, B- student." That was his prediction for me. I don't know that I remembered it but I don't know that I really took it as a great prediction or anything cosmic. But that's another thing that arises in connection with your question.

03-00:02:39

McIntosh: Right. And then another thing that we touched on but didn't really hammer home last time were your academic interests in high school and what subjects specifically you gravitated towards and what of your high school education you really brought to Harvard your freshman year.

03-00:02:58

Smelser: Well, I guess I would have to say that while I took a lot of everything in high school, I decided I wanted to get advanced math. And I went in advanced math classes. I took courses in the natural sciences, except for biology, and then I made up for that at Harvard. But I would have to say that I concentrated

in courses that would have to be called humanities. English, speech, drama. I took a course in commercial law. I thought that would be kind of important for me just as a boy growing up. But I would have to say that the writing and the English and the involvements that were close to my mother's, I suppose, because she was an English and Latin teacher. And so I took word study, for example, which was a small and rarely taken course. I decided to take that. That would be the way I would summarize it.

03-00:03:54

McIntosh:

And your father's interest in moral philosophy, was that—?

03-00:03:58

Smelser:

Well, there's no philosophy in high school. They don't teach it. So my interest in philosophy traced to my personal relations with my father. He would talk about it a lot and he would get me to read things. So that was the avenue in which my interest in philosophy and then I determined, of course, first year I went to Harvard to take a philosophy course.

03-00:04:20

McIntosh:

And so that brings us solidly into your first year of Harvard, I suppose. Do you remember what courses you took?

03-00:04:26

Smelser:

Yes, I do. In the first semester I was in required English. I opted out of that after one semester but this was a required English composition course that all freshmen had to take unless they tested out of it and I took it for one term, then tested out. I took beginning French. I took Philosophy 1, which was history of philosophy. Plato, Aristotle, the Greeks mostly. And I took Social Relations 1, which was the introduction to social psychology. I was already kind of taking a little bit of interest in that subject. So those were my courses in the first term. Second term I took anthropology, already now thinking that I was going to be in social relations as a major. I took a second term of French. I took a second term of—let me see. I'm just a little fuzzy on what the other course was in the second half of my freshman year.

03-00:05:28

McIntosh:

Sure. And we'll get to your acculturation to Harvard society in a bit. But first I wanted to see how those classes resonated with you. Were they more difficult than you expected? Less difficult? What were your expectations of Harvard's intellectual life and how did that reality conflict with it?

03-00:05:50

Smelser:

Well, I would have to say that I probably listened more than I thought to my man who said I'd get Bs. And I expected these courses to be very hard. I knew I was entering a new and much more competitive world and I wasn't sure about what my preparation was. As a result, I studied very, very hard. I had some curious things that might be regarded as disadvantages that were in my life at the time. One was circumstantial and one was psychological.

The circumstantial one was that in arriving at Harvard I discovered that Harvard had admitted more students than it thought were going to be enrolled by about 200. They were anticipating that part of their freshman class was going to go into the service because the draft was still on and they were picking people off. As it turned out, the educational deferment meant that they had this surplus. Thirteen hundred people, largest class ever, arrived and they couldn't house them. They couldn't house them all in their own freshmen housing. So they took 150 of us and put us in the gym dorm. We all slept on cots for a month. All you had was a cot. You just used the gymnasium bathrooms and they set aside this special exercise room, this place where you could study. But it was just a place to go sit. So it was a little awkward and it wasn't the way you like to be introduced to your first year in college.

Well, we coped and managed and got into housing a month later. Interestingly, I joined with one other guy, in a kind of little social movement and drew up signatures from everybody in the Block house, they called it, to present them to the dean asking for a \$100 rebate because they didn't supply us with housing. We did so very reasonably, politely. Sort of a strange thing for a first year freshman to go in and start causing political trouble, but we were extremely civilized about it and the university capitulated at once and gave everybody a \$100 check who had been put in the gym.

I also had a period of loneliness after leaving home. I was homesick. And periods of depression and a lot of intellectual fretting and self-doubts. So it was the first, I'd say, couple of months that I was—I remember the unhappy side very vividly. The interesting thing is, however, it didn't interfere with my studying. I studied very hard and very continuously in all these courses.

And in my freshman year I got straight As in all these courses. Apparently the Harvard people didn't predict that I was going to do that either on the basis of their own projections. That means I got on the dean's list. That was people who got three As and a B or better. And so they gave a little party for the dean's list people. And at that party, the freshman dean, a man I came to know later, came up to me. Singled me out and said, "Are you studying too hard? Are you studying too much?" He reflected his surprise. Well, I don't know. I was studying very hard. There was no question about it. And then I won this freshman prize on the basis of my—it was a competition called the Detur Prize, given to two freshmen for academic accomplishments. They gave us a choice of books. It was a prize of books. I chose Oates and O'Neill *Complete Greek Drama* as my choice of books. So I started off well at Harvard but there was in this context of a little bit of stumbling and staggering. I think that probably is not untypical for freshmen.

03-00:10:02
McIntosh:

It's interesting that there's a parallel there between your high school life in Arizona and your college life at Harvard, which is that you mention that in about seventh or eighth grade you decided that you were going to distinguish

yourself from Bill and from Philip academically, that that was going to be how you carved out a niche for yourself. Did you feel that pressure at Harvard, as well, or did the distinction just come, in a sense, by accident?

03-00:10:29

Smelser:

The reference at Harvard wasn't to Bill anymore or my early teenage years. I had already internalized an academic ambition. It was going on its own steam and I wasn't sort of looking over my shoulder at anybody. I was an ambitious young man who was determined—I didn't make up my mind how I was going to do. I just threw myself into it and studied extremely hard and organized very well and performed well.

03-00:11:01

McIntosh:

So I'm tempted to ask the same question that the dean asked, which was were you studying too hard and were there aspects of life outside of the college or social life within the university that you were also finding satisfaction in at the time?

03-00:11:18

Smelser:

I didn't have much of a social life my first year. I didn't date much. Didn't start dating until towards the end of my first year. Worked very hard. I had a very good relationship with a roommate that I was assigned with. We didn't live in the college dorm. We lived in a strange little university building called Little Hall. It was right on Harvard Square and it was just a few people living. That was the overflow building there, even, for these undergraduates. I developed a very close relationship with one roommate, a young man from Medford, Oregon. We had a lot of common interests and we became very close friends and remained so all through college and were roommates another year, as well, afterwards.

And I had a group, interestingly it had a lot of veterans in it, that I hung out with in the freshman union. But I'd have to say my social life was somewhat stunted. Call that studying too much? I don't know. I didn't get ulcers or feel that I was completely and totally wrung out. That, I guess, would be too much and I didn't experience that.

03-00:12:25

McIntosh:

Well, your association with veterans, that's interesting. Was there a lot of spillover from the war?

03-00:12:34

Smelser:

Oh, yes. This was 1948. You see, the GI Bill was in full force and there was a large group of veterans. It was a nice atmosphere that the veterans brought to the university. They were more mature. They weren't very confused. They knew where they were going. This was a good phase. I don't know that I chose them. I didn't have, "Oh, I'm going to get to know these veterans because they're more interesting." It just sort of worked out that this became a group of people who sat together in the freshman student union for lunches

and dinners and we became sort of a group, I'd say. Didn't do too much independently outside that but we joined together uniformly all the time for meals.

03-00:13:13

McIntosh:

Well, being somebody who had been working through much of his youth and had decided to really dedicate himself to studies, did you—you mentioned maturity. Did you feel a gap between you and your freshmen peers? Did you feel more mature than the people who were at Harvard with you?

03-00:13:36

Smelser:

Mixed. I think, compared to the freshmen from private schools, I think, I felt I was more mature. That's a fairly protected existence and I re-experienced this in Oxford in another way. I didn't look at the world so much in terms of maturity at that time. I was just sort of going about my business. There was one decisive moment in the Blockhouse when a bunch of us who didn't know each other got together and somebody, a competitive young kid, started a game in which we were supposed to display our knowledge about literature, philosophy, history and sort of—and it was kind of a young man's competitive—a little bit of macho stuff that you'd expect Harvard freshmen to engage in because they're all elites or intellectual elites. And in that game, which lasted hours, I found out I knew more than these guys and was really holding my own. I didn't then make a mountain out of that but it was a very interesting episode in my own life and I think it added kind of a bit of confidence in my own mind because I didn't have to worry about being snowed by all my classmates, that I was going to do okay.

03-00:15:03

McIntosh:

So I'm getting a picture of a pretty confident young man who is going about his—

03-00:15:07

Smelser:

Well, with the troubles I mentioned, yes.

03-00:15:09

McIntosh:

Right. Did you encounter any prejudice among the East Coast private school peers in terms of trying to stereotype you as a westerner, as somebody from the sticks, as you called it last time?

03-00:15:27

Smelser:

No. In every quarter in which I was, I never felt any sense of rejection from fellow students or teachers. I kind of established myself as a bright young kid who's doing well at Harvard. I didn't feel in any way that I was gauche or ill-mannered. So I didn't carry around any sense of inferiority, I have to say, and I didn't get any evidence from other people that they might think I was. So I was always fully accepted, I have to say.

03-00:16:12

McIntosh:

Well, that's interesting. So you're in this accepting atmosphere, you're feeling confident, you're doing well academically but yet you're feeling this depression or loneliness.

03-00:16:19

Smelser:

Well, I just left my family. It was a close family and I missed them and I was taking it out in kind of a certain amount of brooding and did have a little difficulty in sleeping and just unhappy moods. They were not debilitating but they were part of my life. That lifted after, say, two or three months at Harvard.

03-00:16:42

McIntosh:

And do you think that was just a product of becoming more comfortable in your environment or—

03-00:16:44

Smelser:

Oh, I think that was away from home syndrome.

03-00:16:48

McIntosh:

Fair enough, fair enough. So what about teachers your first year? Were there any teachers that really stood out to you or were really formative?

03-00:16:58

Smelser:

Yes. I had a philosophy professor named Raphael Demos who was a Greek immigrant who had sold papers in Boston and worked his way—had gone through Harvard himself and he was a most inspiring lecturer. Unfortunately, he got ill about halfway through the term and it had to be taken over by one of the people who was an assistant, a younger faculty member and that detracted from the course in my mind. The social relations introductory course was taught by senior professors of different descriptions in the department who were coming around to offer this interdisciplinary experience. And I was especially moved by a psychologist by the name of Gordon Allport, a very famous, world famous, psychologist at the time who was officially in charge of the whole course. Big course, 350 people in it. But I was intellectually inspired by him and to some degree by some of the other lecturers. And then Parsons came, Talcott Parsons came and gave two lectures on American social structure that were just jam packed and even at that early moment I was intellectually inspired as a freshman by this man who came and just gave these two lectures and then went away. He, of course, was a big figure on campus. He was chairman of the Social Relations Department and was already an internationally renowned sociologist. So I was inspired by some of the big men. Crane Brinton, a historian, taught the second half of my social science course and he also was a source of intellectual inspiration. I had quite a few teachers, I identified with them and liked them. My French and English teachers were nothing to brag about. They were young people, weren't on the faculty. They were pretty much like TA teaching fellows and while I got on well with them and while they taught me well, they didn't have that same aura of Harvardness about them the way these others did.

03-00:19:01
McIntosh: Those professors who did have that aura, like Allport and like Parsons, were they accessible? Were you able to go meet with them your freshman year and talk to them? Or were you entranced from afar?

03-00:19:16
Smelser: The first year at Harvard was mainly large classes, contact with teaching assistants. It's like Berkeley. I've always made the statement frequently that a big university, a big famous university is going to have this phenomenon in the lower division but you're not going to be close to any faculty members. The TAs and I got on well but I didn't—all my classes were big and I didn't feel particularly deprived by not having personal access to the faculty—later on I began to get more involved with professors, but the freshman year was fairly remote from the faculty.

03-00:20:04
McIntosh: And one of the reasons I ask is because the Department of Social Relations was fairly new at that point, was it not?

03-00:20:09
Smelser: Brand new.

03-00:20:11
McIntosh: And so I was wondering if they felt a need to go recruit undergraduates into their program in order to justify the department.

03-00:20:20
Smelser: Not particularly. It was an atmosphere of great intellectual excitement because it was an interdisciplinary effort to unite a number of the social sciences together in a special enterprise. There was a lot of optimism about the social sciences in general at that time. The literature shows it and a lot of ideas that were going to be very helpful in post-war reconstruction, that it was to be applied to social problems. So it was an optimistic phase. My whole undergraduate years were located in a very optimistic aura about the social sciences. So I didn't get any sense that they were hustling to get majors or anything like that. They were operating on their own steam in a highly inspired way and Parsons was the kind of central figure in this integration.

03-00:21:07
McIntosh: Right. And I would imagine it was that optimism and that aura that attracted you to the major—

03-00:21:13
Smelser: I was very excited about the subject matter.

03-00:21:15
McIntosh: And so was the decision to enter into that major a difficult one for you or was it—

03-00:21:21

Smelser:

In retrospect I'd say I was carrying on two remote struggles, one with my father and one with Bill. I took the philosophy course because I really thought I might want to go into academic life as a teacher of philosophy like my father. Even though I was inspired by Raphael Demos and a lot of the subject matter, it didn't really fill me with tremendous enthusiasm. I think this was probably kind of an unconscious working out of some things with my father but it was also the subject matter just didn't turn me on that much, even though I was very much interested in the issues and performed well and mastered the material. I wasn't really grabbed by it. Within social relations, you had psychology. You had social psychology. Clinical psychology, social psychology, anthropology and sociology. That was the mix. My brother Bill had already been at UC here for a period of time. He had started out, and he even got an MA in philosophy. You see, the influence of my father was there, too. Then he switched to clinical psychology for his PhD. So he underwent some kind of struggle, as well. But then he was sitting in the psychology seat at that time. So that made a difference in what I was thinking about in Social Relations and probably had something to do with my choice of sociology as the emphasis that I subsequently picked up.

In my second year, my closest relationship was with a psychologist, a clinical psychologist named Henry Murray who played a very big role in my life during my undergraduate years. And I'll come to that a little later.

03-00:23:25

McIntosh:

Now, this first semester and into the second semester when you're making these decisions, are you in contact with Bill through correspondence or—?

03-00:23:34

Smelser:

Yes. We had a lively correspondence. He would give me advice sometimes, reflect on things. A lot of humor. He and I had a relationship of telling tall tales, occasional practical jokes, teasing to some degree. It was extremely friendly kind of a thing. One time Bill and my younger brother Philip responded to every ad in one issue of *Popular Mechanics* and signed my name and gave them my address as having written in. So I began to get this bombarding, telling me how I could stop smoking and selling me this and that. Guns, everything. One guy came around to try to sell me some kind of a dynamo that I could put in my basement. They never admitted it but I knew they did it. So we had a lot of that going on. But my relationship with Bill during my whole college years was very close.

03-00:24:47

McIntosh:

Okay. So what about with your mother and father? Are you in correspondence with them, as well, while you're in Cambridge?

03-00:24:53

Smelser:

Yes. I have to tell something that is a little incriminating here. I took a little while to break the silver cord. My mother, when I left, volunteered to wash

my clothes, even though I was 3,000 miles away. So in the first semester I would mail her an army box full of dirty clothes and she would wash them and mail them back. Well, in retrospect that seems to me to be a strange tie to maintain with my family. And I did give it up after a few months. But in terms of correspondence, all you did at that time was write. You didn't phone. So I carried on a written correspondence with them that was steady. And then I went home every Christmas. The fellowship I had gave me train fare to go home. So I went home for two weeks every Christmas all during my entire undergraduate years. I didn't go home, I was elsewhere, during the short vacations. But no, I'd say I kept good ties with my family.

03-00:26:11

McIntosh:

I guess what I'm trying to do is get a picture of how much of your family life is still present with you in Cambridge and it sounds like you're getting some advice from Bill, some support from your mom and dad.

03-00:26:23

Smelser:

Well, yes, but I felt basically I was on my own. These relationships continued but my orientation was right there. While I kept my family intact in terms of correspondence and keeping up, no, I knew I was in another world.

03-00:26:41

McIntosh:

And so those decisions of what major you're going to be, those are completely independent decisions without any influence?

03-00:26:50

Smelser:

Yes. I didn't consult with either Bill or my parents about what I was going to study

03-00:26:54

Rubens:

May I just interject? Were you assigned an advisor? How is it—

03-00:26:58

Smelser:

Yes. He happened to be that man who picked up from Raphael Demos. His name was Rhineland. He was an old New Englander. I was his advisee. Faculty members had, oh, maybe ten advisees or some number. He was very good. He talked to me a lot about what I was doing. I chatted with him from time to time. Had us over for Thanksgiving dinner, which was very, very nice, in the first year. And he wasn't a terribly big influence on my choices. I was pretty autonomous in that line of thinking and in those lines of decisions but he was supportive and helpful. I'd go talk with him and it was a gratifying thing to do. Not a very big influence on me.

03-00:27:51

McIntosh:

And just one last question in this vein. What about financial support? Was the fellowship that you had received sufficient to help you live comfortably in Cambridge or were you getting financial support from your family, as well?

03-00:28:06

Smelser:

My support was a very generous one. It was this national scholarship that I talked about earlier. And tuition was \$500 a year at that time. It covered everything. My parents sent me a check for \$35 each month throughout the college years. That was all it was. So it wasn't a big drain for them. It was tied up with their income, which was not really high. They were both teachers in secondary schools, secondary school level. So I have to say I watched my money. I didn't live like a monk but on the other hand I knew that I didn't have that much spare cash to live on. I didn't want to make additional demands on my family. Interestingly, each year at Harvard I won some prize. Won sophomore Jacob Wendell prize, as a junior I forget the name and a senior prize, each of which carried five to seven hundred dollars of cash with it. And that was, of course, very welcome to me at the time and supplemented my fellowship. So I'd have to say it was a wonderfully generous existence that I have in relation to Harvard. Unbelievably. I wouldn't have been able to go if I were demanding several hundred dollars a month from my parents. They certainly wouldn't have been able to—it would have been a crunch on them. They didn't feel any pain in supporting me at that modest level. So I was well taken care of.

03-00:29:52

McIntosh:

And so you didn't have a need to get a part-time job in addition to going to school or anything like that? Did you have time to pursue extracurriculars, as well, or—?

03-00:30:02

Smelser:

I was in a couple of clubs. For a moment I was in an outfit called the United World Federalists, which was kind of a left wing organization advocating world government.

03-00:30:16

McIntosh:

That your father had been a part of, correct?

03-00:30:17

Smelser:

Yes. I did that in connection with my father's influence. That paled in my life. I didn't get too much out of it and I kind of faded out of that sometime in my freshman year. Yes. And then I later, not the first year, but subsequently, joined the Social Relations Society, which was a club of majors in that field.

03-00:30:39

McIntosh:

Were you involved in any athletics?

03-00:30:41

Smelser:

First year was required athletics. You just chose lines of activity that you wanted. I chose swimming, then boxing, then rowing. Sculling on the Charles River. There were three sports. You had to do three different sports the first year. And those were, of course, required but the choice of the ones were up to you. I'm not quite sure why I chose boxing. I got paired up with a guy who subsequently was New England champion in my weight range and I sort of

got beaten to a pulp every time I was in there but I stuck with it. I didn't get out of boxing.

My second year I played on the Adams House—it was my residence house—basketball team and we competed with the other houses. Strictly amateur. I didn't go out for any big sports. But I so enjoyed the rowing that—I'll talk about my rowing at Oxford later on.

03-00:31:50

McIntosh:

Right, okay. Well, if you feel comfortable about having covered the acculturation to Harvard, then we might dive a little more into the DSR and your experience in that program.

03-00:32:09

Rubens:

You wore a coat and tie at Harvard?

03-00:32:17

Smelser:

Required. Every meal. And if you didn't have one, they gave you one. It was really those days. So I would always wear it. They wouldn't serve you food if you didn't have a coat and tie on. And to classes, as well. I had a couple of faculty members who actually threw a kid out. Threw kids out of the class because they weren't wearing a tie. So it was very much that era.

03-00:32:45

Rubens:

Was this a new thing for you to—

03-00:32:46

Smelser:

Yes, yes. That was a very new thing for me. I adapted to it and I just did it and it added a little bit to my bill for clothes. But I shopped in Filene's Basement in Boston. I'd go down there and buy my sports coats and ties there. Well, I got used to that pretty quickly.

03-00:33:11

Rubens:

And Jess had asked you about any kind of status anxiety but I want to ask, on top of that, were there some young men that you met who really were the elite of the country, that you knew were from banking families or from political dynasties?

03-00:33:30

Smelser:

Let me think about that. Charlie Lamont, for example, was the son of the guy who gave several million dollars to build Lamont Library. He was a very rich New England businessman I think. I was not kind of in that world where—that was pictured in *Social Network*, the recent movie, of people aspiring to get into the right clubs with the right people. I was asked to join a waiting club. There were two waiting clubs. One was Pi Eta, one was Hasty Pudding. They were larger and they put on the Hasty Pudding show and so on and so forth. And I was invited to join Pi Eta, which was a so called waiting club from which you're chosen to one of the final clubs, which really—kind of aristocratic ones. My sophomore roommates invited me to join Pi Eta and I

was approved and I said no. I wasn't especially interested in that side of Harvard. It was a distraction. I was basically an academically motivated young man. I didn't have social pretensions to become part of that aspect of Harvard so I said no. But being asked, I didn't feel rejected. So there you are.

03-00:34:56

McIntosh:

Just while you guys were talking I was thinking back to that petition that you circulated your freshman year. Did you have any political beliefs that were beginning to grow at the time or any ideological inclinations that you can remember?

03-00:35:16

Smelser:

Well, they showed up. I guess I'd be on the liberal side of the student body. I didn't join the Young Democrats, although I would have if I had chosen to join a political group. I did join the Federalists, but that was somewhat fleeting, kind of a goodbye gesture to my father, I think. I was ambivalent in that role of taking up this little social movement to get money back from the college. Not that I thought I was going to get punished for it, although in a way you might think a kid coming in from out of town and becoming even a gentle political activist, not exactly the thing you want to do. So I think that was behind the ambivalence I had toward joining this little social movement, though I did it. I carried forward and I got identified as the leader, one of the co-leaders of it.

03-00:36:10

McIntosh:

Yeah. The United World Federalists. That seems kind of part of that post-World War II optimism, too. Like the kind of globalist moment.

03-00:36:16

Smelser:

Yes, it was. A world government was the thing. I got somewhat alienated from what I came to see as an unrealistic visionary quality about it. It wasn't a very practical movement. It was just part of my withdrawal from it, I think. I've had a longstanding—throughout my career, a longstanding skepticism about utopias of any sort and this was one of the early disillusionments that I experienced.

03-00:36:49

McIntosh:

We're digressing a little bit. We'll get back to your freshman year quickly. But to me, one of the distinctions between Chicago and Harvard after World War II is that there are many more world government advocates at Chicago. That's where Richard McKeon and Hutchins are drafting a world constitution. Harvard seems to be going in a slightly different direction, kind of more—less utopian maybe.

03-00:37:16

Smelser:

Well, yes. This was a little corner. It wasn't a very active society. It was small and, as I say, it reflected my transition from my family as much as anything else, or rather than any general big attitudes towards the institution.

03-00:37:30
McIntosh:

Right. Okay. So if you don't mind, we might go into the Department of Social Relations work a little bit. So when did you declare your major, again?

03-00:37:40
Smelser:

I made up my mind at the end of my first year. I took the second half of the introductory social relations, which was run by George Homans, who later played a little role in my life, and I took the anthropology course. I was thinking seriously about choosing anthropology as my main focus within that interdisciplinary department but I was completely turned off by one of the professors. A world-famous man named Clyde Kluckhohn taught the course. He was evidently bored. He assigned his own textbook, his own book, and let us read it and he told jokes. His heart wasn't in it. And I found it a chore to go because it wasn't very stimulating. Then, at the end of that course, I and another chap, who was a second winner of that Detur Prize I mentioned, went to Kluckhohn and began talking about the subject. I did go see the professor, began talking about the course and the final exam and so on. Kluckhohn spoke to us. He understood, we were anxious about the exam and so on. He said, "Well, I'll give you—look at the exam last year. It'll give you an idea of what sort of thing is going to be on the exam." So as it turned out, last year's exam was this year's exam and I lost respect for that man for doing that. I'm not sure why I got such a negative reaction but I thought he's lazy. This man is intellectually lazy and he's kind of cheating a little bit for us and I didn't respect him for that. Later on I got to know Kluckhohn when I was in the graduate school and we talked a lot together and he gave me some advice about going to Berkeley and so on and so forth. So it wasn't a total break but I remember having this reaction to that course.

03-00:39:48
McIntosh:

Were there any professors that were on the opposite side of the spectrum of Kluckhohn? That were really encouraging to you and who you respected off the bat? You mentioned Parsons.

03-00:40:01
Smelser:

I never even spoke to Parsons. My decision to go into social relations didn't have much to do with personal interaction with faculty members, being encouraged by them.

03-00:40:12
McIntosh:

So do you recall the general requirements of the program and what was expected of you after you've made up your mind to become a major in the department?

03-00:40:23
Smelser:

You were required to take a certain minimum number of courses. We had breadth requirements, too. You had to take a major course in the humanities, in the social sciences and in the life or physical sciences. That was part of their general education program. But you had to take a lot of your courses with choice in the social relations department. I began to gravitate more

towards sociology courses. I took one course from Parsons in American institutional structure, a course in political sociology from Barrington Moore. At the same time, I continued the interest in psychology. Took a course in abnormal psychology and then I took my elective courses. I took a course in American and British drama, reflecting, in a way, my father's big interest in drama. And kind of an anomaly course in American and British poetry, which I had never really considered myself either very good at or kind of turned on by poetry but I did it. I'm not quite sure why I did. Found it a difficult course to take.

03-00:41:39

McIntosh:

Do you have any hunches as to what drew you to this Anglo-American dimension? You mentioned sort of like American English poetry, right, British drama.

03-00:41:50

Smelser:

At the same time I took a year long course in Far Eastern history, in Chinese and Japanese history. It was just partly experimental. I wouldn't call my choices of courses parochial in the sense that is implied by your question. I ranged out more broadly.

03-00:42:09

McIntosh:

And was that an emphasis of the Department of Social Relations, to try to get their students as broad of a foundation as possible?

03-00:42:17

Smelser:

Oh, yes. That was its big bragging point, here we're interdisciplinary. We're not tied into narrow viewpoints. We're thinking big things. Parsons had this big imprint about the importance of general theory that was hanging over the department. So yes, this catholicism of interest was something that was very much at a premium and I think it sort of added that stamp of interdisciplinary interest that has always been part of my career.

03-00:42:44

McIntosh:

And so what were some of the first courses that you took in the department in the social relations that really did pique your interest, that were either in anthropology or social psychology or sociology?

03-00:42:57

Smelser:

Well, I was interested in most of the material. In my sophomore year I took a course from Barrington Moore in political sociology. Moore was a kind of a weird teacher. He was somewhat bored with his own subject matter and he was bored -seemed to be bored with the world. But he assigned such interesting stuff that I got truly engaged in the subject matter of the course. I wrote a paper comparing social change in Japan and China in that course. Moore just went crazy over that paper. He just thought it was such a professional job. It didn't matter what else I did in the course. He got so turned on by that paper that he simply decided I was an A student. I was going to be the A student and it was on the basis of that one experience in that one

course that he became a fan of mine. He was an ardent enemy of Talcott Parsons. They had the worst relationship you could possibly imagine but it was Parsons and Moore who recommended me later to the Society of Fellows and it was all from that echo of that sophomore paper I had written for Barrington Moore. And I got really turned on by that.

03-00:44:17

McIntosh:

Were there any other specific papers that you recall doing or projects that you recall embarking on that really did turn you on in addition to that paper?

03-00:44:28

Smelser:

I wrote in a clinical psychology course a paper arising from a relationship I had when I worked at the grocery store near the country club. One of the cashiers and I developed a strong friendship that became kind of erotic. She was maybe eight years older than I was. But it first started out with flirtation and then a little touching and it's all in retrospect very innocent. But it was a meaningful psychological relationship with me and very different from the kind of dating I was doing in high school. So I chose that, my development of a relationship with her, as a subject to write about in my clinical psychology course. That was one of the courses that turned me on, Henry Murray's course in dynamic psychology. Murray picked that paper out, called me in and talked about it. So you asked me about other inspiring papers. That was an inspiring paper.

03-00:45:38

McIntosh:

So you're getting a lot of praise for some of the papers that you're writing. Was there any push for you to publish them as an undergraduate?

03-00:45:49

Smelser:

Only as I became a senior. I did a senior's honors thesis that my advisor asked me to publish. And I wrote it up in a halfhearted way after I had gone to Oxford but it never got published.

03-00:46:01

McIntosh:

Now were you required to take any methodology courses?

03-00:46:05

Smelser:

Yes, statistics. I took introductory statistics in my junior year with Samuel Stouffer and I decided I wanted to take advanced statistics, the PhD requirement statistics, as an undergraduate. Already committed. I got moving. I had this professional interest developing, as well. So I took advanced statistics from Frederick Mosteller, who taught it. It was extremely difficult at first but then I got into it and it was a joy. I just loved it.

03-00:46:36

McIntosh:

Well, what's interesting in this Parsons/Moore split is that you do have different methodologies on the table and I was wondering if, in the department of social relations, those fissures were understood by the undergraduates and

if you had the option to align with one or the other or if the department was structured in a way where those politics were outside of public view.

03-00:47:02

Smelser:

No, they were open enough. You got the differences between the teachers. They would so often criticize each other. Murray and Allport each criticized each other in their two respective courses, for example, and they were rather far apart on the issue of psychoanalysis as an approach. Moore wasn't at all shy about voicing his political opposition to Parson's viewpoint and to Parson's type of theorizing. Parson's tended to be the above-it-all type theorist who, at least on the surface, let all these criticisms bounce off of him. But nonetheless, he stuck to his guns and defended a certain theoretical position. I knew the field was not unified at all and there wasn't any secret about it.

03-00:47:50

McIntosh:

Now, within the department, what was the emphasis on learning the great works in the different disciplines, as compared to learning new techniques? You mentioned integrating psychoanalysis, for instance, which was a kind of cutting edge advancing the discipline.

03-00:48:10

Smelser:

I would have to say that it was a mix. Certainly both Moore and Parsons, who were two of the bigger influences, were macro and historical in their interest and it entered into all of their teaching. Over on the psychology side, it was a lot more empirical nitty-gritty and experimental and so on. And, of course, they were riding the wave of the studies in industrial sociology, which were highly practical and other things. It was a big mix and kind of to answer your question, I have to say both.

03-00:49:01

McIntosh:

To me, it seems like sort of a daunting environment for an undergraduate. There is this mix. There are these different political impulses within the department, these disputes among the teachers. How were you able to find your niche within the department and find your way within the department? Did you have a clear understanding in the beginning?

03-00:49:23

Smelser:

I stayed kind of catholic in the department. In other words, I was much influenced by Murray in depth psychology. I chose as my undergraduate honors project—you did write a thesis as an honors undergraduate there—a social psychologist named Gardner Lindzey who was later to become a very close person in my life. And I wrote an empirical—tight little empirical study of the social organization of several women's dormitories in a nearby college. Friendship patterns, a sociometric study. It was highly empirical, based on interviews and the social structure of a microorganism—several microorganisms there. [This thesis is discussed at the end of this interview.] At the same time, I was really drawn to Parsons' general theorizing and that's what turned me on most of all, was the—I was very much interested in theory.

So I had a real multiplicity of interests that carried me on, even as an undergraduate.

03-00:50:26

McIntosh: And when in this process were you first up for the Society of Fellows?

03-00:50:31

Smelser: That was later. That's when I went to Oxford.

03-00:50:34

McIntosh: Okay. And so you go to Oxford when exactly? That's—

03-00:50:37

Smelser: 1952, the fall of 1952.

03-00:50:40

McIntosh: And when did the application process begin—

03-00:50:43

Smelser: Well, it's multiple. I had to be approved by Harvard and that was in the early weeks of my senior year. I got approved by Harvard. Then I had the issue of, once having been approved by Harvard, do I apply in my home state, Arizona, or do I apply in Massachusetts? I made the pragmatic decision to apply in Arizona. Less competition, even though it was paired with California as a competitive state—in the western states. Six states were together in each region. So I decided I'd go to Arizona. I had a better chance. So I had to go to a regional interview in Tucson to compete with some other Arizona applicants. One of the interviewers, a quite parochial man, got into a fight with me about my honors thesis about studying these girls' dormitories. He thought it was really stupid. He thought I was kind of a weirdo of some sort. I'm surprised he didn't block it. But they approved me. And then I drove to Pasadena to be interviewed along with the two finalists from each of the other states. There were twelve of us and four of us were going to get fellowships. So I drove over to Pasadena, had an interview.

There was one story with the interview that has to be in my interview and that has to do with Robert Gordon Sproul. He was a member of the interview committee. Big man. Huge voice. Lot of laughing and joviality. And as the interview was going on and they were asking me about my major and my athletics, the usual thing a Rhodes interview has in it, Sproul came in and said, "What groups are you in at Harvard?" I said I was in the Social Relations Society and, as a matter of fact, I was chairman of that or president of it at the time, and I said, "This is an undergraduate majors society. We have academics both visiting and domestic come in to talk to the undergraduates about their work and subjects that are relative and the field is mostly—" and I said, "It's an academic society." He said, "Did you ever engage in any political activity?" It's 1952. Okay. Berkeley was right in the middle of throes of the loyalty oath crisis. And I didn't think too much about the question at the time. So I responded and I said, "We did one thing. We mailed \$25 to the professors

protesting against the oath at the University of California.” And so Sproul was on the other side of this. He was fighting for the oath. The group, the interview group, burst into guffaws at this point, laughing at Sproul. And then Sproul had to join into the fun, so this—but I didn’t know what Sproul had in mind—I later thought could that have been a trick question? Did he know in advance and was he checking up to see if I’d give an honest answer or a not honest answer to the question? I don’t know. Maybe I made too much of it. Maybe it was an innocent question. I don’t know. But nonetheless, it kind of coincided with this very interesting political situation at Berkeley and set off a torrent of humor and laughter when the answer came out.

03-00:54:22

McIntosh: So a potentially explosive situation kind of defused, it sounds like.

03-00:54:27

Smelser: Was a positive thing. If I had hemmed and hawed and covered up, I think I’d have paid a price for that.

03-00:54:37

McIntosh: Just out of curiosity, was that your first sort of real personal encounter with the kind of Red Scare issues after World War II and things like the loyalty oath?

03-00:54:49

Smelser: I was deeply interested and disgusted by the McCarthyite phenomenon that was affecting Harvard as well as a lot of other places. I was never a true activist. However, in my sophomore year I won a prize called the Jacob Wendell scholarship. They give a dinner every year. I’ve been invited every year to this dinner ever since. It’s the sophomore prize, right, and that was that \$500 prize. And it’s run by the Wendell family, which is an ancient New England, Connecticut-based family. They gave a lot of money to Harvard and they named it after the donor. Well, one of the Wendells who was still taking an active interest in the fellowship, after the dinner tried to recruit me into the CIA. He was a CIA person. And I think he made a habit of going around picking off bright Harvard undergraduates to try to get them into the CIA. And he wanted to send me a form in which I was going—I’d reveal the usual things that would be preclearance. I wouldn’t have joined until after college but he was priming me and so on. When I got this paper, I just told him, I said, “I’m afraid I’m not interested.” And see, I had that sort of feeling about it. That was just a totally foreign avenue for me to be even thinking about at the time. So I guess that reflected, as much as anything else, my political dispositions.

03-00:56:31

McIntosh: That’s interesting. So it had really no allure for you, that kind of government service.

03-00:56:35

Smelser: No, none. No, I was a committed academic by that time.

03-00:56:39

McIntosh: Interesting. Okay. Well, to get back to the environment of the Rhodes Scholarship interviews. How did you feel about them? Did you feel confident? Were you—

03-00:56:50

Smelser: I was scared to death. It was a very competitive thing and I was not altogether certain that I was going to succeed, even though when I was in seventh grade, in my big burst of academic ambition, I came across Rhodes Scholarship in some kind of encyclopedia or some little book that I came across and it sounded so romantic to me. I said to myself, "I'd really like one day to have one of those." And I even went home and told my mother that I'd come across this and I would really be interested in it. She sort of laughed. She didn't poo-poo it. As a matter of fact, she kind of supported me. I knew she didn't believe what I was saying. So I had my eye on it. It was a big prize in my life and I was, of course, absolutely delighted when I got it.

03-00:57:51

Rubens: I think we should stop to change the tape.

[Begin Audio File 4]

04-00:00:03

Rubens: Why did the Social Relations Society come to support the professors at Berkeley?

04-00:00:08

Smelser: Well, I believe the mechanism—if I recall correctly. It's a little dim in my mind. But one of the men we had to come and talk to the social relations society was David Krech. He was a professor of psychology here at Berkeley and he had been adversely affected in the oath. I think he was one of the people who was fired for a period of time. I got to know him later here at Berkeley. But he came to talk to us and it was right in the heat of the Berkeley crisis. And in a kind of a fit of anger at the situation in Berkeley we simply voted, the executive committee of the Social Relations Society, voted to send some money.

04-00:00:51

McIntosh: Okay. So I want to close out the Rhodes Scholarship application process just with one last question, which is: we know that when you were in seventh grade you stumbled upon a mention of it in some reference book or something. But as an adult, did you have a better idea of why you actually wanted a Rhodes Scholarship and what you wanted to accomplish with it when you were going through the process?

04-00:01:19

Smelser: It didn't fit into the grander plan that I had intellectually. It was a huge honor. It really fed into, in a big way, really determined that I was going to go after it after I went to Salzburg and spent that very decisive summer in Europe.

04-00:01:40
McIntosh:

Well, yes. Let's talk about that. Was that your first experience abroad?

04-00:01:45
Smelser:

Yes. I had never been abroad in my life. I did not initiate the application to apply for the Salzburg seminar, which was a summer seminar run by Americans, teaching American culture, literature, history and so on to approximately fifty European students in the summer. Or six weeks in this really romantic castle, Schloss Leopoldskron just outside Salzburg. And it had been going for three or four years. It was started in '47 and I was in '51. By that time I was developing closer relations with the faculty members and getting something of a reputation as an undergraduate in the department. Both Florence Kluckhohn, who was the wife of Clyde Kluckhohn, and Parsons recommended me strongly to the Salzburg seminar. Parsons had been there a couple of years before and he thought—he told me later he thought I was an ideal candidate. So it's four Harvard post-graduates and four Harvard undergraduates to join these fifty European students. So I was just totally overwhelmed when I got that opportunity. I decided not to go home that summer. I decided to go to Europe on my own before the seminar began.

So I went to Europe five weeks before the seminar. Went to London, visited the family of one of my New York advertising friends who was English whose parents lived in a London suburb and I stayed with them. Then I bought a bicycle and I bicycled up to Oxford to take a look at it and then from Oxford I'd go south to Newhaven and I got on a boat with my bike. Went to France. All alone. This was all alone. Bicycled for a month completely solo. Crossed France to Salzburg, sleeping in peasants' fields, sleeping under bridges. I slept in a housing project in Paris and it was a glorious experience for me to do this. I had very little money so I was scrimping. I had one meal every couple of days in a restaurant. I have such a romantic feeling about that month in France. Bill had been in France a few years before, spent several months, so that also kind of attracted me. And I had visited one of his friends in Paris at the time.

And so then I went to Salzburg, which was this kind of glorious experience. I was assigned just to be kind of in the social and cultural, intellectual dialogue, and to go in some classes and to just be a participant. No other specialized assignment. And these Europeans were all older than I. I was probably the youngest person, with the other undergraduates, in the Salzburg seminar and I felt very apprehensive about my ability to pull my oar because these were European intellectuals. I had a picture of them as being ultra sophisticated and I still had this idea that I was something of a yokel even though I'd had three years at Harvard. It was a very communal atmosphere, as you might imagine. Everybody really slept in the same big enormous loft on cots during these six weeks.

04-00:05:09
Rubens:

This was all-male?

04-00:05:10

Smelser:

Male and female. Well, Harvard was all male, so all of us Americans were male but the Europeans were mixed. And I even had a little romance there with a Dutch woman. Now, I speak from my point of view. The seminar had a lot of intellectual and political dialogue and there was a lot of residue of feelings from World War II, hostility. There were people from enemy countries there, which had been killing each other years before. I truly had the feeling that I was going to be overwhelmed by the culture and the intellectual level of this setting and it turned out I wasn't. This was just a complete and happy surprise for me that I was able to hold my own in very abstract and general and value laden discussions and felt a full member. Right? And it was a very engulfing experience. It was probably the first defining experience for my odyssey thinking, because it was like a journey, an ocean voyage that was so wonderful. Everyone was so emotional at the end. It just sort of not only gave me an enormous amount of confidence but it also stimulated this lifelong interest in European culture. I've spent five years of my life in Europe, mostly England, but this just was what triggered that intellectual and cultural interest in all of Europe.

04-00:06:48

McIntosh:

Do you remember who your American peers were at the conference?

04-00:06:50

Smelser:

Yes, yes. There was a young boy named Bob Herzstein who later became a very famous lawyer in Washington, DC. I had Donald Fagg, who was an anthropology graduate student. Don Meyer, a graduate—I guess I was closer to the graduate students than I was the undergraduates. A philosophy graduate student named Bob Anderson that I was close to. Donald Meyer was a historian who went to UCLA. We formed a working relationship. We went to Vienna a couple of times together, so many Americans did, along with a couple of Europeans. Yes. Intellectually a very high level. Teachers there were Henry Steele Commager, David McClelland, George Homans was a teacher there. So it was kind of a Harvard enterprise but with some others there. And Howard Higman a man from Colorado I became closest to. He was a political scientist. It was just a quite glorious experience for me.

04-00:07:51

McIntosh:

What was the structure of the conference?

04-00:07:53

Smelser:

It was not a conference. It was really courses. American literature, Alfred Kazin the critic was there at the time. He gave a course in American literature. David McClelland, one in psychology, Henry Steele Commager in American history. It was just like courses. They met a couple of times a week. They didn't require you to write papers or anything like that. There weren't exams but they were really presenting facets of American culture to post-war European young scholars who we hoped, or the founders hoped would be influential in their own societies. The Salzburg seminar still exists, although

it's become more vocational. You invite trade union leaders, you invite specialists, accountants and so on. I think this great general mélange of scholars pattern, which I found so attractive, didn't last. I've gone back to Salzburg many times. I'm so romantic about it.

04-00:08:51

McIntosh:

Well, that summer just seems so fascinating to me because you go from this period of intense solitude to a period of intense—

04-00:08:59

Smelser:

Collective. It was my first. I was surprised because I had always considered myself, if you think of self-image—and this was during my first several years at Harvard—I considered myself, despite the fact that I had friends and dated and went around and had social contacts, I considered myself fundamentally a loner. I was rowing my own boat and I was going my own way and not an introvert because I didn't—it didn't express itself that way. But psychologically the inner feeling was a solitary—or more solitary than social. And this totally took me by surprise. I sort of felt this is not like me to get involved in this kind of collective enterprise that was so warm and engulfing and emotional. And so that, in a way, sort of—it was very important from that standpoint of reshaping my own image.

04-00:09:54

McIntosh:

Now, that was an atypical summer for you, right? Previous summers were spent not in Europe?

04-00:09:58

Smelser:

Yes. After my freshman year I worked in Phoenix on the *Arizona Republic* as a proofreader. Then they advanced me to a reporter. What I did was I'd come there in the summer and I would fill in on the vacation times of all other reporters who were going on vacation. So for two weeks I'd be on the federal beat, for two weeks I'd be on the police beat. For two weeks I'd write feature stories, things of that sort, and then I'd sometimes work on the editorial desk. So I had all kinds of different assignments in this. I now know that a lot of confidence was put in me to assign me into this kind of role as a twenty year old youth and working under deadline and writing and doing some editing and so on. Salzburg was away—then I came back to Phoenix at the end of my senior year before I went to Oxford. I worked there for a while and then I worked as a carpenter's apprentice in Washington, DC just to earn some money so I'd have some extra money to have at Oxford.

04-00:11:02

McIntosh:

And what about your summers before your sophomore and before your junior years?

04-00:11:09

Smelser:

Well, before my senior year was Salzburg. That was my European summer. Sophomore year I was a reporter. After my freshman year I was still a proofreader. Because I had been a proofreader on the *Arizona Times* in high

school they took me on at that. I have to say that this journalist experience was one that really, really stuck with me. I think it's had a really benign influence on my writing and academics tend to be unclear people and I've always prided myself on a direct and clear prose style and I attribute that most of all to—I don't know what you attribute it to but journalism certainly plays an important part in developing that style.

04-00:11:50
McIntosh:

Well, just before we sort of officially embark to England, I'm just trying to figure out where this New York aspect fits in.

04-00:11:57
Smelser:

Ah, here's what's happened. When I was in Phoenix, a man by the name of Bill Taliaferro worked for the *Arizona Times* where I had worked part of the time as a proofreader. He befriended me. He was a sportswriter and he and I had a lot of sports interest in common. He had a wife by the name of Jane Taliaferro that I met a couple of times. Really liked her tremendously. I developed already a friendship when I was in Phoenix. We went to some sporting events together. We really hit it off. And then they went to New York about the time I went to Harvard. And they contacted me when I was at Harvard. And then they invited me to come to New York to stay with them during the spring vacation of my freshman year. So I went down there and, of course, I was—it was New York. I hadn't been in New York ever before in my life. They lived in an apartment on East 55th Street in which the movie *the Lost Weekend* was filmed. They had lived in it. And they were in the advertising world. They were copywriters for big Madison Avenue advertising firms and they were in the culture which—if you've seen the television program *Mad Men*, it's not too unrealistic. They overplay the drinking and they overplay the infidelity, but nonetheless it's a pretty fast life that I got into. And developed relationships with a number of their friends, who they'd always have around, and then their friends would ask me around and so on. So I became a kind of a—and my close friends in New York touted me as this brilliant young Harvard genius who was going to really shake the world. So I felt very, very welcomed there and one of these guys was trying to influence me to go into advertising. It was really a very—kind of, in a way, a quirky part of my existence but I'm quite fond of it.

04-00:14:01
McIntosh:

Was it alluring in any way? Were you ever tempted to go into advertising or anything like that?

04-00:14:06
Smelser:

Never. One of the things I observed is how practically all the people, when you began to talk to them in any depth, all felt really resentful at being in advertising. They thought they should be in something more creative, like playwriting or novels—they thought they were too commercial, too tainted and so on. There's a lot of self-hatred in that advertising industry and I just kind of got—I was fascinated by it but turned off. I had absolutely no interest.

04-00:14:32
McIntosh:

Okay. One last note you had here before getting to Oxford is the 1A and draft story. Do you know—

04-00:14:37
Smelser:

Oh, yes, yes. The Korean War was on. This is the summer of '52 after I graduated and I was no longer educationally deferred because I had been— When the Chinese went into North Korea, a huge alarm, that was my junior year, came over my classmates and me about being pulled out of college into the draft. And that didn't happen. The educational deferments continued. But my educational deferment ended. However, there was a federal statute that put Fulbrights and Rhodes Scholars as eligible for educational deferment. But my draft board didn't choose to read this or didn't choose to honor it and they classified me 1A and said, "You're going to be drafted," which would have meant postponing for two years, because that was the term you served. Two years going to Oxford. I was not unpatriotic. I didn't have an ideological antagonism to being in the service and I didn't have any antagonism toward the Korean War. So it was not a political issue for me. It was a personal issue. I requested an interview with the local draft board to plead my case and I went before them and it turned out to be a very right wing parochial group. In fact, one of my favorite customers at the grocery store where I worked was on that board. We had a good relationship there but he turned out to be some kind of local fascist who was really, really, really punitive. I got really beaten up in this interview. They said, "Why didn't you serve in the ROTC in high school?" I said, "It was voluntary. You didn't have to one way or the other. I thought I would be in physical education programs rather than ROTC." They said, "What makes you think that going to Oxford is going to serve the country any better than carrying a gun in Korea?" That kind of very hostile and punitive stuff. They wondered what kind of economics I was going to be studying at Oxford and it showed the level of this guy's sophistication. He said, "Are you going to be studying socialist economics or Churchill economics?" So really something silly. But there was absolutely no bending them. They classified me 1A. They rejected my appeal.

I had only one avenue left and that was the state board, state selective service board. So I decided, well, I'll take that avenue and I appealed to the state. Something very interesting happened. My dad, who was not your usual political string puller contacted the head of the whole educational district, the superintendent of the whole educational district and asked him to put in a word for me with the state draft board. And the state board reversed the local board and I, by the skin of my teeth, was able then—I was thinking in the middle of that somewhere I'm going in the service. I really had that full anticipation but then suddenly got reversed and I—

04-00:18:05
McIntosh:

So was that purely a feeling of relief or was there ambivalence about it?

- 04-00:18:09
Smelser: No, I pretty clearly preferred to go to Oxford than to serve in the military. I imagine, being an honors graduate, I got a summa at Harvard, they probably would have put me in some kind of technical branch and I wouldn't have been in the combat but I didn't want to interrupt my career. It was a personal matter.
- 04-00:18:31
Rubens: Did any of your fellow classmates go?
- 04-00:18:33
Smelser: Oh, yes, yes. And got killed. Some of my high school friends, a few of my high school friends were killed in Korea. I don't know what the fate of other graduates from Harvard were because we had dispersed at that time. But nonetheless, this was an interesting episode just before going to Oxford.
- 04-00:18:53
McIntosh: So you sort of narrowly escape that.
- 04-00:18:57
Smelser: Skin of my teeth.
- 04-00:18:58
McIntosh: Right. And then so after that, you know for sure that you're going to Oxford, correct?
- 04-00:19:04
Smelser: Yes.
- 04-00:19:05
McIntosh: And so when do you depart?
- 04-00:19:07
Smelser: I'd met my first wife as a junior in college and we fell in love and we determined that we were going to get married. However, the Rhodes Scholarship forbade marriage at the time. It was all male and they forbade marriage. Cecil Rhodes thought it was bad for you or something. So we couldn't get married. I'd lose my fellowship, which was what I was going to have to live on. So she came with me to England and she lived in London and worked for the United States Air Force for that period and we traveled together and everything and got married at the end of the fellowship, of the Rhodes Scholarship.
- 04-00:19:57
Rubens: What is her name?
- 04-00:19:58
Smelser: Her name was Helen Margolis.
- 04-00:19:59
Rubens: And how had you met her?

04-00:20:02

Smelser:

Radcliffe. I met her in the general social comings and goings. Harvard-Radcliffe. I'd had another girlfriend before that didn't pan out in my junior year. But we met each other in the spring of 1951. Before I went to Salzburg, and fell in love. So she came with me on the SS United States. All the other Rhodes Scholars went on the same boat. I was the only one who didn't join the whole group for that reason.

04-00:20:32

McIntosh:

So was Helen excited about the opportunity to go to London or was this something she was—

04-00:20:38

Smelser:

Oh, yes. Oh, yes. No, no. It was consistent with her feelings about the world. She didn't feel in any way dragged along.

04-00:20:48

McIntosh:

Were you eager to have her there, as well?

04-00:20:52

Smelser:

Oh, yes. The unhappiness in our relationship had not appeared yet.

04-00:21:00

McIntosh:

I guess I'm trying to get at less the personal dynamic between you two, which sounds strong at that point, and more of the tension between your romantic life and the really real industry it takes to complete a Rhodes Scholarship and partake in that culture.

04-00:21:20

Smelser:

Well, once again, I went to Oxford with partly the idea that it was an advancement in my career, which it was. It's always helpful. It's what they call cultural capital to have had a Rhodes Scholarship. There's no question about it. And you capitalize on it in ways I'll tell you about later. But I was also kind of filled with the idea that in some sense it was an interlude in my life. First of all, I couldn't study my chosen field. They didn't have it. Sociology did not exist at Oxford. So I thought of other options. There's a field called philosophy, physiology and psychology. There was a field called philosophy, politics and economics and I chose to read that. All kind of related to my general social science/humanities interest. And certainly influenced me a great deal later. But I did have the feeling that I was—I suppose you would call it I wasn't going to be as driven as I was at Harvard and that was what led me to decide I was going to get into full-time rowing in my first year at Oxford, which I did.

Turned out to be a huge commitment. That's a religion. Rowing is a religion there, even at the college level. I had only the sculling experience to bring with me, so I was really a beginner. During that year I got on the crew. Got advanced. I sort of advanced in skills from lower ranking boats to upper ranking boats. In the spring, I was already called upon to be on the first crew

of Magdalen College and was being groomed to try out for the blues, which is the whole Oxford University team that runs that Oxford/Cambridge race. So I kind of propelled myself upward in this rowing hierarchy. But it took all my time. We rowed three hours a day, six days a week. You were totally exhausted by the time that three hours was over. And I studied hard and I did well in my studies but I felt that it was a very different kind of feeling from my Harvard experience, largely because of this rowing involvement.

04-00:23:52

McIntosh:

I'm hoping we can flesh out that feeling a little bit. You mentioned a different mental orientation that you had, maybe kind of taking a slightly more relaxed approach to study—

04-00:24:07

Smelser:

A way station. I saw it as a way station.

04-00:24:10

McIntosh:

Seeing it as a way station. What about the environment that you encountered there? How did the environment differ from the daily life of Harvard?

04-00:24:18

Smelser:

Well, I can distinguish between the academic and the social environment and really have to. The academic environment was a very good one and this was all tied up with the tutors. I decided to do an undergraduate degree rather than a graduate degree because the undergraduates, they pay attention to the undergraduates at Oxford and you have these tutors and there are various subfields of philosophy, politics and economics that you had to take and there are a few electives. And I had these relations with tutors. They were all good from the very beginning. The English have a system, at Oxford anyway, where the dons spot their good students. They identify them. And these colleges compete like crazy with each other for the number of first class degrees they're going to get and that's a mark of their status is how many undergraduates get firsts. So these dons pick out the firsts or the potential firsts and they actually cultivate them, your sponsors. They're not the examiners, these dons. You write papers for them every week and read them and get criticism and they give you feedback and you take what they called collections, which were trial run exams at the end of each term and they would judge them. "Ah, this looks like you might be able to get a first in this." They really just joined you rather than evaluated you in some critical way because all your academic results depended on the exams at the end of the second year. All of them. Nothing along the way would contribute to your final ranking, final academic ranking at Oxford. So this was a new experience for me and very positive in the sense that all my tutors and I had a good relationships, very quickly, I was swimming in that atmosphere and I liked it. I liked preparing these essays. I liked the response they gave to them. I liked the whole pack. I went to some lectures where the tutors themselves didn't really encourage you. They said, "Go to the lecture for entertainment," and so I did a good deal of that with very famous people like G.D.H. Cole and others

who were at Oxford just to get the experience of hearing these great figures. But my whole thing was tutorial writing and so on and so forth.

Socially I'd have to describe myself as somewhat alienated. The English were all, about that time, four years younger than I. Eighteen years old, and as I've observed, slow maturing, so I think it was more than four. And I had been through college already. I just felt a lot of things they did were really silly and so I didn't get into that kind of boyish culture.

04-00:27:12

Rubens:

A lot of drinking and—

04-00:27:14

Smelser:

Drinking, game playing, kidding, a little bit of innocent violence, a certain little bit of property destruction. I just said, "This is not my bag," so I didn't—I developed individual relations with quite a few people and I had an advantage. Magdalen College was highly stratified. At that time it was half public school and half not public school. Quite a large number of foreigners. And you could walk into the dining room and you could tell where anybody was sitting, because it was cliqued into groups by social class. You had the lords sitting up close to the high table and then you had different groups and down at the one hand you had the colonials. It was all very predictable. Magdalen was midway between being a Winchester fed school into a more general school and there were quite a group of grammar school boys, which were the middle range public—state schools. And I got along with all of them. Being an American—these guys would assume I was from Boston or something. It was all of this—kind of their own view. When they heard I'm from Arizona I got a lot of ribbing about the six guns and all that kind of thing. It was all in good humor. But I'd sit anywhere, whereas the other students wouldn't. They all sat where they were designated. So the social side of it, I didn't care for too much, even though it didn't constitute a blockage for me in any way. And I was on the rowing team. I had a solidarity with the guys on the rowing team, most of whom were younger than I, but nonetheless, in this collective enterprise you really began to develop good relations with them.

04-00:28:56

McIntosh:

I was going to say that with athletic prowess usually comes acceptance, right, at least among the other athletes.

04-00:29:03

Smelser:

Yes, yes. There wasn't a competitive relation. See, I kept going up the ranks. I stroked the boat in the final spring races and I'd come from this little guy they put in a tub on the edge of the river. And I had nice relations with these people. I didn't have a single negative relationship in that rowing group.

At the end of the first year, I underwent a change. I said, "I want to get a first class degree here." About five percent of the class gets firsts. So this burst of

academic ambition came back up on me and I decided that I wasn't going to row the second year because I couldn't make it. I just couldn't spend that kind of time. So I told the boat club captain that I was not going to row the second year. Talk about the freeze out. He just turned his back on me and walked away. I didn't care about it. I'd made up my mind. I knew I wasn't going to row. I didn't like being rejected but I didn't take it seriously. I knew what I wanted to do. The coach of the rowing team was quoted to me as saying, "Well, Smelser's okay. A good oarsman but he didn't go to the right school." That was his comment on it. I didn't have the culture, right. I didn't have the culture and the commitment. So that was their little world and I decided not to stay in that world my second year, so once again I dedicated myself really hard to my studies.

04-00:30:41

McIntosh:

And it almost sounds, sort of paradoxically, that being an outsider gave you a freedom there that some of the people who were really entrenched in that culture might have not felt.

04-00:30:52

Smelser:

Oh, there was a lot of that. There was a kind of rebellion in Magdalen College, which was my college.

04-00:31:00

Rubens:

Did you choose your college?

04-00:31:02

Smelser:

Yes, yes. I had to. Within five days after I got my Rhodes Scholarship I had to choose my college. I didn't even know five colleges. I had to list five colleges. I didn't even know five colleges. So I called up Clyde Kluckhohn, who'd been a student at Oxford and I called up a couple of other people. Called up Henry Steele Commager, I'd been at Salzburg with him, and began to get advice from all these people. So I listed finally five colleges. Magdalen was one. The admissions committee, one of my tutors told me, didn't like the fact that I had studied these girls dormitories either. They thought, "What is this? What is this academic subject? He's fooling around." There was a debate about me as to whether or not Magdalen wanted me and they finally said okay and then I did okay in that atmosphere. But I began studying very hard at that time with the determination to get a first class degree.

04-00:31:54

McIntosh:

And from day one you were within the philosophy, politics and economics department?

04-00:32:01

Smelser:

You had to do that the whole time. Six of the eight studies, areas in which you read—they called it reading—were mandated. And I only had two voluntary choices and I took economics, statistics and formal logic as my two options. But otherwise they were all fixed for you.

04-00:32:29

Rubens:

I don't know if you want to take this up now. But it's during your second year, isn't it, that Parsons contacted you?

04-00:32:38

Smelser:

Yes, that was my link with Parsons.

04-00:32:40

Rubens:

It's a big story. Why don't we pick that story up next week.

04-00:32:44

McIntosh:

Yes, there's plenty more to talk about. But it does seem like we're on the cusp of getting away from just the culture of it and into the academics of it. So maybe it is a natural time to stop.

04-00:32:58

Smelser:

Okay, fine with me. That's good.

04-00:33:04

Rubens:

I have one just follow-up question though. Why did you pick the girls dorms as the subject for your honors thesis?

04-00:33:15

Smelser:

During my Harvard years, in my sophomore year, I was taking this course from Henry Murray. He got twenty students to volunteer, twenty of the top students in the class to volunteer to undergo a whole series of tests at the psychology clinic. That's my guinea pig story. I was one of twenty who spent the next two and a half years being given every conceivable psychological test you can imagine, one after another after another, combined with interviews and combined with research—and they paid me. So it was a little ancillary source of income. Henry Murray was the guy and Gardner Lindzey, a psychologist who was an assistant professor there adopted me. Gardner became my clear friend. Became socially close together.

04-00:34:09

McIntosh:

Was he much older?

04-00:34:12

Smelser:

He was ten years older than I. He was a young man. But he took an interest in me and we developed a lot more than a formal academic relationship. He was the one who alerted me to a contact at this local college. And I had been studying sociometry, which was one of a little subclass of a group type of analysis and I was quite interested in it. The permission to study these was already granted. And Gardner Lindzey wanted me to work with him. Henry Murray wanted me to work with him but I thought I'd—Henry Murray was very dominant man. He liked to have subordinates. He liked people to do things his way and I smelled that and I didn't want to work with Murray. I later had a conflict relationship with him in graduate school because I didn't go into clinical psychology. Gardner Lindzey was very permissive, very intelligent, very happy to advise me in one thing or another and I liked him a

lot. He was permissive but at the same time helpful. And I had this permission over there so I—it kind of all fell together and I did a quite ambitious undergraduate honors essay based on it.

[extraneous conversation omitted]

Next time we'll talk about my thesis at Oxford, my relationship to Parsons, and then my return to Harvard.

04-00:36:28

McIntosh: Your return to Harvard and publishing there.

04-00:36:30

Rubens: And that's when you married? Before you returned to Harvard?

04-00:36:32

Smelser: Married right after I took the finals.

04-00:36:38

Rubens: All right. This is just an offhand. You talked about the class systems manifesting itself in the dining hall at Oxford. Was there anything like that at Harvard?

04-00:36:55

Smelser: Oh, yes. You had this final club culture. It was focused down in Elliot House, which was a sort of aristocrats' house down by the river. And I was a non-resident tutor at Elliot House when I was in the Society of Fellows, so I went down to eat and talk and socialize quite a bit. And then there were houses which were sort of—had a more proletarian tone, like Leverett House and Dunster House which were far away from the campus and so on. You got a sense of the ranking according to the type of people who chose what houses. And there was a clear kind of private school/public school divide there. Because I sort of was alienated from that kind of thing—and without aspiration. In other words, I didn't want to go to the club. I didn't particularly want to seek out these upper class people. I thought they were a little, sometimes, silly. So it didn't form a meaningful part of my environment except to kind of feed into this generally thread of alienation that expressed itself at Harvard.

04-00:38:14

Rubens: What about Social Relations?

04-00:38:14

Smelser: The Social Relations Department was not regarded as an elite department. It and political science probably had the lowest status in the social sciences at Harvard. And so the elite major was history and literature or certainly some of the sciences and so on. And there was this joking about the Social Relations courses being the gut courses, Mickey Mouse courses and so on. So that kind of hung around me. I sort of resented that because I didn't feel they were any

less demanding than anything else. But nonetheless, that was one element of the status system, too, that that department was not given high status. Still isn't.

04-00:38:50

McIntosh:

That's really interesting because the common intellectual history narrative about the social sciences is that after World War II Harvard becomes this epicenter of social scientific prestige. Where a lot of private funding's coming in, as well, and government funding, as well. So it's interesting that within the campus—

04-00:39:17

Smelser:

Yes. It was an anomaly, that that larger social support scene didn't spill over because of the traditional high status of the humanities. That's what Harvard was at one time, all humanities, right, and the relatively higher status of the physical sciences. Not so true here. Not true here.

04-00:39:38

McIntosh:

I don't want to make too much of this spillover. But since we're back at Harvard briefly, was there any intersection with MIT and did you have any exposure to the people there?

04-00:39:53

Smelser:

None. In my graduate years I did a little hobnobbing with the economists but not as an undergraduate. I was an usher for the football games at Harvard my first two years, meaning you went and showed people to their seats and got in free. And Harvard had twelve home games and their record was one and eleven. I went faithfully to the games.

04-00:40:22

Rubens:

Well there's so much to talk about. I'll be arbitrary and say let's end for today. We have a road map for next time.

04-00:40:24

Smelser:

Okay. All right, fine.

Interview #3 March 15, 2011

[Begin Audio File 5]

05-00:00:00

McIntosh: This is our third interview with Neil Smelser. Neil, when we left off last time we were at the beginning of your second year at Oxford. But what I wanted to just go back over was the political climate that you encountered when you were over there. Obviously we're in the early 1950s, a kind of heated time geopolitically, and I wanted to know if you were exposed at all to any of the tensions brought on by the Cold War.

05-00:00:39

Smelser: Well, 1951 was when the British Labor government was voted out of office, so it was the Churchill term during the time I was there, between 1952 and '54. I think that wasn't a backlash against Labor, necessarily, because they accepted almost everything that Labor did. There were some rollbacks later on. But the thing that was most salient in the atmosphere at that time was something going on back in the United States, and that was McCarthyism. Joe McCarthy, between 1952 and '54 was riding high. Those were my years at Oxford. He did not have his downfall until 1955, just after I returned to Harvard and hearings were scheduled. And, of course, the British students, they were not, I wouldn't say, especially sophisticated politically. But everybody knew about McCarthyism and there was a kind of almost taunting theme in some of my interactions with British students. And I talked about this with my tutors, as well, although they were not punitive or anything. But the idea was that America might be headed in a fascist direction. That was a thread of speculation and fear on the part of people in the middle and to the left. And interestingly, somehow or other, the fact that you were American, at least in the minds or in the talking of these students, meant that you were somehow or other responsible for McCarthyism. I was as shocked and dismayed at McCarthyism as anybody but it didn't help to say so because you were somehow or other American. And also, there was a little bit about the race relations in the United States, too. How can you tolerate this kind of thing going on in your country? Is it a free country or not and so on. So I wouldn't say that this was the most salient thing in my life but it was a thread that occurred in my social life and I got very impatient about it because I felt that it was a—felt you were being wronged to assume these things about you that you didn't share.

05-00:02:40

McIntosh: Well, since it's only a thread, I don't want to harp on it too much. But I do want to ask a couple of follow-up questions there. Because you're in a unique position that most Americans aren't in during this time of being abroad, did you feel that being taken to task for your Americanism caused you to compensate in any ways?

05-00:03:05

Smelser:

No. I just let this roll off my back in the end, even though I was somewhat hurt by it and didn't like it. I was so accepted at Oxford, just as a human being and as a student, my general life was pleasant, if not completely engaging. I didn't have any real alienation from the Oxford atmosphere at all.

05-00:03:34

McIntosh:

So you didn't feel any need to prove your distance from McCarthy or your nation as a whole?

05-00:03:41

Smelser:

No, no. I don't think I adapted. I just felt I knew where I stood on that pretty well and I didn't have to convince myself that I was vulnerable or invulnerable to these criticisms. It's just a needle in your life.

05-00:03:53

McIntosh:

Okay, so then why don't we get on into your life at Oxford in your second year?

05-00:04:00

Smelser:

Okay. As I indicated, I decided to stop rowing at this time because I was just interested—taking this three hours a day and being exhausted afterwards just was not a way to dedicate your time to studies. So I self-consciously simply turned to the idea that I was going to do my best on the final exams on which your entire results were based. And my tutors were very sympathetic. That was one of the interesting things about the British system, is that they were cheerleaders, as well as teachers, because they were not the ones who were going to examine you. So it was an interesting solidarity that one developed with the tutors. And they were very encouraging and they had picked me out as a person who was a likely candidate for a first class degree, so they were very encouraged by my decision.

My scout, my servant, college servant—we had a guy who made your bed and shined your shoes every morning. I was terribly embarrassed to have this kind old guy waiting on me. But he got interested in this issue. He thought I was making the wrong decision. He thought I was headed up for high ranks in the rowing world and he couldn't quite understand why I'd left. One day he put his hand on my shoulder and said, "Sir, a blue is better than a first." And I guess in England it might be. For certain lines of business, for example, it might be, but I thought that was a very humorous thing.

But anyway, I decided to stay and I studied very, very hard, including vacations. Very hard to prepare myself for these examinations. And with a few distractions as possible. My wife to be was living in London and I would go down there on weekends or she would come to Oxford on weekends. And I don't describe that as a distraction but it was a main other social side of my life at the time. There was an outfit called the Oscar Wilde Club, which was kind of a social club that I—I don't know that I was a member but I got

invited to a lot of the gatherings. It was mostly just parties, sherry parties and everything else. But I'd say my social life was not empty but I had made this deliberate effort to study as hard as I could.

The exams. They are six hours a day, four days. Twenty-four hours of exams in a row, in a row, four days running, and you just—I decided I wasn't going to study the last few days just because I thought my mind would be cluttered. It was an ordeal. You knew that everything hung on it. And furthermore, you didn't get the results right away. As a matter of fact, they gave an oral exam one month later. They're called *viva voce*. And the results depended on the exam results plus the viva results. And they didn't even tell you after the viva what you got. You found out by reading it in the *London Times*. It was an old tradition. It's like West Point posting up the ranks in class. Well, the *London Times* publishes the degrees at Oxford and Cambridge at a given day and that's how I found out, was that way.

Well, it was a terrible ordeal. What happened is on the written exams, I apparently was on the line between a first and a second. This is my speculation, because they gave me a serious viva. If you're absolutely in for a given degree, they don't bother. They just say congratulations or so and so. They gave me a viva on this day about, say, 10:00 in the morning and I spoke with them, nine, nine examiners all in robes, and I was the one boy in the spotlight there. After the ten o'clock interview, they said, "Will you come back at 11:30 for more conversation?" So I did. We had another conversation. "Would you come back at three o'clock?" Third. I knew this was a signal that I was on the ropes one way or the other. So I got back to the three o'clock and near the end, they asked me a question. It was in Oxford philosophy, in which there was a lot of analysis of the ordinary language. So they asked me to parse, to analyze, the difference between the following two sentences: I promise you and I love you. So I underwent what I considered a pretty good exposition of what exactly was involved in terms of your relationship to whom you uttered this. It was the Oxford way, and I'd been trained in this and I'd gone to all the lectures and tutorials. And so I'd been in there about twenty minutes at that time. Then they asked me a final question. They said, "What is the difference in the causal theory between Aristotle and Hume?" And I just froze because I know Hume down pat but I was very shaky on Aristotle. But these guys then were passing notes to each other at the same time they'd asked me this question and I began to talk and then they said, before I even got into it, "The exam is over." And I was excused and I didn't know what in the hell that meant.

But I was running around town in the meantime, closing my bank account, paying bills, everything that you do in the last day before you leave town. One of the examiners happened to be walking down High Street in the same direction I was after the exam and he said to me, "The last part of the examination was more interesting than the first," and that was his way of telling me that I'd got the degree. I wasn't sure about that and I didn't learn

until three weeks later when they published it in the *Times* that I in fact had got a first. But it was this very borderline—it made a difference later because when I was nominated for the second time to the Society of Fellows—I didn't make it the first time. I'll tell you a little bit about that, too.

The Society of Fellows, very Anglophilic. It's this Cambridge sort of—copied after—they have sherry and they have port, all this stuff, all this style at their dinners. They're oriented to England and the fact that I'd got a first at Oxford was a very big symbol for the senior fellows who interviewed me. If I'd gotten a second, I have a feeling I wouldn't have gotten into the Society of Fellows with a second class degree. We don't take second class people would be the kind of philosophy. So these things kind of pile on each other. It was just the luck of the draw that I didn't have to fall on my face on Aristotle on causation the last interview.

05-00:11:51

McIntosh:

That does sound like a huge ordeal, but like it did ultimately have important consequences for you later on.

05-00:11:58

Smelser:

Well, it had a happy ending. That was the important thing. And, of course, the fact that I studied philosophy, politics and economics proved to be very, very important resources for my entire further career.

05-00:12:09

McIntosh:

Well, this might be a slightly dry exercise but I do want to just get some specifics down about your second year at Oxford, in particular. Do you remember names of the tutors that you were working with?

05-00:12:23

Smelser:

Yes. Harry Weldon was the senior philosophy tutor. Kenneth Tite was my politics tutor. GDH Worswick was my economics tutor. And then I had Oscar Wood and a few others who tutored me a little bit in special fields. But yes, I remember them and I was close to all of them.

05-00:12:49

McIntosh:

And so you were reading in philosophy, politics and economics.

05-00:12:52

Smelser:

Yes.

05-00:12:53

McIntosh:

Did you have any freedom in designing your own—

05-00:12:57

Smelser:

Yes. Six of the eight papers that I read were required. English social history, political history nineteenth century, comparative politics. Those are required in politics. Two in economics: economic theory, economic institutions. Two in philosophy: modern and premodern philosophy. Then I could choose the last two. I decided for my own—for two reasons. I chose economic statistics

as one of them. It was tough but I'd done a whole year of statistics at Harvard, so I considered this to be a bit of a breeze. It was a tactical decision on my part. I wasn't in love with statistics but I pretty well knew almost everything that I was going to learn there, so it was in a way—a strategic choice on my part. Then I decided, again in the interest of rigor, not ease, to take formal logic, which was sort of mathematical, as well. Well, one of them was easy because I'd had it but the other was very hard. My formal logic was very hard. So that was the pattern of courses that I read there.

05-00:14:09

McIntosh: And do you remember from this time any text being particularly influential for you?

05-00:14:17

Smelser: Well, I don't believe I could pick out specific books. I remember some of the readings and, interestingly, in the middle of the time there after my first year, I was asked by my economics tutor to sit in the summer for a prize essay on—it was called the George Webb Medley Prize and it was for undergraduates. It was in economics. He said, "You take it. You would be a serious candidate for this." One of the papers was economics history. I had read no economic history in my Oxford studies and so I really had to spend a good deal of that summer boning up on texts in economic history because that was one of the exams. I didn't get that prize but I added to my exposure to economics.

05-00:15:11

McIntosh: And so this is a time when economic doctrines are being hotly debated, obviously. Were there any clear leanings at Oxford in terms of what you should be—

05-00:15:27

Smelser: Well, in retrospect, most of my tutors and most of the readings I took, were, I'd say, Keynesian in orientation because this was the post-war full employment society kind of period in England and the tutors and everybody else were exposed to this. I'd say Keynesian and neo-Keynesian writings were the dominant part of it, though there was some in growth in welfare economics. It was a variety of exposure but yes. In answer to your question, I would say Keynes was the main—

05-00:15:56

McIntosh: Okay. And that will clearly have implications for your work on *Economy and Society* with Parsons.

05-00:16:02

Smelser: Yes, it really did.

05-00:16:03

McIntosh: But before we get there, just one last little detail. Do you recall the names of the people who were on your orals exam?

05-00:16:12

Smelser:

Not all of them. None of them were my tutors. That's a rule. This philosopher Peter Strawson was on it. He was the guy who asked me about I love you and I promise. I remember that vividly. But I don't remember the others. They were all strangers from other colleges than Magdalen. So it was a campus wide, you might say, or university wide committee that administered these orals. So I don't remember the individual members.

05-00:16:40

McIntosh:

And you mentioned the ordinary language leanings of Oxford. In your readings in philosophy, was there any partiality towards that style?

05-00:16:54

Smelser:

Oh, yes. That was what Oxford philosophy was known about. It was called ordinary language philosophy, analytic philosophy. That had been the child of Wittgenstein and Bertrand Russell and through that—and I went to the lectures. I did not have them as tutors but I went to the lectures and discussion groups of the two leading ordinary language philosophers there, Gilbert Ryle and John Austin. I was inspired by these two guys because they're so brilliant. And I got to be fairly fluent in this ordinary language analysis but I didn't believe in it. It just did not take on me as being a real way to figure things out. I thought these people were social scientists in disguise and they weren't admitting it, that they were just talking about language, language as reality and it was a certain type of language. It was Oxford language. It wasn't all language. This is one of those subjects in which I maintained a skeptical distance but also profited from because I mastered it. They were tremendously enthusiastic about this movement in philosophy. They thought they were rewriting philosophy. My politics tutor, who was also impatient with ordinary language people, said, "You have sixty philosophers here at Oxford all sitting out on limbs and sawing off the limbs." They're going to fall off. He thought it was a kind of pointless game in a way. I didn't go that far because I thought there was a lot to be learned from it. But I was not taken by it. It did not consume me.

05-00:18:32

McIntosh:

That's interesting because back at Harvard a very similar shift is going on, as well. Did you encounter ordinary language philosophy when you were back at Harvard and was it another thing that you could use to advance?

05-00:18:53

Smelser:

Well, it certainly sharpens your mind. It is a form of analysis. You analyze the meanings and what's going on interactively. It's almost kind of a social science. It's the scientific analysis of language form of linguistics. I did not take philosophy when I was in graduate school. I took a couple of courses in undergraduate and these were more classic philosophy, history of philosophy and ethics, so I didn't get exposed there. However, a couple of my colleagues in the Society of Fellows were philosophers and so I got to know them and so I kind of continued in talking with them. But I didn't keep involved. It proved

to be an intellectual asset to me, I think, in my writing. Added a lot of clarity to my thinking and writing and capacities in ways that I have trouble putting into words. But I clearly felt I learned a lot from this kind of exercise.

05-00:19:54

McIntosh: It also seems that maybe even intuitively while you're at Oxford you kind of felt as though this discipline might be a bit of a dead end, in a way.

05-00:20:08

Smelser: I felt that. I felt that. I wouldn't call it a fad but I would call it a kind of a development that in fact has—analytic philosophy still dominates the field but that particular variety has sort of cooled down and doesn't have that kind of world-saving imagery that it did then.

05-00:20:31

McIntosh: Right. Okay. Well, during this second year of yours I believe is when you begin corresponding with Talcott Parsons. Is that correct?

05-00:20:40

Smelser: Yes, yes.

05-00:20:40

McIntosh: So when did that begin?

05-00:20:42

Smelser: Well, I have to tell you that as an undergraduate I took two of Parsons' courses, one called Institutional Analysis and the second one I believe I audited but didn't take called Sociology of Religion. I was very inspired by this man and saw him as a major person in the department even then. He knew about me. He recommended me to the Salzburg seminar but kind of on the word of other people because he didn't know me very well—personally. But our acquaintance was such that when I got the Rhodes Scholarship I actually made an appointment, went to talk to Parsons, because he had studied at Heidelberg and he'd been at the London School of Economics. I kind of had a general conversation with him about European intellectual life and people that might be there. So I was a little renewed in his life and he knew I was going to be at Oxford.

It so happened in that year, 1953-54, he was appointed the Marshall lecturer at Cambridge. He spent the whole year there. And one of the requirements of his appointment was to give the Marshall lectures, which I think were four or five lectures, and they had to be in economics. Now, he had been a faculty member in economics at Harvard in the first part of his career and he'd written on what we now call institutional economics, though he wasn't an institutional economist. He wrote on Weber, he wrote on Marx, he wrote on the classics in economics.

So he knew I was at Oxford and he gave these lectures, which were badly received by the economists at Cambridge. They thought he was a kind of

interloper and they couldn't understand him. He was a very difficult man to understand. So the whole thing was kind of a failure, I'd have to say, for him. But in any event, he mailed me the lectures that he had given and asked me to comment on them. Now, this was sort of a blow to me, because this invitation creates a difficult relationship with this very senior man, double my age, nearly the age of my father, who was regarded at that time as sort of the king of sociology in the United States. Certainly of sociological theory. So I was a little scared by receiving these papers and being asked to comment on them but I did and I immediately noticed that from the standpoint of economics they were out of date. That he had only read Keynes for these lectures and he had not acquainted himself at all with developments in economics in the twenty some odd years since Keynes wrote his general theory.

So it also so happened that my college roommate, a man named William Moffat, was living in Oxford at the time. I was very close to him. And he had been a major in economics at Harvard. So I gave him the papers. Said, "I'd like you to read these, too." And he read them and he came up with several suggestions, including a very serious reinterpretation of Keynes that I had been on the edge of making but I hadn't put it in exactly the right kind of words. But Moffat put it down just right.

So I wrote Parsons saying I'd had a conversation with Moffat and we'd kind of come up with the following reformulation. Basically I told Parsons he had Keynes wrong. And it was a fairly brave move to say that. And then I looked at a lot of other things in those lectures that were out of date that he just missed, that a lot of things have been going on with more contemporary theory that he didn't get. So I said, "Well, here's a place where you might develop that and you might develop that because it's incomplete," and so on. A long letter. Several single spaced pages. Sent it to him.

He came to Oxford to give a lecture at that time and he said, "We have to talk more." So we had a long meeting at Oxford when he came to give a lecture to the anthropologists, and he said, "Well, you've got to come to Cambridge for a weekend. I'd like to talk more." So I went and spent a whole weekend at Cambridge, talking with him about this theory that he was working with. I didn't know what was happening. I was just being asked to do this. Then he said, "Well, you know, I'm teaching at Salzburg," where I had been at the Salzburg seminar, "in the summer for a couple of weeks. I have some money on a grant. Could you come to Salzburg for a week and we could continue these conversations?" So I did. I said, "Okay." I went to Salzburg. Very intense intellectual reformulations began to develop. It was really very, very exciting. A lot of things were bubbling around.

In the meantime, I had been turned down for the Society of Fellows but had been accepted into the graduate school at Harvard, so I was going to return to Harvard. But nothing was planned about further work. But it was quite clear that we were in—a lot of intellectual fire was being lit in our conversations.

And I also felt confident, and was holding my own, and even was able to elaborate and push forward. And Parsons knew that. He wasn't a guy who would say something and then make believe it was completely true. He was a fairly flexible thinker, even though he had his own areas of intolerance. It was a back and forth. I'll say something about his character a little later on.

But when I went back to Harvard, within two weeks after going back to Harvard, he asks me to be a co-author of the book that was coming out of the Marshall lectures, which of course blew me away totally. Here I was just entering graduate school. I was twenty-five years old and here he invited me to do this in my first year of graduate school. That was unheard of. I obviously recognized the importance of this invitation at the time for my own career and was sort of flabbergasted. This kind of says something about my relationship with Parsons. I remember actually thanking him on the spot for inviting me to be the co-author. Now, he was a distant man and he couldn't handle emotions very well. And so he didn't respond to this outpouring of thanks that I gave him for choosing me. But he fell into this instrumental stance. "Well, I think maybe you can work on—the division will—" he began talking nuts and bolts about it without recognizing—without taking into account—I knew he had to have a high opinion of me and was respectful. He wasn't personally embarrassed. I didn't blurt it out. I just thanked him. But he couldn't quite handle the emotional side of that exchange.

05-00:28:15
McIntosh:

And so your exchanges before getting back to Harvard with him at these various meetings across Europe, they were—did you feel supported and encouraged by Parsons?

05-00:28:30
Smelser:

Oh, yes, yes. At that time I had applied to Harvard and to Columbia for graduate school. These were two of the leading places at the time. I'd gotten admitted to both of them. Robert Merton at Columbia had written me a handwritten note pleading with me to come to Columbia. Parsons was working on me very hard not to go to Columbia, to come back to Harvard, though he didn't offer that co-authorship as bait. That wasn't part of the picture. He just wanted me to come back. And he wanted to talk to me about what kind of research I wanted to do. So he was very supportive of me and I didn't get the—I'm not sure why I didn't get the feeling that I was this little unequal person feeding little bits of insight into this thing. But I had a feeling that he was fully accepting and I didn't have this excess baggage of wondering what he was thinking about me or was I doing well or wasn't I doing well. It had a certain ease to it which I was extremely grateful to him for because he was not very judgmental and he was always supportive.

05-00:29:38
McIntosh:

An extremely rare relationship, it sounds like, for a grad student and a—

05-00:29:44

Smelser:

Yes, yes. I think it was absolutely unusual for this to be—Mike Heyman once said to me, twenty-five years later in an off-handed comment when he was chancellor at Berkeley, and he and I had a lot to do with each other during that time. He said, “Have you ever been intimidated in your life?” meaning that you’re not an intimidatable person. I kind of was very surprised at that comment. But I think my feeling of lack of intimidation was more Parsons doing than mine, but I didn’t and so it was an easy relationship and a very unusual relationship with that kind of status difference and age difference.

05-00:30:29

McIntosh:

But it sounds that you did have a lot to offer intellectually that he didn’t have at that time.

05-00:30:35

Smelser:

Well, I offered in particular several reformulations of areas in which he was having headaches and they turned out to be very clarifying. I’ll just say that in all modesty. And Parsons knew it. They were not only clarifying but they carried us further. They suggested next steps. And so that was what the excitement was. An idea would come up and then we would carry it to further applications. Theoretical applications, often very abstract. I happened to know Parsons work, so that helped me. I didn’t have to be struggling. He’s unclear in many respects but I read a lot of his stuff and I was already toward mastering his own particular theoretical style and content. So I was well prepared for it. But nonetheless, this was the kind of magic. There was a domino effect. And I made a number of the suggestions. I began to lead toward a lot more formulations. This man was so in love with ideas. He would get tremendously excited, almost like a little boy when we began talking about some new intellectual development that he hadn’t thought of. So there was a back and forth, no question.

05-00:31:44

McIntosh:

And so just to be clear for the record, you had read the *Structure of Social Action*?

05-00:31:48

Smelser:

Oh, yes. I read that. I’d read his essays. And in 1951, when I was a junior in Harvard, two major books came out, one called *Toward a General Theory of Action*, edited with him and Shils and his book on the social system, which is one of the most turgid books ever written by anybody in the world. But I was so stilled enough in that culture I went out and bought both those books when they were published immediately. And I didn’t have too much money but I said, “I’m going to go get those books.” So I was that interested in the man that I had steeped myself in his writings, including the most recent ones.

05-00:32:24

McIntosh:

It is interesting to get this window into Parsons behavior because, like you said, he is a bit of a cumbersome writer. But I get this picture of a very excitable, energetic, kind of lively person.

05-00:32:41

Smelser:

Well, it's very, very interesting. I once made the observation that Parsons would get very excited in talking about the weather and in talking about the social system and very little in between. It was a very funny combination. He was a very shy man. Every time you saw him, it was almost like you were approaching a stranger. It took a while to unthaw, to warm up. He sort of wouldn't look at you too much. And he was a little tiny man. Not imposing at all. People described him as being a postal clerk or working behind a pharmacy counter, that kind of ordinary look that he had about him. But here we were in the world of abstract thinking, where he really did—his libido was really loaded into that. When I would talk to him about kind of ordinary things of life, it was kind of difficult. The man was shy and he was unemotional unless he'd get excited about some high theological principle in the social sciences. It was a funny combination.

05-00:33:49

McIntosh:

Well, if you don't mind, let's try to reconstruct some of the ideas that you all are exchanging during these conversations before you start writing the book officially. So you are well aware of his theory of action that he's developing at this time. You've read the social system, as well, and so you understand the architecture that he's essentially trying to erect. What is it that he is coming to you for? You mentioned the development of Keynesian economic theory.

05-00:34:20

Smelser:

Okay. Here's Parsons' animus in this book. He had recently undergone a major theoretical reorientation which he called social system theory in which he postulated that every social system, whether it be a small group trying to solve a problem, whether it be a corporation or whether it be a whole society, these are systems of social action, all of which have to meet certain exigencies. They have to have defined goals. They have to have resources that are adaptive. They have to pay attention to integration and they have to pay attention to the fundamental values by which they're made legitimate. He said, "This is a universal feature of all social systems," and his basic theoretical message to the economist was the economy is a social system and it has all these exigencies as well, and furthermore, it's only one of several social systems in the society and it maintains systematic exchanges with these other systems—the political system, the integrative system, the value system of society. Economics is embedded in this. It's not a world of its own and it carries on these exchanges via markets, usually. Consumer market, labor market, capital market, innovation and so on. So he was talking about economic theory being a special case of social theory, his social theory.

Now that, of course, enraged the economists because nobody wants to be a special case of anybody else. So that was one of the reasons. But this opened up an interpretation of what the markets are in an economy, to what are they directed, how are they controlled, what kind of exchanges go on, and it was in these areas where I was able to move in and kind of carry on this dialogue between the specifics of economic theory on the one side and the generalities

of social system theory on the other. I knew them both. And so I was intellectually quite at home here and I engaged in several reformulations of what Parsons had written about the markets. Some of them were corrections of his interpretation of Keynes. Some of them were original ideas that I brought that he hadn't incorporated. So this was a dialogue at a high theoretical level, a continuity between different theoretical traditions, analyzing the economy in a new way, analyzing the economy in a way that you also analyzed politics and social solidarity in a society, as well as the economy. So I believe I'm responding to your question. This is the intellectual territory in which we were exploring.

05-00:37:17

McIntosh:

And that's interesting because a lot of intellectual historians like to talk about the imperialism of rational choice, that economics tries to take over other disciplines, but here you're talking about the exact other—

05-00:37:29

Smelser:

This was another kind of imperialism from above, you might say. It wasn't actually exporting, it was an incorporating mode. And that, of course, was one of the things that created tension with the economists. And further, it raised issues they weren't specially interested in. They were more interested in technical solutions, models and so on, and so here was an overarching kind of almost philosophical sort of approach that didn't really interest them. The book in the end got rather negative reviews from economists just because they thought we were wrongheaded, going in the wrong direction, or it didn't interest them.

05-00:38:11

McIntosh:

And so in reading that book and in looking at some of Parsons writings that are going on at that time, one of the key issues is reformulating a theory of value and what it is, how to rank choices and understand motivations and things like that.

05-00:38:30

Smelser:

Yes.

05-00:38:30

McIntosh:

And that seems to be a fundamental departure from the Keynesian model.

05-00:38:37

Smelser:

Oh, yes. Well, the book was filled with intelligible criticisms of rationality as the orienting motive in economic life. And Parsons, of course, had a long history of this, going all the way back to Weber. He had written essays on the limitations of economic rationality. In fact, he said, "Economic rationality is not a psychological system." He said, "It's a normative system. It's something we've institutionalized and that businessmen are behaving according to a normative system, not in their self interest necessarily, because we've institutionalized self-interest rather than it's being a feature of the human

character.” So he was already well at home and well known as a critic of economic theory and we carried it steps further in this book.

05-00:39:28

McIntosh:

But it also strikes me that in addition to this direction from above of trying to incorporate a lot of things under Parsons’ system, there’s a movement from below, as well, of taking psychological theory and an understanding of the human personality and trying to subsume that into your system, right?

05-00:39:54

Smelser:

Well, it’s interesting you say that because when I went back to Harvard in 1954, fall of 1954, I had a fellowship but it wasn’t what you call an enormously generous fellowship. So he hired me as a research assistant at the same time on a book that he and Bales were writing on family, personality and social interaction. So he was trying to apply this theory down to small group processes, to family structure, even to the development of personality. So it was a very grand system that Parsons was pushing forward there at that time. That didn’t enter very much into the *Economy and Society*, however, that personality side.

05-00:40:37

McIntosh:

Okay. And so when is it that you actually go back to Harvard?

05-00:40:46

Smelser:

I go back to Harvard. And I’ll say something about that first year. I went back to Harvard in September of 1954.

05-00:40:57

Rubens:

A married man.

05-00:40:58

Smelser:

Married man. I got married in the summer right after my exams at Oxford. Came back to Harvard at that time. Settled down. In the first year of the graduate school of social relations, you have to take four required courses. Sociology, social anthropology, social psychology, clinical. I had to take those. I also was writing this book with Parsons. I’m going full steam ahead with that book. We started right away. And then in the fall I got renominated to the Society of Fellows by Parsons and by Barrington Moore and they interviewed me and I was accepted. That, of course, changed everything. The Society of Fellows had a rule that you couldn’t meet graduate requirements when you’re a junior fellow. I was going to be a junior fellow beginning 1955, summer 1955, and I had that year from ’54 to ’55. So I made the rash decision I’m going to take my orals examination, as well, at the end of this year. They usually take them at the end of the second year. Said, “I’d like to take them at the end of the first year so I don’t have to go around.” I don’t want to have to do things in reverse when I get into the Society of Fellows. Write my thesis and then take my orals. That seemed to me very stupid. So I just made the decision to try for my orals in that first year. So I’d have to say that was a

really, really tough year, first year back at Harvard, because I was doing all these things and they all had to be done.

05-00:42:34

McIntosh: Do you remember many of the details about just your daily schedule there and—

05-00:42:38

Smelser: Work, work, work.

05-00:42:39

McIntosh: Work. Up early and work?

05-00:42:41

Smelser: Yes, all the time. That was probably the hardest working year I ever had. The schedule dictated it. I had to do it. It wasn't that I was trying to work leisurely on a book. But Parsons would draft these things, get them to me, I'd get them right back, and so on.

05-00:42:55

McIntosh: And, again, as a first year student already co-authoring a book with Parsons, did that cause any separation between you and your peers?

05-00:43:09

Smelser: Yes. I subsequently learned, it wasn't so much at the present time, but some of the people I was in graduate school with at that time confessed to me later that there was what would be a mixed emotion, envy was one, of the sense that I was Parsons' favorite. There was that sort of feeling among my peers. And he held biweekly seminars with about maybe seven of his graduate students and this was when I was working with him and he kept floating the ideas that we were talking about. And I would always sit next to him. So it was quite clear that symbolically I was in a special place and Parsons was, of course, the dominant intellectual force in the department at the time. Almost all the students sort of went to him, studied with him or studied in large part with him. So yes. It didn't ever come in the open. There were no fights or no accusations or anything. As a matter of fact, I joined a group of graduate students myself and we studied together for orals at the time. So I didn't have enemies at that time. I was getting on in graduate school the way everybody else was, even though there was this overtone that I was in a special place.

05-00:44:35

McIntosh: Now, this might sound like a sort of flippant question. You're Parsons favorite, but was Parsons your favorite?

05-00:44:44

Smelser: Yes. Among the faculty members, clearly. I was close to a man named Samuel Stouffer who was an empirical sociologist and one of the requirements for graduate training was that I had to do some empirical fieldwork or statistical analysis or something. So I chose Stouffer to work with and I went out to a small New England community. He was studying community conflict and this

community had recently voted to go dry and it had blown up. There was a great conflict going on about that issue. So I went out in the field as an anthropologist almost and studied and did my work with Stouffer. I was very close to Gardner Lindzey, the social psychologist who had supervised my undergraduate dissertation. I was still close to Henry Murray, the clinical psychologist, though he was mad at me for working with Parsons. He wanted me to come and work with him and he was open about it. He was somewhat insulting about it and it sort of ruined our relationship, ultimately, between me and Murray, with whom I'd been very, very close. I still saw Clyde Kluckhohn, the anthropologist —I invited him to the Society of Fellows and he took an interest in my work and so on. Barrington Moore I didn't see because I was scared of the man. He was an unpredictable kind of irascible character who supported me all the time. He really liked my work and he pushed me to go into the Society of Fellows but I just couldn't relate to his personality, so I just kept a distance from the man. Alex Inkeles, the sociologist, was also a person I was close to. My first wife had babysat for him when she was a senior and I'd even got to know him before I went abroad because I would always be going out there and hanging around. No, I had a range of people I was close to, but Parsons was the one.

05-00:46:49

McIntosh:

And what's interesting is that you and Parsons are clearly developing this relationship and obviously are having to immerse yourself in his style of thought in order to help with this book and be able to communicate with him. But you also have this education outside of the DSR, outside of Harvard, that you got at Oxford. Did you have other pursuits outside of the Parsonian framework that you felt a need to explore while you were pursuing your PhD?

05-00:47:24

Smelser:

In that intense first year, no. I was really subordinated to the routine of the graduate school and in particular this work with him. I'll say some more about the work itself in a minute. But no, I was really constrained. Then when I got into the Society of Fellows, and was taken out of the department. I didn't hang around the department at all except to work with him. And he was away one of the years I was working on a dissertation. I was away one of the years I was working on my dissertation. So even that relationship was attenuated after that intense first year of collaboration with him. He was chair of my dissertation committee but a lot of it was carried on at a distance.

And then in the Society of Fellows I got all these other interests of people who'd show up who were interested in economic history, they were interested in this. Everybody came to the Society of Fellows. It was kind of a prestigious place for people to come and all kinds of people were parading in all the time. I kind of developed my closest friends in the Society of Fellows. One was an economic historian, Henry Rosovsky and another one was a student of English, Jeffrey Bush. There were no other sociologists in the Society of Fellows. None. And so my life got very dispersed during those three years.

05-00:48:41

McIntosh:

And you mentioned in a previous interview the lack of good standing that the DSR had at Harvard and that sociology in general had at Harvard. Had that changed since your sojourn abroad now that we're in the mid-fifties?

05-00:48:59

Smelser:

Oh, you mean the place of Social Relations in Harvard?

05-00:49:01

McIntosh:

Yes. And since you're the only sociologist in the Society of Fellows—was it a sort of badge of honor to be a sociologist at that time or was Harvard still largely focused on humanities?

05-00:49:16

Smelser:

Well, there had been several notable social scientists in the Society of Fellows. George Homans had been a junior fellow. William Foote Whyte I believe was a junior fellow. There were not many. I'd say the Society of Fellows—there were a few physicists and chemists in it. But I'd say the dominant tone was humanism. Historians and language and English. Crane Brinton was the head of it. Willard Quine, a logician, was a member of it. Harry Levin, who was an English professor and hated anything other than English and thought social science was just sort of a fraud, he was in it and he was a heavy—so I felt a little marginalized but it didn't affect me—it wasn't a day by day thing. The behavior was generally civil. The junior fellows all thought they were very special and they all honored one another because they were all junior fellows. But the general tone, I was a little alienated first from the precious Anglophilic culture of the Society of Fellows and then I was a minority of one with respect to my own field.

05-00:50:29

McIntosh:

Okay. So pushing through this first year a little bit and getting into your oral exams. Do you remember who was on your committee there?

05-00:50:38

Smelser:

Yes. They kept the committee secret from the students at that time. It was a brutal sort of—and I didn't choose them. So Stouffer was on my orals committee, Parsons was on my orals committee, also Andrew Henry. I believe James Dusenberry. He was on my thesis committee. I'm not sure whether he was on my orals committee or not. I don't remember the rest actually. I remember the questions very vividly from Stouffer and Parsons but—

05-00:51:10

McIntosh:

If you don't mind enlightening us, what were those questions?

05-00:51:15

Smelser:

Stauffer asked me to comment on a line of social theory that was pretty passé at the time. William Fielding Ogburn's theory of social change and it was the sort of thing that a graduate student could easily stumble on because it wasn't very much in the culture. As it so turned out, I had recently read it. Accident, complete accident that I had read this. So I was able to really be a virtuoso on

this. That won me over with Stouffer. But they also asked me another interesting question. He said, “Suppose you were invited to a dinner of honor in Boston and the mayor was the host of the dinner and there was some unclarity as to whether or not the local archbishop or Ted Williams, the baseball player, would sit to the right of the mayor. How would you solve this one?” A very interesting question for an orals examination about—it was in my field of stratification. It was one of my special fields, social stratification. And this was in the area of social stratification. Well, I sort of bumbled and stumbled. There is no real answer to that question and I thought I didn’t do very well on it. Parsons asked me—I wouldn’t call them setup questions but he asked me serious questions about Durkheim and Weber on economic sociology. That was my other field, economic sociology and stratification were my two fields. And they were, of course, easy for me to answer. And Parsons, I think, knew that they were easy for me to answer. He wasn’t exactly feeding me questions but there were no tricks there.

05-00:53:01

McIntosh:

At that point, he seems to be such a proponent of yours that I would be surprised if he were really out to get you in the oral exams.

05-00:53:08

Smelser:

I don’t believe I did brilliantly on those orals examinations but I passed them and that was my aim at the time. I wasn’t trying to get some kind of special distinction. I went through them fine.

05-00:53:23

McIntosh:

And the specialties of economic sociology and stratification. How were you exploring those concepts?

05-00:53:29

Smelser:

Vast readings. By choosing those, you laid yourself open to being asked any questions about research in those areas.

05-00:53:38

McIntosh:

And so who were some of the leading figures for your studies of stratification?

05-00:53:45

Smelser:

Well, Lloyd Warner was a major figure in his study of community stratification, of communities at the time. Parsons himself had recently written a very big analytic essay on stratification. There was a lot of concern about if it was overlapping political sociology and stratification. It was associated with the writings of C. Wright Mills at the time. And then there were a lot of lesser lights—a lot of empirical work on comparative prestige systems and so on.

05-00:54:21

McIntosh:

So Mills was somebody who you were reading to prepare for—

05-00:54:25

Smelser:

Parsons had a total dislike of Mills. Parsons was notable for the fact that he never criticized people to their face or openly and he tended to avoid—he had the mode of letting criticism roll off his back and he didn't answer it very much. But there were just several figures about which he had a very negative animus. One was Karl Marx. Not alive but, nonetheless, he was very, very critical in his sense of limitations about Marx's theory. Thorstein Veblen, he had a negative animus to, partly because his early studies were pretty close to Veblen's. But Veblen he saw as an anti-theoretical figure. He just didn't have use for him. And Mills. He hated Mills because Mills was so vicious in his attack on everything Parsons was doing. Those were the three bugbears that Parsons—otherwise he was kind of—“Yeah, people have their views and they're not right.” Homans criticized him all the time but he stayed above it.

05-00:55:31

McIntosh:

If you don't mind, I wanted to follow-up on that a little bit and your discussion of Parsons' take on Marx. Did that shape your own personal views of Marx during the time? The subsuming economics under sociology is in a way a direct affront to Marxism.

05-00:55:48

Smelser:

Well, I never had the animus against Marx that Parsons had theoretically. I always felt myself a little more open than Parsons. And I didn't like the passion Parsons—this in principled opposition that Parsons had. That was not my style to just stick someone as an enemy, right? And I never fought with him about it. No sense starting feuds when you don't have to. But in my dissertation, I read everything Marx wrote about the working classes in *Capital* and other places. I took Marx seriously as one of several theoretical approaches to explaining why the workers were discontent and why they joined into protest movements in the nineteenth century and I took a kind of open view. Ended up being critical of Marx's explanations based on class consciousness, but nonetheless incorporated Marx respectfully but critically into my thesis. I don't think Parsons would ever have done that. He never said anything about it, never objected to it. Subsequently I—

05-00:57:07

Rubens:

You will teach Marx.

05-00:57:08

Smelser:

I taught Marx in my first course when I came here. I edited a book with the University of Chicago Press on Marx called *Marx on Society and Social Change* and wrote a serious intellectual introduction to that work. I was really much more involved in Marx and took Marx more seriously, you might say, than Parsons ever would. I never saw myself as a Marxist. I wasn't always orienting to Talcott, but nonetheless, I had a different kind of orientation.

[Begin Audio File 6]

06-00:00:00

McIntosh: One thing that we're yet to really flesh out in detail is the working climate with Parsons on a day to day basis.

06-00:00:11

Smelser: Well, as I said, he very early had a division of labor in mind and he knew that I knew certain areas better than he. So he asked me to write certain passages in the passages and sections of the book about which I— In other words, I wrote the—when we reinterpreted Keynes and Schumpeter, the economists, on their views of markets and economic functions, that fell to me to draft these. We wanted to have a chapter, a big chapter, on economic processes, which include business cycles and trade cycles. Parsons was completely unfamiliar with this, so I wrote that entire chapter in the book. Certain portions of the chapter on economic institutions I also drafted because I knew more about some economic institutions than he did. The last chapter was on economic growth and development and I wrote half of that chapter, approximately, on technical economic models of growth, which he was not in command of. So I guess I would say I drafted—the Marshall lectures were already there. They were already almost three—but they got completely reworked radically. They were very different from what they were as he delivered them in the end. So I guess I drafted about a third of the book.

Then, and here's something that I have never been able to explain to myself—Parsons is a very bad writer. Extremely abstract. Didn't pay any attention to his reader at all. He had no respect. I took the introductory chapter home one day and I rewrote it. Every sentence. I just went over it and rewrote it. Didn't make an effort to change the meaning but made an effort to make it readable prose. And I did it on my own. I didn't ask him. And then I presented him with it. Scared. I was very nervous. There's a junior collaborator stepping in, taking an aggressive act. It could be an aggressive—I was violating his prose at the worst, you see? And he sort of liked it. He thought it helped. So then, on my own, I went chapter after chapter, every page of that book, I personally rewrote. Redrafted. Sometimes radically. Shortening sentences. Trying to make it clear. What are we really trying to say here? How can we say it better than it's being said? I spent a lot of time doing that. And Parsons was extremely gracious, except for the last chapter, which I will confess to you I was more aggressive than in the other chapters. And I handed it to him, and here's an interesting interchange. He read it and he evident—I could tell just by the—I knew him well enough to know when he was reacting. He thought I'd maybe gone too far. But he didn't tell me directly. He came to me a week later and he said, "I asked my daughter Ann to read that chapter and she said, 'Well, I think he's maybe gone too far.'" Quoted his daughter rather than himself to me. But then he didn't want me to change it. He just registered his opinion but accepted everything I did.

So that book, from the standpoint of the prose, is really mine. That's not to say the ideas are mine. I didn't try to violate the ideas. I just wanted to make the exposition clearer, and I think I did. But the thing I can't account for is why I felt free to take that initiative, being in that role with Parsons. I guess I did have the sense that I had his confidence, or else I wouldn't have ever done it if I were fearful that he would say, "What are you doing?" and so on. So that was a very interesting side of that, the stylistic responsibility that I took. Marty Lipset asked Parsons, when they were going to hire me here, he said, "I realize he's been—" Lipset, who's never minced words— Lipset said, "I realize he's listed as co-author but is he really a co-author?"

06-00:04:59

Rubens:

Which way was he leaning?

06-00:05:00

Smelser:

He was suspicious. He thought that Parsons was generous with me, that this was just Parsons book, right, and Parsons really set him straight. He said, "No, he's a full co-author," even though I'm referred to as a junior author in the preface.

06-00:05:19

McIntosh:

And what was your schedule with Parsons like? Would you meet with him weekly? Would you meet with him—

06-00:05:27

Smelser:

Oh. I went to his house. They had a summer house in Vermont. We'd go up there, we'd work together. I'd meet with him weekly. I went to Martha's Vineyard one time. I wasn't with him then, but just before and just after I spent a couple of days with him. We, of course, would work solo when we were actually drafting but we were kind of in daily, or every other daily contact about this thing because it was all growing. It was a very big enterprise and taking a lot of both of our time and we were doing it together.

06-00:05:57

McIntosh:

So in close contact but it does seem like he gave you a lot of leeway, basically—

06-00:06:03

Smelser:

He did. We did send my chapter on social processes and growth to an economist, Bert Hoselitz, who was a friend of Parsons and later became a friend of mine. And Hoselitz raised some real criticisms and I reacted to them. And Parsons really wanted to be sure that I was satisfied that Hoselitz was not pointing to a fatal problem in the manuscript. So I would say that the relationship, if you wanted a total view of it, was extremely congenial. We didn't have any fights at all at that time. There was some tension later on. We'll get to that.

- 06-00:06:44
McIntosh: Right. It's interesting that Parsons, even in his daily life, seems to avoid conflict at all costs.
- 06-00:06:53
Smelser: [laughter] That was absolutely true with him.
- 06-00:06:55
McIntosh: So when was the manuscript completed?
- 06-00:07:00
Smelser: We completed the manuscript in the summer of 1955.
- 06-00:07:05
Rubens: After you'd taken your orals?
- 06-00:07:06
Smelser: After I'd taken my orals. I remember the last writing was done in June. I went to Martha's Vineyard, took the whole manuscript and did that final stuff with it. And he had a contractual relationship already with Routledge & Kegan Paul to publish it in England and he was also the regular publisher with The Free Press, so we had absolutely no problem about getting things published. And it came out one year later. The usual lag. I was already back in England working on my thesis when a copy of it arrived. I was in the British Museum when a copy arrived of it. It was summer of '56.
- 06-00:07:48
McIntosh: So you mentioned your thesis. Along the way, you're getting ready for orals, you're working with Parsons closely on this book. How did you find time to develop ideas about what your thesis might be and when did those ideas begin to percolate?
- 06-00:08:05
Smelser: Well, when Parsons was in Cambridge—and I went to visit him in the early stages of our getting to know each other on this book, Parsons was very much interested in where I wanted to take my research. I had just taken those George Webb Medley economic history exams. Several of my papers at Oxford were on British social and economic and political history. I didn't fall in love with economic and political history but I really built myself up as far as knowledge was concerned and so I got the idea I might want to do some work in the social history of Britain during the industrial revolution. I had also deliberated about somehow or other taking general system ideas and doing some work in international relations. It was a much less articulated project in my mind. And when Parsons asked me where I was thinking about going, I mentioned these two possibilities. He quite clearly said that the one on English industrial revolution really sounds to me exciting. He put a vote and he really influenced me to take that up. So I kind of knew before I left Oxford that my research was going to go in that direction.

I took my, what do they call it, thesis prospectus exam also in that first year. So I had to develop what I was going to do and that's where I met Rostow and James Dusenberry, because they were on my thesis committee. So I was kind of on my way in framing what I wanted to do even before I began my term at the Society of Fellows.

06-00:10:04

McIntosh:

What's interesting to me about that is you come from the DSR, which is sociology, anthropology, psychology, right, and you go to Oxford, which is politics, philosophy and economics, and then you add history—

06-00:10:17

Smelser:

That's right.

06-00:10:18

McIntosh:

—on top of this, so now there's another discipline that you are almost giving yourself to have to go master, in a way.

06-00:10:28

Smelser:

Yes. I had written a couple of historically oriented term papers as an undergraduate and, of course, I'd studied English history. But no, you're absolutely right that I stepped into history. This, as it turned out, turned out to be a very bold step. I didn't really realize it was a bold step. Because here I was, a foreigner and a sociologist, coming in and writing about English social and economic history. Fields which English historians thought they owned. No question. They fought a lot but they thought they owned it and what's this interloper going to be—coming in, bringing in a foreign type of interpretative system into this material that we plunge into and study empirically all the time. That was the kind of challenge that I had coming into this field that was owned by other scholars. As it turned out, my book was much more warmly received in England among the historians than it was among sociologists.

06-00:11:42

McIntosh:

And back at Harvard when you're shopping this idea for a thesis around, for a dissertation around, was it warmly received among the faculty there that you were talking to and who were you talking to about it other than Parsons?

06-00:11:58

Smelser:

Well, one of the people who came around to the Society of Fellows more than once was this economic historian, Alexander Gerschenkron. Very famous man in Russian and French continental history. And I happened to know his daughter when I was an undergraduate and got to know him just so ever slightly when I was an undergraduate. But he came out to the Society of Fellows, so I began broaching ideas to him. So he was not a member of my dissertation committee. The reason I chose Walt Rostow was that Rostow was an economic historian of Great Britain in the nineteenth century. Written a huge book on business cycles in the nineteenth century and was the empirical expert in the field and so I said I've got to have this man. This was before his controversial days in the Johnson Administration. He wasn't politically

labeled in connection with the Vietnamese War and other matters at that time. So he was basically a scholar of English history. I chose George Homans, arch enemy of Talcott Parsons, because he also had written in British history, on an earlier period, but nonetheless he was a sociologist who had written extensively in history. I chose an extra member of my committee, too, James Dusenberry, an economist. So I really had a pretty wide mix of people on my dissertation committee.

06-00:13:18

McIntosh: Parsons, Homans, Rostow, Dusenberry, correct?

06-00:13:31

Smelser: Yes.

06-00:13:33

McIntosh: And so why bring in an extra person? What did Dusenberry have to offer?

06-00:13:37

Smelser: He was an economist. He was a formal economist. I had taken a course with him as an undergraduate in economic sociology. He and Frank Sutton had designed a course in economic sociology and I had taken that from them. So I knew him and I wanted to have an economist on my committee. He seemed the right one. Interdisciplinary, not doctrinaire.

06-00:14:02

McIntosh: This is a historical enterprise in terms of research, clearly, and gathering data. But it seems like you have a model that you're exploring, as well. So what was that model that you had developed and how were you hoping to implement it?

06-00:14:19

Smelser: The model was one that Parsons and I developed in the last chapter of *Economy and Society* that he had been working on and with. It was called a model of structural differentiation. And basically what it does is to say that under certain circumstances of rapid historical change, certain modes and structures become outmoded, archaic, inefficient in the economic sense and begin to become the focus of dissatisfaction because they're really not working in some new setting. And so the mode of change over time is that these previously diffuse social arrangements—I had in mind, just to give you an example, the cottage worker system in Britain where the whole family worked in the home and they not only wove and spun but they also tended to certain crops. There was a multiple kind of functions and the cotton manufacturers would bring yarn to them and they'd weave it or they'd process it, spin it, weave it and return it. It was called the putting-out system. And it grew to be enormously inefficient when the cotton market began to expand.

Well, the process of differentiation is when these roles which were previously diffused in a whole wide range of activities then begin to get more specialized and for that reason become more efficient. It was in that context of increasing

specialization that I interpreted the rise of the cotton mills in the countryside. Here was the adaptation on the part of the cotton manufacturers to make labor more efficient, but in the meantime they took the laborers out of their family, became wage labor. The differentiation is they differentiated between the family and work roles. And that was, of course, one of the main stories of the industrial revolution, to make labor more specialized. Wage labor. And you depended on the market rather than on this diffuse set of activities in the countryside. So this model of structural differentiation, I said, this applied to the British industrial revolution. So I basically rewrote the story of economic change in a seventy year period according to this model of increased specialization and then I rewrote the story of how a family became a more specialized institution, as well. Those were the two threads, two arms, of my analysis.

The other part of it that was really more original than the idea of differentiation is that differentiation proceeds according to a series of stages. That is, dissatisfaction. There's all kinds of symptoms of disturbance and upset and sometimes violence. Dissatisfaction becomes rife and then this dissatisfaction gets handled and channeled in different directions and inventive systems begin to be tried -new types of schooling that would take care of what the family didn't do and so on. So there was a process of differentiation as well as the fact of differentiation. And it was that model that I applied systematically to the rise of a new type of family, to the rise of trade unions, to the rise of consumer cooperatives, to the rise of saving banks. All new institutions in the industrial revolution, all of which addressed this increasingly archaic family structure that became more vulnerable during the industrial revolution. And these new forms I analyzed according to a common theoretical model. That was my thesis.

06-00:18:05

McIntosh:

Now, when you begin talking about differentiation in stages and Rostow is in the picture, a lot of connections can begin to be made, obviously. How much did Rostow's theory, as opposed to Rostow's empirical work as a historian—

06-00:18:26

Smelser:

Not at all. He had a different type of stage theory. He just was looking at take-off. I wasn't influenced by Rostow in that regard. I was influenced by his book on interpreting British history in the nineteenth century and trade cycles. And Rostow was not a daily figure in my life at that time. We were at different institutions. He was at MIT, I was at Harvard. I was abroad, he was away. He read the thesis, he loved it. That was it.

06-00:18:55

McIntosh:

Okay. And when you said that you were rewriting this period of British economic history, what was it? What were you revising? What were the standard histories that you were reacting against?

06-00:19:11
Smelser:

The most dramatic and the most controversial and the most lasting of the contributions of that thesis was that I reinterpreted the timing and the content of worker protest during the industrial revolution. It had previously —and in Marx and the British socialist historians— worker protest was sort of seen as a natural outgrowth of some type of exploitation. That they had been disadvantaged in the industrial revolution. That their wages had been depressed, that they were depersonalized in the marketplace and that this form of exploitation, even though it took different forms in Marx than it did in the British historians, was the idea that the industrial revolution was a brutal and somewhat savage assault on the working classes. What would you expect but a protest? I tried to make this more specific in saying there were a lot of other things going on.

And in particular, I was very much interested in the fact that people's family relations were being disrupted in the cotton industry by the changes that were going on. Previously these workers were permitted to hire their own children and they would hire two or three children or their wife and they would work around a machine or they would work on the factory floor as a family unit. Then you began to get technological changes in which each worker had to have twelve to fifteen assistants, rather than three of our, and they stopped hiring the kids and the wives. This was another form of differentiation. They took the wage earner out of the home and the family. Now, I interpreted this as a meaningful type of event in the lives of workers and I analyzed the symbolism of the protest, which emphasizes, among other things, community. And furthermore, this protest went on when wages were increasing and that was a puzzle to me, too, as to why the standard poverty or repression model didn't seem to be working because in the very worst exploitation of labor the workers were quiescent. So I said there must be something else going on here. So I used this angle of the differentiation of community and family out of the economic enterprise as a new way of interpreting not only the timing but also the symbolism of these movements. So I would say that that line of analysis, which I carried out repeatedly in connection with all these different institutional changes, was a thing that caught the attention. It caught the polemic attention of the left, socialist historians, because I was challenging the standard explanations of why workers protest and then it also had an original twist.

The first review that book got was in the *Economist* magazine in Britain and they were just terribly excited that somebody was writing something other than the stock stuff about British social history. And some of the historians liked the idea, too.

06-00:22:25
McIntosh:

That word symbolism stood out because I wanted to clarify. Did those protests have symbolism for you as a researcher now or were you ascribing a kind of

intention to the people who were protesting? That they understood that they were creating symbols?

06-00:22:41

Smelser:

I did not take a principled stand on that issue. I guess if you ask me to interpret what my operating approach was, I was saying that these workers were seeing their lives disrupted in ways in which they did not fully articulate what was going on. I didn't have a total rational model in that sense. I argued that they're experiencing various kinds of discomfort and that institutionally the system was creaking and irregularly changing. They wanted to preserve the child labor. This was another unorthodox interpretation I made. That far from objecting to the working of their children, they wanted them to be in the factory with them for a variety of reasons. One, increased family income, two, they kept control over them. That socializes kids on the factory floor. No, I didn't say the workers figured it all out and did it, but there was an assumption that there was a lot of discomfort going on and the content of the opposition reflected it. So I was making inferences.

06-00:23:53

McIntosh:

Okay. Now, the reason that I ask is that this dissertation is a really interesting combination of the macro, meso and micro.

06-00:24:02

Smelser:

Yes.

06-00:24:03

McIntosh:

And when you're talking about adaptation and strain, that's language from a kind of structural functionalist paradigm.

06-00:24:14

Smelser:

Yes, it was in that tradition. This was where I was.

06-00:24:17

McIntosh:

And so the reason I ask about symbolism is because one of the common critiques that's lobbed at Parsons' functionalism is a lack of agency that can be ascribed to the individual actors. I guess I wanted to just focus on whether or not you saw that as an issue, whether that was even on your radar screen of trying to address the idea of agency in the research that you were doing.

06-00:24:51

Smelser:

Not in those words. In retrospect, and I wrote this down in an essay some years later, I was responding to a great many criticisms of Parsons that were in the air at the time. Three in particular. One, Parsons was criticized for not being empirical, that he was just an airy theorist who worked with systems that are in the air and so on. I always, always felt myself to be much more empirically oriented than Parsons. This book was clearly a major heavy duty empirical historical research. No question about it. The hands were getting dirty. Secondly, Parsons was criticized for not being able to characterize change in society. It was a static theory. That systems equilibrated themselves

and came back into line and deviance was handled and somehow or other there was an going—systems were ongoing, right? Here was a deliberative effort on my part to take a seventy year period in which the world was revolutionized. If you'd had anything there, all you had was change, right? Then the third thing that Parsons was criticized for most of all was that he couldn't handle conflict. He just thought the world was kind of stable. Here was another situation. The whole second half of the book was the class conflict interpreted in the new way, right? Now, I didn't sit down at my desk and say, "This is when I'm going to modify Parsons," and a lot of this was a matter of, at best, a semi-conscious reflection on my part. But that book broke or addressed, you might say, these kinds of criticisms that my mentor was most noted for.

06-00:26:57

McIntosh:

Right. You mentioned this is a sort of semi-conscious departure. Was there any conscious understanding during the two years while you're in the Society of Fellows, correct, and while you're working on your dissertation of embarking on your own project here?

06-00:27:15

Smelser:

There had to be. There had to be. I didn't experience a lot of it directly until the end of my time at Harvard, and I'll tell you about it. Parsons was present during my first year in the Society of Fellows and I worked with him and I talked with him. So I mostly was in the libraries working, beavering away. Well, he's a busy man, too. We didn't meet all the time after we collaborated. So I was launching on my own. The second year I went to England. Buried myself in the British Museum. Went to Manchester. Did most of the writing of my thesis away from him. He came through London twice. We met, we talked, he read some things and that was it. Third year I was in Cambridge finishing my dissertation in the first semester, he was in Palo Alto at the center, so we were away from each other then, too. So I was on my own and I didn't have a daily—whenever I'd see him we would talk and he would read things. He didn't have too much input into what I was—he was pretty satisfied with what I was doing. So I was embarking on my own, de facto, and felt myself out from under him.

When, at the end of my period at the Society of Fellows, I was going off into my career, I had job offers from Berkeley, Michigan and Harvard—to stay at Harvard. Parsons was really, really pushing me to stay there. He still had the idea that I could join his enterprises and be in his seminars. And I was distinctly uncomfortable about that. That was one dimension. I had a lot of other reasons for wanting to leave and come to Berkeley, but one of the factors in my mind was a very easy way of moving out of that relationship with Parsons. I remember, after I had decided to come to Berkeley, we had him and his wife over to the apartment, and the occasion was to tell him. So midway in the conversation I told him that we were going to Berkeley. He was obviously crestfallen. He was emotional. He was let down. He didn't want

me to go. He wanted me to stay there. But I was fixed in my mind. I wanted to go. His wife tried to ease the situation. She said, "Well, you can fly from San Francisco to London now non-stop." So she was making a joke about that. "Well, it isn't so bad after all." But then Parsons, after I was at Berkeley, continued to want me to come to seminars back there, to come to discussion groups that he had. He had money to pay for it and he would invite me repeatedly and I would always refuse. Never broke the friendship. I just politely refused to get involved in this kind of continuing—which was a subordinated relationship that I had with him in this setting. So it's the way I broke finally from Parsons. So twenty years later it became complete and we'll get to that.

06-00:30:58
Rubens:

I wanted to ask you just about your—how much you entered into his family life. You mentioned that he had his daughter read the final chapter you wrote for him.

06-00:31:10
Smelser:

I knew his wife, Helen Parsons, who was a wonderful woman. They had us up to Vermont on three or four occasions. They had a country place and invited us to parties. Very fully involved in the social life. He included me in his social life, along with other faculty members. So I was a full citizen in the social life of the department there, above and beyond my relationship with the graduate students. With the faculty I was getting to know them all, and visitors and so on and so forth and others in the Society of Fellows. So I knew and liked his wife a great deal. She's a very down to earth type who didn't take much nonsense from him. She'd always ask him, "Are you pontificating again?" whenever he was talking away. And I knew his daughter, Ann, who was pretty close to my age. Tragically a suicide several years later. Knew his son Charles. Not as well but got to know Charles when he came to the center when I was director. He was a philosopher at Columbia and then at Harvard. And then he had a younger daughter by the name of Sue. She was quite a bit younger, but I knew her, saw her socially. So yes, I was in the family. And matter of fact, Ann sort of used me—his oldest daughter, she was a bit of a rebel and she sort of used me as an avenue to criticize her father. And a very tragic little moment when I had Sue over to the house one time and she was there with a friend. It happened to be David Halberstam who was an undergraduate at the time. And Halberstam was interviewing her about her father, and after a question or two, she said "Ask Neil. He knows him better than I do." Which really made me sad, that this interchange took place.

06-00:33:19
Rubens:

Now, you mentioned in a couple of your autobiographical pieces that you were introduced to, and were aware that the faculty at Harvard was quite taken with psychoanalysis.

06-00:33:31
Smelser:

Yes.

06-00:33:32

Rubens: And would you discuss that at all with Parsons?

06-00:33:36

Smelser: Yes. Matter of fact, among the faculty I was close to Inkeles, Lindzey, Robert Bales, who was a small group man. I became friendly with him, as well, Parsons and another chap by the name of Ron Taguri. Never kind of heard of him again. All of those were in the Boston psychoanalytic training program at the time.

06-00:34:07

Rubens: Just everyone knew this? It was understood?

06-00:34:10

Smelser: Oh, yes, it was public. The culture of the department was quite psychoanalytic. I knew Gordon Allport, as well. Had a friendship with him and he influenced me very much in my thinking about social movements and collective behavior. He wasn't as psychoanalytically oriented. As a matter of fact, he was somewhat hostile to the whole thing. Homans couldn't care less. But my group of buddies were really in the culture. And Kluckhohn had been psychoanalyzed and it was just kind of—cocktail talk was all the talk when I was—it was really the dominant social culture of the place at the time. A lot of the English critics were using psychoanalysis as their framework for criticism. Historians were influenced. I don't want to over-exaggerate it but there was a very strong thread of psychoanalytic culture and that certainly had a—I got into it and I sort of secretly pledged to myself that I might want to go into it myself.

06-00:35:22

McIntosh: I was curious what Homans reaction to the whole psychoanalytic thing would be because he's not as—

06-00:35:27

Smelser: Behavioralist.

06-00:35:27

McIntosh: Yeah. And exchange theory and onto rational choice, right?

06-00:35:30

Smelser: Oh, yes. He had no use for it at all. He just thought it was nonsense.

06-00:35:37

McIntosh: What strikes me is that there's an interesting tension between the goals of a lot of those people you mentioned who were a part of the Boston Psychoanalytic Institute, specifically Parsons, which is to create a universal theory, but getting away from the Homans model of rational choice and totalitarianism.

06-00:35:58

Smelser: Well, that's not what influenced Parsons. Parsons was drawn to Freud and he liked the idea of a personality as a system. And so this was fundamental. He

couldn't care about Homans view at all. He had as little respect for Homans as Homans had for him, really, though he didn't voice it that much. He thought Homans was misguided. So Homans didn't figure much in Parsons outlook except that he was backward.

06-00:36:25

McIntosh: And now the movement towards a grand theory. Was that something that was overtly discussed?

06-00:36:34

Smelser: Well, Parsons thought that the whole Social Relations Department was going in that direction. He thought he was the vanguard of this theory and the really high points of its growth were in the late forties and into around 1960. So I was right in the middle of this whole idea about Social Relations creating something. And Parsons assimilated this to his own. He assimilated the department and his own theoretical program to one another. And, of course, people like Stauffer sort of let him do it, though he didn't care about it. Parsons was just not an empirical sociologist. And Homans was actively hostile. But nonetheless, Parsons kind of carried on as though this were a mission, both the department and his own sociological program.

06-00:37:22

McIntosh: And meanwhile, you're operating in this space where you have a relationship with Parsons but you're also out on your own. But you had applied to Columbia and had this correspondence with Merton. And so you must have been aware of Merton's work at the time of the middle range theory and all that stuff.

06-00:37:38

Smelser: Oh, yes. Yes. Merton was a very big figure.

06-00:37:40

McIntosh: And so when Parsons would be trying to advocate for this push towards a grand theory, did you have any inclinations to align with Parsons over Merton or one way or another?

06-00:37:54

Smelser: Well, I was Parsons' student and not Merton's. I was very well aware of the tension between them and their theoretical position. Merton wrote this essay on theories of the middle range. Quite clearly Parsons was his polemic target, even though he didn't take out after him like a dog. That was quite clearly what he was criticizing. And I was aware of this and I was obviously closer to Parsons' camp because I was Parsons' student but I didn't stay up nights worrying about it.

06-00:38:24

McIntosh: Well, when is your dissertation completed officially?

06-00:38:30

Smelser:

I submitted my dissertation in November of 1957. That was six months before my junior fellowship was over. And so I got my degree at the beginning of 1958.

06-00:38:53

McIntosh:

And at that point are you already on the job market? Or what was your experience with the job market per se? At that point it's still an old boy network, is it not?

06-00:39:05

Smelser:

Well, the job market kind of unfolded without much effort on my part. It was certainly a seller's market. Expansions. It was a period of vast expansion of higher education and the major research universities were in the middle of it. There was a struggle for the top few people in the country, to hire them. So I never applied anywhere. The first thing I got was a feeler from Marty Lipset when I was at the American Sociological Association, four months before I submitted my dissertation. He asked me if I might be interested in being considered at Berkeley. So this was premature as far as I was concerned. I was thinking I'd take an academic job. Then Berkeley offered me the job. I never took any initiative in expressing an interest in staying at Harvard. They offered me the job to stay as an assistant professor there. Michigan came along out of the blue and offered. So it was a very different sort of job market than you now think. So in a way, it was kind of an effortless and premature thing, something I didn't have to worry about. All three of those places were wonderful.

06-00:40:08

McIntosh:

I think you mentioned—was there a nibble in Chicago? Even though that school represented such different—

06-00:40:13

Smelser:

I'll mention the job offers. I got a job offer from Columbia the day I arrived at Berkeley. Tenure. Merton gave me a call, a phone call the day before I started teaching and extended an offer on his own. It was a tenure appointment at Columbia. The following year Michigan and Wisconsin came after me and the following, Chicago, and two years later Harvard again. So I was very much in the market and perceived as an attractive young man in the field of sociology at the time. So I never initiated a job offer on my own ever. Never applied for a job.

06-00:41:17

McIntosh:

And so there are a number of schools in that list that would have given you the opportunity to kind of have that distance from Parsons and from Harvard. Why Berkeley specifically?

06-00:41:29

Smelser:

Well, we're getting into the next phase of my life here. Berkeley was an extremely attractive place to be as of 1958 when I arrived. It was growing

rapidly. It had a sense of boldness and confidence. They were hiring the best people that you could imagine, both young and established. One of my original motives for coming here was my older brother was living in Berkeley. That was a continuous draw to stay. So I did not say to people who offered me jobs, "Go away, I'm not interested." It wasn't that. And in 1970 I came very close to going to Harvard and then some years later I came close to going to Princeton but never did. The offer at Harvard was quite obviously that I was being picked to replace Parsons because he was retiring that year. That was just in the air. No one ever really said it but that was in the air.

06-00:42:45

McIntosh: Okay. And so I think we'll leave Berkeley for next time. But just another question I had about wrapping up at Harvard. You finished your dissertation in November, right, and then you had another semester.

06-00:43:03

Smelser: Six months.

06-00:43:04

McIntosh: Right. And so during that time, what were you doing?

06-00:43:08

Smelser: How did I spend my time?

06-00:43:09

McIntosh: Yeah.

06-00:43:10

Smelser: Well, I did revise my dissertation. The dissertation turned out to be an unbelievably mammoth enterprise. It was 900 pages of text. Obviously too long to be a book. So I spent, I'd have to say, two full months rewriting, compressing, making it into a book. Once again, I didn't have a problem with it because one of my teachers at Harvard, Peter Rossi, had gone to Chicago and he immediately told the Chicago Press, "Go after that book." And so I was invited to submit. So I guess up until about March or April I was rewriting my thesis into a book and sent it off.

Berkeley had said to me when I was hired, they said, "You can teach anything you want but we want you to teach one course and that's a new required course in sociological theory to undergraduates." They said, "You have to teach that." That was the only thing they laid down. That wasn't exactly a burden for me because this was my emphasis and I was kind of delighted. They thought to require anything was perhaps an imposition. So I said, "Okay. I'm really going to take this course seriously." So I began working on it, even in the late spring of 1958 when I wasn't due to teach it until the following fall. So that spring and a good part of the summer I worked on that. I continued the negotiations on my publication of the thesis. I began to do a little bit of work on the next project I undertook on collective behavior. So I kept myself busy. I didn't go to the beaches.

06-00:45:22

McIntosh:

Right. And was there any impetus to begin formulating a program for future research? You mentioned beginning to put together the collective behavior project.

06-00:45:38

Smelser:

Well, if you examined my whole history up to that time and the whole few years at Harvard, it was a natural assumption I was going to be a continuing and productive scholar. In a way, it was a question I didn't have to decide. It was in me so strongly that I was going to move forward as a scholar that I took it for granted and organized my life around that premise.

06-00:46:11

Rubens:

We left out just one important fact. We will talk about your family as a whole at some point but your first son was born.

06-00:46:19

Smelser:

Yes. He was born the day I brought in my thesis. Yes, that was marvelous. It was in Boston. There was a near-tragic moment there when I was driving to the hospital with my wife, who was in labor, in a rented car that I was using for a research project with Soauffer. It was a horrible rainy morning and I got rear-ended badly by a car behind me, and knocked my car into the car in front of me. Got squeezed in between. Interestingly, the driver of the car behind me was named Kelly and the guy in front of me was named Kelly—a Boston setting there. And I was panicked, of course. And, of course, the police arrived and they said, "Go to the hospital." So that was it. But that was in December of 1957.

06-00:47:16

Rubens:

So you had an infant that six months you were preparing your book and the course for Berkeley.

06-00:47:20

Smelser:

Drove across the country. In that period of that relatively empty period, this was the first months of my oldest son's life. Very much involved in it obviously. We drove across the country in the summer to settle in Berkeley with him in a car.

06-00:47:38

Rubens:

And his name is Eric?

06-00:47:41

Smelser:

Eric.

06-00:47:41

Rubens:

Who was he named for?

06-00:47:43

Smelser:

A name we liked.

06-00:47:45

Rubens: And so you were actively involved? You had one of the least pressured times, I would imagine, in your life.

06-00:47:52

Smelser: I was very involved with the infant. Yes, no question. I was proud and deeply involved. Yes.

06-00:48:03

McIntosh: Is there anything else that you'd like to add about—on your notes you mention—

06-00:48:09

Smelser: There's one thing I would like to talk about a little bit, that is the culture of the Society of Fellows a little more. This Anglophilic sort of culture and also sort of very prestige conscious culture. We had a lot of very famous people come around. It was an elite thing. Aaron Copeland arrived one day. Hans Bethe the physicist arrived one day for dinner. TS Elliot showed up and, of course, that drove all the humanists mad. There was a very funny story connected with it. The poet John Hollander, later became poet laureate, was a junior fellow with me and he, of course, when TS Elliot showed up, the guy went crazy and tried to monopolize him. And I happened to be sitting across from TS Elliot though Hollander was—and my friend George Jeffery Bush. I asked him if he had much chance to talk to TS Elliot and he said, "Well, I tried but John Hollander was sitting on both sides of him." [laughter] That was the kind of thing. And Harry Levin was at one end of the room. "Tom? Tom?" yelling to Elliot in his familiar way. This whole idea with the port being wheeled around in a silver little wagon after dinner. And quite obviously, the sense of self-importance.

06-00:49:40

Rubens: How many were in the society?

06-00:49:42

Smelser: Twenty-four. Eight were chosen each year. And we had lunches every Tuesday and every Friday and a dinner every Monday night. So it was a community and everybody attended it very faithfully. Some people were abroad doing their research and I was for my middle year. What I did was I took refuge in a couple of friends that I felt I had in common there, Rosovsky and Bush in particular. And we became very close and they kind of half shared my semi-alienation from that particular aspect of Harvard culture. Okay.

06-00:50:18

McIntosh: Fantastic. We'll end for today.

Interview #4 March 22, 2011

[Begin Audio File 7]

Rubens: All right. So today's the 22nd of March. Neil, good morning.

07-00:00:42

Smelser: Good morning.

07-00:00:43

Rubens: And we wanted to start with reviewing and assessing what your particular contribution was to Parsons, to the book that you wrote with Parsons, but also that then was realized in your dissertation and subsequent book.

07-00:00:58

Smelser: Okay. Talcott Parsons, after he gave his Marshall lectures, was struggling with the idea that the economy was only one system of multiple systems in a society and he was struggling further with the idea that there must be some systematic relationship among these systems. An exchange relationship. He had kind of put it down in primitive form in the lectures and was still wondering whether or not it could be formalized, whether or not it could be made consistent with his own theory and whether or not it can be made consistent with large bodies of economic and social theory. So the Marshall lectures were very incomplete in that regard.

And in particular, when I went to visit him for that very decisive week in Salzburg in the fall of 1954, we took this up. And I knew a lot more about markets than he did because I had just finished a lot of study of both historical and contemporary market structures. I began to feed in information to him about the structure of these markets, and in particular the role of different forms of money. Interest, consumption, wages, profits and so on might fit more systematically into his idea of where the economy links with the larger society. And I even had substantive suggestions which led to what we call the double interchange at each boundary of the economy, yielding a theory of consumption markets, labor markets, capital linking up with political concerns particularly and innovations linking up with the integrative system of society. Parsons had not made this terribly explicit and in particular he had not formulated this careful double interchange between markets, which led us into a lot of theories about instabilities in the markets and so on. So my contribution was actually to put some more meat on to this general idea which he had conceived. But my own reformulation just kept leading us in new directions and further directions and really informed two of the five chapters in that book very fundamentally.

07-00:03:30

Rubens: Well, [sociologist] Jeffrey Alexander [in his essay on "Smelser as a Sociologist of Synthesis"] is arguing that it was such a distinct contribution

that it really enabled his theory. I never know how to pronounce it –does one say just the letters A-G-I-L

07-00:03:39

Smelser:

A-G-I-L, yes. It formalized the AGIL and led to much more specific connections. I would have to say that Alexander perhaps exaggerated the originality of my contributions, but on the other hand, this was something—Parsons hadn't got there yet and I'm not sure he would have if I hadn't been—had my own resources to bring to bear. That's a hypothetical question.

07-00:04:09

Rubens:

So it's this particular formulation that then is elaborated in your book?

07-00:04:12

Smelser:

Well, it's used. My book was known really much more not for the theoretical elaboration of Parsons' work—I used the framework in *Economy and Society* as the framing theoretical structure for my work. But what was considered original about my work was that I had taken a decisive step and really given a plausible, if not convincing, interpretation of a well worked historical theory in the context of a general theory. I'd put a lot of meat and flesh on those bones by making an actual historical study. Secondly, and this was more important to the historians than the sociologists, British industrial history of that era, nineteenth century, was really overworked according to two themes. One, a heroic theme. Britain had led the way into the modern world. It was the first nation to be industrialized and so there was this kind of almost nationalistic interpretation of it that was about. But more powerfully, the interpretations that came from the left were that the industrial history was, above all, exploitation of the poor and that there was a great tradition in the Marxian literature and in the British socialist literature reinterpreting and reworking.

Now, I brought together a new explanation of the disturbances and working class protest that was different from either of these. I just didn't accept that. And furthermore, I thought I resolved some anomalies about worker behavior in the industrial revolution by pointing out that, in fact, it was in the periods when they were improving that the protests became much more intense. The question is why. If either of these other frameworks were brought to bear, it shouldn't have happened that way. I systematically documented in great historical detail the perplexity here and tried to give a more sensible account in terms of the theoretical framework of change which I was bringing to bear.

07-00:06:33

Rubens:

Right. I thought it was important to elaborate the theoretical part, as an amplification of our discussion in the last interview.

07-00:06:39

Smelser:

Yes. I think that's an honest account of the emphases and the readings that were given to that book and the reason that it caused a certain amount of

excitement. The left wing British historians didn't like it because they thought I was apologizing. So it became a controversial book, as well as an original contribution on that particular score.

07-00:07:03

Rubens:

And where was that controversy manifested?

07-00:07:05

Smelser:

In the reviews. And in subsequent journal articles. The usual after play of a work comes out. You'll get scholars around challenging this and that and the other aspect. So it was at the normal development in the literature.

07-00:07:21

Rubens:

Alexander also points out that in 19161 Homans is citing you as having achieved a theoretical elegance that never would have been seen in—

07-00:07:37

Smelser:

Well, here's what happened. Homans and Parsons were vigorous enemies. Homans was on my doctoral dissertation committee. He didn't direct me very much during the course of it. Matter of fact, I didn't communicate with Homans. But he read the whole thing carefully and he wrote me. It was the funniest little communication. He wrote me a note. He said, "Excellent job. Figures on page 132 don't add up." That was it. That was his whole critique. So it kind of breezed through as far as he was—he thought it was a good piece of historical work. Then, in his presidential address, Homans undertook yet another attack on Parsons for the reasons that he wasn't scientific, he wasn't behavioral. The usual Homans critique. And then he looked at my thesis. He says, "Smelser's written this brilliant historical work," and he described the book in very glowing terms. Then he said, as a way of getting at Parsons again, "The thing is, Smelser didn't use Parsons' theory. He used another theory," a better theory that's closer to the way that Homans himself would have interpreted it. So it was sort of a backhanded series of interpretations. But Homans was always very favorably disposed to that work, even though he and I mixed it up in different meetings around the country, at the American Sociological Association. We were on opposite sides really of the theory in sociology but nonetheless he had this mixed attitude. Obviously favorable towards me as a research scholar and historian—

07-00:09:26

Rubens:

Well, also as a theorist. Alexander argues that beyond an elegance, there is a new and explanative theory.

07-00:09:39

Smelser:

It was interpreted, generally speaking, my thesis as an original piece of work, yes.

07-00:09:42

Rubens:

So I'm glad that we got this in. Now, when you came to Berkeley, how did you regard—evaluate—the sociology department.

07-00:10:04

Smelser:

Well, I came out for an interview. It was one of these in-between interviews. They were obviously interested in me but the job offer had not been made. So I met with every senior member of the department that day. I didn't give a job talk. There was a luncheon which was sort of collective but it wasn't the usual—what we now know as the job talk. And all the interviews were basically pretty friendly and a person who later I was to become quite friendly with, Leo Lowenthal, at the end of the day, said, "You've got this job." He sort of more or less knew what the tone of the department was. I was feeling pretty confident. All the interviews were friendly. Kingsley Davis sort of gave me a hard time. He sort of gave me an orals exam almost on the nature of the British industrial revolution, which I couldn't quite figure out, but obviously I was prepared.

And then that night I went down to Palo Alto to visit with Parsons and Marty Lipset called me up that night and said, "You got the job." I think maybe I told this story. Parsons was trying to get me not to decide because he wanted me to stay at Harvard.

07-00:11:22

Rubens:

Right. Well, you mentioned clearly that one of the reasons that you came to Berkeley is because you wanted to be out from Parson's shadow.

07-00:11:29

Smelser:

Well, actually, and I don't think I mentioned this earlier, that I actually told Parsons, who made an effort to persuade me to stay at Harvard, I actually said at one point, "Talcott, I really would like to set up my own shop." And the look on his face was such that he understood exactly what I was saying. He sort of nodded. But still, he was pretty insistent that I should stay there.

07-00:12:04

Rubens:

But in terms of how you were seen by the department, you mentioned there was a niche that you were particularly filling? You mentioned preparing the theory course.

07-00:12:11

Smelser:

Oh, yes. They wanted to have a course in systematic theory to beef up their major, really, and to kind of make it a somewhat more rigorous major than it had been. So they had voted even before they offered me the job to this new required theory course for upper division students, and then when they interviewed me they said, "This is what we'd like you to do." This was, I think explained, the only condition of my taking the job. And it was obviously a welcome assignment for me because this was my bag actually.

Interestingly, and I will get more into this on the first two or three years, there wasn't anybody in the department who was really a student of Parsons or a sympathizer with Parsons. As a matter of fact, a number of the people like Blumer and Bendix and Selznick had publicly been quite critical of Parsons'

mode of theorizing and didn't really think of it as leading in fruitful directions and had been quite outspoken about it. And this didn't seem to make too much of a difference in my personal treatment, with a couple of exceptions. I felt I was being too much put into the Parsons mold and I objected to that. But I didn't feel that I was kind of an isolated minority. As a matter of fact, I was subsequently told by a graduate student, maybe three years ago, an Italian graduate student who was a PhD candidate at Berkeley at the time, that there was a big rumor mill among graduate students about my arrival and what did that mean as far as the department was concerned. Is Bendix really throwing in the towel and admitting that Parsons is okay? The way graduate students overdramatize sort of things. So there was some advance notice of me, particularly since I had written this book with Talcott, coming to Berkeley. It wasn't just like some stranger coming from another institution that seemed to be worth hiring. There was already a reputation that was coming along. Everybody knew it and there was a lot of talk about Parsons and Talcott visited the department a couple of times. I obviously was his host at the time because I was his former student. But I'd say my reception in the department was extremely warm.

07-00:15:02
Rubens:

Blumer was the chair at the time—

07-00:15:07
Smelser:

He gave up the chair the month I arrived and Bendix came in as chair. Bendix was very welcoming to me. I first lived in West Berkeley down by Acton Court. Bendix had inherited a substantial sum of money from the German government because of the maltreatment of his parents during the Nazi era. Bendix took this money, which was an outright payment from the German government, and bought a house next door to his. Six months after I arrived, it came free and he asked me if I wanted to rent it. So I did. I moved next door to him up on Mosswood Road above the International House. These houses were cheek by jowl. So I used to make jokes that Bendix was not only my employer, he was also chairman of my department and he was also my landlord and he should open a company store so that I could have to shop at it. So he and I were very thick. He didn't make any secret of his reservations about Parsons and sometimes I would show him some things that I was writing. We had critical discussions of it and so on but it was basically a positive relationship in so far as one could be close to Bendix. He was a somewhat remote man but I think I was as close as anyone to him.

07-00:16:48
Rubens:

And his specialty was—

07-00:16:49
Smelser:

He was an historical and theoretical sociologist. German by birth. And a student above all of Max Weber. And he differed with Parsons substantially on their interpretation of Weber and the translations and everything. So there was always a kind of—I wouldn't call it enmity. When Parsons and Bendix

met they were respectful and discussed. Civil with one another. But it was always a bit of a tension.

07-00:17:17

Rubens:

So just to flesh out a little bit more the department. There were major strains that existed in sociology—and later you'll write an article on this with Seymour Martin Lipset, published '61.

07-00:17:33

Smelser:

Yes. Yes. We edited a book on the basis of that article.

07-00:17:37

Rubens:

That's right. *Change and Controversy in Recent American Sociology*. You say 'we', meaning the two authors, are structural functionalists but we don't think that it has to be an either/or, theories versus methodology. So I'm trying to get you to locate yourself in terms of the strains of sociology at the time.

07-00:18:03

Smelser:

Well, yes. I was certainly labeled as being on the theoretical side and on the abstract side and certainly not an empiricist in the sense of the term, although my own research and my own inclinations always led me to ask empirical questions and I was pretty well trained in statistics and other methodological subjects. If there was any kind of tension between Parsons and me in all our work we did together, I was forever trying to bring Parsons down to reality to see if we could say something about the way the real world was functioning on the basis of these very vast and abstract categories which Parsons typically dealt with. So I was already by temperament in between these tensions. Of course, two of the people that I developed a good relationship with were both methods teachers: Charles Glock and Hannah Selvin in the department. There's always a little tension. That's what the field was all about. That, of course, was a key tension in the department. If anything, the theoretical side was stronger. Not Parsons' type of theory. It was a theoretically conscious department. We had difficulty getting people to teach the methods courses. It was in that tilt, unlike a lot of the Midwestern universities and North Carolina and other places which were very positivistic in their orientation. Very rigorous and emphasizing methodological training. We tended to be more in the Columbia/Harvard box of emphasizing theory a lot more. The department was very strong with respect to political sociology at that time. Lipset and Selznick and Bendix were the kind of three towering giants and they were all political sociologists in their own way.

07-00:20:10

Rubens:

Kornhauser was considered—

07-00:20:11

Smelser:

Oh, Kornhauser was the other one. He's junior. He was at my rank. But he added to it. I left him out largely because subsequently he left himself out of the department after the political struggles began. But no, he would be very much in that category. He was sort of a protégé of Lipset in a way. So I didn't

have any particularly strong feeling or animus about political sociology. I thought it was a very valuable line of inquiry and that the study of political structures and inequality, which was another thread that played such a big role, were completely—and was kind of proud of the fact that the department was so strong in these areas. From the very beginning, I placed myself temperamentally in the middle of things, middle politically or organizationally, and did not line up with contentious ideological or methodological positions in the department. Always, you might say, kind of a peacemaking role. Going back to my seventh grade teacher who told me I should have been a diplomat. There was a certain amount of truth in that.

07-00:21:29

Rubens:

And then intellectually also. Theoretically you were a synthesizer.

07-00:21:32

Smelser:

Absolutely. All the interdisciplinary impulses that I had and all the work I had done had been to draw things in, to incorporate what seemed relevant to a given problem, right, rather than to fix on an approach that I then would defend and use and set off against others. I tended not to be a polemic and I tended almost never to get into slugfests in reviews and counterviews and fighting and journal exchanges. I just stayed away from it almost entirely, with a couple of exceptions.

07-00:22:13

Rubens:

And the '61 article that I'm referring to, it has a kind of benign quality. You don't identify people. You just are talking about trends. But there's a phrase you have, "the current vitriol in recent public controversies," but you don't say who those people are.

07-00:22:31

Smelser:

Well, I've sort of reviewed them in the last few months for these interviews, as to what the divisions were and so on. The fifties were a pretty optimistic period in the social sciences, both with respect to their promise and their capacity to be brought to bear on policies, their scientific status. That was all upbeat and it was only a decade later, in the late sixties and into the seventies, that they began to take much more of a beating and much more fundamental division set in in that decade. So Lipset and I were kind of reflecting the times in that book.

07-00:23:15

Rubens:

You use the phrase "vitriol in recent public controversies". So there was something that was afoot. I don't know if those are remnants of the McCarthy era or if it's in the discipline itself. That's what I think you're talking about.

07-00:23:27

Smelser:

Well, a lot of it, certainly the discrediting of Parsons did not wait until the sixties. Barrington Moore was a major figure in that. So was Ralph Dahrendorf. So was Lewis Coser. These books all came out in the fifties and were pretty fundamentally critical of Parsons' sociology. The tidal wave

didn't hit until about ten years later. That was one of the ranges of controversies already.

07-00:23:57

Rubens: So let's talk about your theory course.

07-00:24:08

Smelser: Well, the actual teaching of the theory course turned out to be a very positive experience. I think I described how thoroughly I prepared the course in advance because I had that free time and was quite enthusiastic about the prospect of doing it. There were fifty students the first year and one TA, a man named Harry Nishio from Japan that I remember very well. It was a hard course. These were among the most abstract and difficult theorists around at the time. I now think it was kind of foolish for me to assign them because of the difficulty for semi-prepared undergraduates in abstract social theory. I didn't have any rebellions in the classroom. Most students worked very hard and tried to get it. I did my very best to try to bring clarity to what was very obscure prose that I was asking them to read. And I think—

07-00:25:14

Rubens: So you lectured in this course?

07-00:25:16

Smelser: I lectured in the course. Mainly a lecture course, although I always, from the very beginning, and it was easy to do with that many students, always welcomed interruptions from any source and would stop the lecture at a given time. Only on a couple of occasions did I have to say, "Let's return to the lecture." Even as the class got huge in the later years, I always made an effort to open it up to anybody who raised any questions of clarity or even opposition to anything I was saying, to speak up as much as can be expected in a class of that size and difficulty, I did encourage participation and discussion.

07-00:26:05

Rubens: And, of course, it's this course that leads to your book that had—

07-00:26:10

Smelser: Yes. Well, what I did was I taught this course seven years in a row. Each year it got larger, and so I accumulated not only students—the department's majors were getting a little more popular. And this was required of majors, so it would grow some in that regard. But it began to get a kind of reputation in other departments and faculty members in other departments would send their students around to it. I think Social Welfare even required their students to take it as a background, theoretical background to their own work. So that over that seven year period it grew to 350 students and I had six TAs. So it became more of a managerial—I continued the lectures. I changed the content. I would reexamine the content year after year. I asked students for feedback on which readings—how they reacted to the readings. So each year I would review what I had taught before and often would change a theorist

that had not proved to be successful in my mind as an educational experience for the students. I got on very well with the TAs. I met with the TAs weekly.

07-00:27:26

Rubens: These, of course, were graduate students.

07-00:27:28

Smelser: These were all graduate students.

07-00:27:30

Rubens: Did you have your own graduate students from the beginning?

07-00:27:33

Smelser: Yes. From the very beginning. I'll talk a little about that. I also began in my first few years to teach the graduate course in theory. I collaborated, early, within the first three years I was here I think I collaborated with Kingsley Davis, who had taught that course a lot. And so he asked me to teach it with him, co-teach it. He was much my senior. He was probably twenty-five years older than I, and a student of Parsons. But he had almost rebelled against Parsons and gone in a very positivistic direction. He'd become a demographer in the meantime. I was apprehensive about teaching with Davis because Davis was a difficult man. Quite authoritarian and quite snappish in his judgment. And Marty Lipset said a stupid thing to me when I agreed to teach with Kingsley. He said, "Well, he taught that course twice before with junior faculty and neither of them got promoted." Isn't that a tasteful remark to make?

07-00:28:52

Rubens: Yes. Well, certainly a warning.

07-00:28:55

Smelser: Well, it was. It scared me when he said it. But Davis and I got on. It was more a parallel play than collaboration. We would give our own lectures and—

07-00:29:06

Rubens: You divided up the theorists?

07-00:29:07

Smelser: We divided the lectures up. Davis didn't tell me this directly but I learned from people who he talked to who then came back and talked to me that he really respected my teaching in that course. But I never learned that directly from Davis. I taught once with him. I later taught with Philip Selznick and I later taught with Arthur Stinchcombe.

07-00:29:37

Rubens: And then much later, with Michael Burawoy.

07-00:29:53

Smelser: Oh, that was an uncomfortable collaboration.

07-00:29:56

Rubens: Do you want to just talk about it now?

07-00:29:58

Smelser: Yes, that's fine. Burawoy is a Marxist. He was an aggressive Marxist. Early on in the course he took to personally attacking my own work on the industrial revolution, on collective behavior, on subsequent work and so on. the graduate course publicly.

07-00:30:17

Rubens: And how many students were there about?

07-00:30:19

Smelser: Oh, in that time there must have been fifty or sixty. You've got all the graduate students in our department and then it drew in these ones from political science and other places. But Burawoy made himself unpleasant. So we had some fairly vigorous exchanges. I didn't sort of just let it happen. I argued with him and subsequently the number of students who took that course, they expressed—they said, "What was Burawoy up to?" They thought I had a better idea about how it should be taught civil behavior and so on and so forth. So no, that was not a pleasant—he initiated it. Burawoy initiated it but I sort of felt he had some kind of agenda in teaching it with me. I taught with Burawoy in 1980.

07-00:31:13

Rubens: You mentioned earlier that from the beginning you had graduate students.

07-00:31:27

Smelser: Well, the point is we had in the department at that time—I think they also did this about the time I arrived, they instituted that a graduate student had to be examined in his orals examination in four fields and one of them was mandatory and that was theory. That was the only one they had to take. So quite obviously teaching the theory course, I was chosen by many graduate students to be the orals—I was on dozens and dozens of orals examinations from the very beginning. And furthermore, this gave me an access, I suppose you'd call it, to graduate students, many of whom subsequently chose me to be their dissertation director or member of the dissertation. I was very heavily burdened, even in the beginning, with students who wanted me on their committee. And this was a gratifying part of my role, even though demanding. I was fairly conscientious in my directing and reacting to students work. So I got heavily involved in doctoral dissertations. I had, over the course of my career, sixty or seventy dissertations that I directed.

07-00:32:43

Rubens: At some point when we get later into your career we'll reflect on the graduate students who you most admired or who made the biggest contribution.

07-00:32:50

Smelser: Okay. See if we can take that up.

07-00:32:54

Rubens: Yes. We're just setting up your early years at Berkeley. Now, other courses that you taught?

07-00:32:59

Smelser: I taught a course in social change.

07-00:33:02

Rubens: By '62 your book will have come out on the theory of collective behavior.

07-00:33:07

Smelser: Yes. I didn't teach collective behavior until after the book came out. That was sort of a mistake for me to teach it because I had sort of said everything I had to say. Unlike a lot of other teaching I did, that was a little stale. I assigned the book. How much new could I say? I got a little bored.

07-00:33:32

Rubens: So we'll come back to that book, of course.

07-00:33:36

Smelser: But I taught a course in social change, undergraduate course. I taught a graduate course in economic sociology because of the link with Parsons and I was currently working on a book, a general book on the sociology of economic life along with the book on collective behavior. They both came out in the same year. So this was a course for graduate students. Arthur Stinchcombe was in the course. He was finishing up his degree here at the time. Interestingly, I got some pressure from the economics department not to name the course the sociology of economic life. Some people in that department thought they had a monopoly on the word economic and so I got a little delegation of economic professors who asked me not to use the term.

07-00:34:23

Rubens: Literally? A meeting was set up and they came—

07-00:34:26

Smelser: Yes. Two or three. They came in agitated with me. And I said, "I'm going to teach it the way I want." Called the Sociology of Economic Life and it was a graduate seminar. So there was a heavy teaching load. There were five courses. I can't recall exactly the others I taught at the moment but this gets me into the teaching side of it. I have to preface that Lipset was very much a guardian angel for me. He was the active force in my recruitment. He was the one who kept in touch with me. He was the one who agitated that I come to Berkeley. He was the one who really promoted me a lot. He was the one who initiated this article that we wrote together on the fifties and the book we edited with that article as an introduction to. He was very much an active sponsor of mine.

There was a little group that Clark Kerr set up on the campus at the time called the Center for Integration of Social Science Theory. It was a tiny little enterprise but it gave young faculty members one semester off for two years

and they supplemented your salary with the funds that Kerr had directed to the center. And Lipset pressed very hard for me to be named to that in my second year. So it meant my teaching load was cut into half for two years and I met with this group of scholars, interdisciplinary group of scholars, in a weekly or biweekly basis for that two year period. The members of that group were Erving Goffman, Richard Lazarus from psychology and Austin Hoggett and Fred Balderston from business administration and economics. Very helpful. And I was writing *Theory of Collective Behavior* at the time and I would bring my chapters in and we'd talk a lot about them; I would get feedback from all of them. And it was a very helpful enterprise for me for intellectually and also reduced my teaching load down to two courses. Two courses per year rather than the four that was instituted the year after I came.

07-00:36:36

Rubens:

Where did you folks meet?

07-00:36:37

Smelser:

Well, they had some office set aside in Wheeler Hall somewhere, as I recall. They did not have a home base. It was just a program rather than a center. We called it a center but it was just a bunch of people, bunch of scholars talking to one another. All social scientists and we read each other's work assiduously. It's a nice intellectual setting. Good little seminar.

07-00:37:06

Rubens:

Now, you were on a committee with the SSRC that was on economic growth.

07-00:37:15

Smelser:

Economic growth. One of my mentors, though we were never in the same institution, was a man named Wilbert Moore who was at Princeton. He was also a student of Parsons. He wrote a lot in economic sociology and in social change. On the basis of my dissertation, he recommended to Simon Kuznets, the economist, who was running a committee called the Committee on Economic Growth for the Social Science Research Council, a high powered group of economists and anthropologists and sociologists. Moore was on it and Bert Hoselitz the economist was on it. Melville Herskovits the anthropologist was on it. It was an intellectually eminent committee and I was the real junior guy on that. I was a little bit intimidated, particularly by Kuznets. But Kuznets took a liking to me. It was through that committee—they wanted me to undertake an intellectual project and so I decided that I would do something in the area of economic growth and have something to do with stratification. So this time I took the initiative, contacted Lipset, and asked him to co-edit a book with me on the basis of a major conference on social mobility and social stratification in the process of economic development. Turned out to be a fairly important book. It got reprinted about five years ago, as well. In that case, once again, a further collaboration with Marty.

- 07-00:38:51
Rubens: So that cross-fertilization, that kind of interdisciplinary intellectual milieu, you're getting that from the very beginning.
- 07-00:39:01
Smelser: Yes. We brought in anthropologists and political scientists and historians into this conference.
- 07-00:39:07
Rubens: How long were you on the committee?
- 07-00:39:13
Smelser: I was on the committee for four or five years. It was a fairly major assignment. I'm not exactly sure why I left it. But I was there for quite a number of years. Found it very intellectually gratifying.
- 07-00:39:25
Rubens: How often would they meet?
- 07-00:29:26
Smelser: Oh, three or four times a year in New York. It was New York based with the Social Science Research Council. It began a long career with the SSRC for me, too.
- 07-00:39:36
Rubens: So two years that you spent with this—
- 07-00:39:43
Smelser: Theory center here. But as of my arrival here, I was on that committee on economic growth in New York and so that was a major intellectual involvement outside of the university.
- 07-00:39:56
Rubens: I'd like to ask about your interaction with Clark Kerr. Of course, Clark Kerr becomes president of the university the same year that you begin your stint here. I was wondering when you met him.
- 07-00:40:12
Smelser: I met him a few days after I arrived. This was a social occasion for all new appointments. It was a party that he and Glenn Seaborg, who was the new chancellor, hosted. Kerr didn't know me from any stranger. But we shook hands, we talked a little bit. He, of course, was now beginning his stint as president of the university. Interestingly, I did not have a relationship with Kerr until the FSM blew up. We can get to that in that episode.
- 07-00:40:50
Rubens: We will do that.
- 07-00:40:51
Smelser: That's when it began and it only developed after he left the presidency.

07-00:40:58

Rubens: But you're saying about the theory group that Kerr—

07-00:41:03

Smelser: Oh, Kerr just set it up. He didn't administer it.

07-00:41:05

Rubens: And it was on Lipset's recommendation that you joined it.

07-00:41:10

Smelser: Lipset, who pressed me. There was a little selection committee or something that collected the people who got on it and Lipset pressed for my inclusion. He wanted to make life as comfortable as he could for me here.

07-00:41:26

Rubens: Now you had published some book reviews, early and also wrote an article before your book with him.

07-00:41:51

Smelser: It was in a journal called *Explorations in Entrepreneurial History*. We wrote an article in 1956 and then our book, *Economy and Society*, came out in 1956. So yes, I was—

07-00:42:01

Rubens: You were no stranger to journal publications.

07-00:42:04

Smelser: No. Are you getting on to my editorship now?

07-00:42:07

Rubens: Well, no. I wanted to first get your participation in other professional associations because we were talking about the theory group and then the SSRC. I was wondering how active you were becoming in the profession. Did you attend conferences?

07-00:42:23

Smelser: I went to all the meetings. And I would give papers but I would have to say that my appointment to the editorship of the *American Sociological Review* in 1961 was the first real engagement that I had. That was only two years after I arrived here. Maybe I should talk about that. It was a big, big part of my early years here.

07-00:42:49

Rubens: Then we can come back and do your research.

07-00:42:51

Smelser: Okay. We'll cover that work on collective behavior after this. Well, I got a phone call from Parsons, above all, asking me if I wanted to be editor of the *American Sociological Review*. He happened to be secretary of the ASA at the time and he was on the editorial committee that chose editors. He had pushed me strongly for this. I have to tell you that initially my reaction was a very

odd one. It was a huge job. It was a very big job. They have 350 articles submitted a year. You were responsible for sending out to associate editors, making judgments on communicating with it, coordinating the publication and printing of the journal every issue and so on. So it was a huge job. And I said to myself, kind of in a funny objective way, “Why should I, at this very early stage in my career, get involved in this deep and very demanding editorial enterprising that from the standpoint of my career is not the most rewarding thing I should be doing?” Editing is good, it’s a feather in your cap, but it’s nothing like publishing original and exciting and recognized works. And I also said, “Why did Parsons even suggest that I do this? It seems to me premature in my career to be doing something like this editorial work.” That was the internal dialogue that I was having at the time.

07-00:44:34

Rubens:

Some of the questions I would be asking of you.

07-00:44:35

Smelser:

Yes. Well, and I’m not quite sure why I didn’t simply say, “Talcott, I’m very flattered about this but my irons are in the fire and I would like to focus on my research and publication.” For some reason, I didn’t say that. I agreed to do it. I’m not sure why. I sort of thought I could take it in stride and continue my—well, as I did. Continued doing a lot of research. But I took it on and it proved to be—editors then were given a lot more freedom than they are now. It was not a controversial position. They wanted me to become editor and once I had agreed to become editor they said, “Edit,” right, and there was no surveillance. I got some complaints that were directed to the ethics committee on parts of disgruntled authors, maybe two or three complaints, but they never even brought them to my attention. I only heard about them later. So it was a wonderfully free sort of enterprise.

07-00:45:45

Rubens:

Who are they?

07-00:45:46

Smelser:

The publications com—

07-00:45:48

Rubens:

The editorial board?

07-00:45:49

Smelser:

Well, there was the ethics committee that they went to, the editorial board of the ASA. They just turned me loose, right. Of course, this was a beautiful way to be treated when you have this kind of responsibility. I had an editorial advisory group of twenty scholars that I would send out manuscripts to and take the advice of. It changed one-third each year. And I had a full-time secretary that the ASA provided. I got one course off from the department and the department gave me an office in addition to my own study. So there were some perks connected with it. No income. None at all. Only later did they start paying.

07-00:46:32

Rubens: Where were they based?

07-00:46:35

Smelser: ASA was a floating institution at that time, based mainly at NYU. But then it moved to Washington in 1962, I believe, because that's where the action was. Federal supportive research was beginning in a way and it made all kinds of sense to go to Washington. All the other professional associations either had moved there or were in the process of moving there. So I was not supervised by them, although I did form a relationship with the secretary of the association and with the printer. I had a deal with the printer.

07-00:47:08

Rubens: So how often were you traveling for that?

07-00:47:11

Smelser: Oh, three times a year. The editor of the ASA was also put on the council of the American Sociological Association, so I became, simultaneously, a member of the governing board for the three year period that I was editor of the journal.

07-00:47:29

Rubens: So you were on the SSRC. You're going to conventions for that.

07-00:47:32

Smelser: And I traveled to all meetings of the Council of ASA, which I think were maybe three a year. So I was already traveling quite a bit, all to the East Coast, to Washington and to New York. So I was already getting myself involved in the national establishment of the profession and the interdisciplinary groups.

07-00:47:54

Rubens: We've outlined some of the fissures in the world of sociology. As editor, it seems like it would be an incredibly powerful position and a political position to be choosing articles that speak a certain line over others.

07-00:48:13

Smelser: Oh, yes, it was. And I'll have to tell you what my personally evolved editorial philosophy was in editing the journal. Historically and to some extent right up to the present day, the ASR has been a journal that represents a solid positivistic empirical treatment, scientific design of research and has stayed away somewhat from theoretical articles, from historical articles, from social psychological articles. The softer side of the field has tended to be neglected in the publication policy by virtue of the editors they choose, by virtue of the understanding of what's either good or safe sociology, whatever. I decided to make it more catholic, broader. So very early I accepted an article on evolutionary theory which was almost—nobody read about in those days. I organized a whole article on human evolution, of people who'd been recently writing on it. Sort of a semi-commission. I didn't commission many. I could invite people to submit but I didn't commission articles because of the culture

that you were willing to consider everything that came in. So I didn't commandeer an editorial commissioning at all. I kept that aspect of reviewing everything that came in intact. I also published a few things that would be considered far out in sociology. In particular, I'll mention an article by Philip Slater. He is at Brandeis. He was a student at Harvard. I didn't know him very well. He was a little after I. He wrote an article. What did he call it? On social regression, which was an application of psychoanalytic theory to some group processes. It was very imaginative and very creative article and long. I decided to publish it as the lead article in one of the issues. The journal came out six times a year.

07-00:50:33

Rubens:

Wow.

07-00:50:35

Smelser:

And Lewis Coser, who was also at Brandeis at the time, told me, he said, "That was a very brave move on your part," and he said, "I think that's the most criticized article that's ever appeared." That I would go in this soft direction, Freudian direction in a scientific sociological journal, publish something like this. So I thought I pushed the boundaries a little bit. I never experienced any real direct criticism of this and I dare say a lot of the articles I printed were very interesting because they were on the edges and they were not in the mold of the standard empirical testing of rather limited hypothesis and observing the methodological canons. I didn't throw those out the window, obviously, and most of the journal still was in the traditional mold. But I took some steps.

There was another interesting aspect of my editorship and that was I began to get submissions from Berkeley faculty, thinking I was a soft touch, perhaps, that our young colleague was editor of the journal. Fortunately I'd been given tenure the same year I took on the journal. Oh, no. As a matter of fact, promoted to full professor the same year I took on the journal. So I didn't have this idea that I was this anomaly of an assistant professor dealing with my superiors' writings. And a couple of them sent me articles that had been hanging around for a while, that they hadn't published elsewhere, and they weren't very good. So I had to reject articles by Kingsley Davis and by Herbert Blumer, just because, in my opinion, they weren't good enough. I was scared, even though I didn't fear career retribution. These were the senior people in the department. I was this young, still an upstart almost, telling them they couldn't publish in the review.

07-00:52:42

Rubens:

Was this the second year of your editorship?

07-00:52:48

Smelser:

Well, I came '58. I took it on three years later. In '61, and I'd already been promoted by that time. I forget the exact calendar of it. But they both

submitted and I just found that I had to put on an objective editorial face onto this and so I told them, both of them.

07-00:53:18

Rubens: Were there any personal repercussions?

07-00:53:20

Smelser: Both of them told me I was a fool but I didn't change my mind.

07-00:53:25

Rubens: It just strikes me that you're juggling so much. By '62, aren't you starting analytic training?

07-00:53:37

Smelser: Maybe I should say a little bit what my work schedule was. I'm a morning person in terms of my metabolism and energy. So almost just the time I arrived I decided I am going to lay out my day as follow. I'm going to get up at 6:00 in the morning and I'm going to start working and I'm going to work non-stop until noon. I will not schedule anything else in the morning unless absolutely necessary. So by noon I will have almost put in a workday. I would schedule my teaching always in the afternoon. Whatever limited amount of committee service I had, departmental meetings and so on, were always in the afternoon and I would meet with people who were coming into town in the afternoons. I regarded those morning hours as very sacred and I was very productive at that time of day. And I did this sort of absolutely religiously. It was a very—

07-00:54:47

Rubens: This was at home? You would work on the ASR at home, your courses?

07-00:54:51

Smelser: I'd work on the ASR. I'd work on my library books. I would prepare lectures that I needed to. Everything I could do on my own I would do at home. I was a fairly efficient worker. There's one story. When I moved into Bendix's house and it had a fantastic view of the whole Bay and it had a roof over a living room that I could sit outside and do work. I had a card table out there doing work like this. And in the afternoons when I happened to be out there, I would put on earplugs and I would listen to baseball games as background. I was always working but this would be background noise of some interest and so on. Bendix, who lived next door, saw me. He'd see me every day out there writing away with these things on my—he thought I had some technological wizardly with these earplugs. He wondered how much productive I was getting without—these earphones were all about. It was kind of—

07-00:56:01

Rubens: It was kind of advanced technology because you must have been using a transistor radio?

07-00:56:05

Smelser:

No, I plugged it into a regular radio. It was very low tech. But anyway, that's a very interesting little kind of humorous sideline to that. But I worked very, very hard, very steadily. Already, in 1959, my family was growing. I had a daughter in 1959. I was a good father. I took a lot of care of my kids.

07-00:56:32

Rubens:

There was Eric and now and Tina.

07-00:56:35

Smelser:

She was born in '59. And so I sound as though I were some kind of machine. I wasn't. I did carry on a family life. And I will say a little more about that later. And I carried on some social life with colleagues and so on.

07-00:56:57

Rubens:

I've heard there was a poker game you participated in. I don't know how early that was started.

07-00:57:03

Smelser:

Oh, that was a little later and I was in it with Kornhauser and Goffman and some people from social welfare. It started maybe '62. We'd play every couple of weeks. The reason I can remember that is that Kornhauser in '64 got alienated from it and had a huge fight with a couple of other members and left over the politics of the FSM. That's how I can date it, because it was an ugly departure from the poker group.

07-00:57:38

Rubens:

So how long did this last, this poker group?

07-00:57:41

Smelser:

Poker group, that early version of it, lasted maybe five to six years. Then later on I was in another group in the seventies that lasted also about the same period of time. Different group.

07-00:57:58

Rubens:

So contrary to any sense that you might be a machine, you're listening to baseball, you're part of a poker game, you're involved with your children.

07-00:58:05

Smelser:

Yes, life, my life was going on.

[Begin Audio File 8]

08-00:00:08

Smelser:

Something I didn't mention earlier, is that when I arrived in Berkeley with my wife and son in I believe August of 1958, we stayed temporarily in a faculty member's house and then got a place to live. But the day I arrived I got a message through the department that Bob Merton was trying to get in touch with me. The next day I returned a phone call and he said, "We would like you to come to Columbia as a tenured associate professor." The very day I

arrived, right. And so I was, of course, totally flabbergasted. The circumstance of his knowing about me was that I had submitted my thesis to the University of Chicago Press and he had been selected by the University of Chicago Press as a reader. Very appropriate reader. He'd done work in British social history. He was a major theorist in the country. They sent him the thing. He had known about me from Parsons. He was Parsons' student also. And he had read this. And he not only read it, he sent me a seven or eight page review that he wrote for the University of Chicago Press saying, "Publish this, absolutely," and so on, and he went on to elaborate why and why and why and it was on the basis of a reading of the thesis that he convinced his buddy, Paul Lazarsfeld, that I'd be an addition to the Columbia department and they took a very bold move of offering this young man a tenured position.

I reported this to Bendix, who was the chair. Created a terrible problem for Berkeley. Here this guy just arrived on the front steps of the university with this kind of offer from Columbia. I put myself in their shoes. What would you do with this kind of situation? Well, the department very hastily convened and voted unanimously to recommend me for tenure at Berkeley.

08-00:02:22

Rubens:

I was going to ask how that came about so soon.

08-00:02:24

Smelser:

One month. Two months after I'd arrived. That says a lot about your reception, even in advance, of how you were regarded by the department. This in turn put the administration on the spot. Are we going to promote some person who's just arrived to full professor? And so Lincoln Constance called me in. He was dean at the time. Called me in, talked to me. He said, "We can't do this. We can't promote you." He said, "What I'm going to do is to tell you in as strong a way as I can tell you that we will bring this up for promotion next year and I will personally support it and please don't go." However, they did not match Columbia at the time but they did everything but. And so on the basis of that, plus the fact the next year I got an offer from Chicago and the following year I got an offer from Michigan and Wisconsin, the pressure on the university was there. The external endorsements were there and the promotion went through as Constance had promised. The department was completely unanimous all the way through. I guess that tells you more than anything else about your personal reception.

I didn't read it as the fact that I'm now arrived or anything like that. I was still a very young assistant professor. You don't know how well established or how received you really are. Your fantasy life is very active. You can always imagine the worst. But nonetheless, the realistic treatment by the department was extremely favorable and warm. Well, the whole atmosphere, of course, was one of expansion. Guaranteed Berkeley's quality. We're going to get the good people. They'd hired really strong people like Goffman, like Trow and others about the same time as I was hired, so I knew that their national

competitive impulse was extremely strong. I knew that the commitment to quality was extremely strong. And so looking back on it, it seemed to be a very rosy moment for me, and it was, although, in a way I sort of couldn't believe it—that I just basically forwent all the anxieties of a young assistant professor waiting around for four, five, six years struggling to continue the productivity and so on for a promotion. In a way it was a fortuitous time in history because the demand around the country was so strong. Institutions of higher education were expanding rapidly. Their faculties were expanding. It was a seller's market. I once said everybody owes it to himself to be born in the Depression. That is, it was extremely favorable and I never forget that. I don't attribute all this by any means to something about me. The system was operating in a very dynamic way and I happily was, in a way, a beneficiary of that particular academic culture and that particular academic time. And, of course, Berkeley shared in it and they wanted to keep the good people. So they were extremely responsive.

08-00:06:03

Rubens:

You've written in a couple of places in *The Academic Market*, and some of your review essays on higher education, that this was really the golden era of both this department of sociology but also of the university system.

08-00:06:17

Smelser:

Well, yes. It was the baby boom. Expansion. They enjoyed a very rosy period in the veterans' era. Then here came another fantastic era. There was a period of long economic growth globally. California was thriving and full of resources and so on. And we were also very strongly in the competition with the Soviet Union as a result of Sputnik and the Cold War.

08-00:06:53

Rubens:

So much federal money coming in here.

08-00:06:54

Smelser:

Federal money was beginning to flow in. Foundation money was flowing in. Corporate money had not yet started. It was just the era of plenty. Now, there was one respect in which it was not a golden era. We had not one woman in the department and we never even thought of hiring one. That was such the culture that no one ever thought that was a problem.

08-00:07:14

Rubens:

Now, Margaret Hodgkin remains on the list as professor emeritus but she wasn't an active—

08-00:07:21

Smelser:

No, she was never around. She was fully retired and was away. She was a professor in the department but I never met her. So she was completely withdrawn from the scene. Like Robert Nisbet, They were gone from the scene. I met Nisbet later and came to like him a lot. But no, she wasn't around.

08-00:07:42

Rubens: So, taking the editorship of the ASR had nothing to do with getting tenure or—

08-00:07:47

Smelser: No. No, no. I didn't give you a very good account of what my reasoning was for taking that job and I don't think I can. But the fact that I was already tenured certainly eliminated that issue. That I was not going to be suddenly caught down, my research grinding to a halt and I was doing this crazy editorial stuff. That image was not there. So that promotion to tenure freed me to choose and take on that very interesting—turned out, interesting editorial work.

08-00:08:26

Rubens: I don't know if we did enough on that. But maybe we'll get back to it in terms of lines of discipline.

08-00:08:35

Smelser: Yes.

08-00:08:35

Rubens: But I do want to ask you about your students, about the quality of students in relationship to trying to decide whether you go to Columbia or to any other place. How did you find your students?

08-00:08:49

Smelser: Well, Berkeley was thriving with respect to graduate students, as well. I wouldn't say that the quality of students that I would be directing or dealing with was the highest consideration in my mind at the time. My assumption was that all three of the institutions, Harvard, Berkeley, Columbia, that were in the air, and Chicago, were all at the top of the heap. They were, and in a way, their graduate students were interchangeable. I found my relationship with my graduate students to be positive. They were smart and I had good relations with them. I had an especially close relationship with Gary Marx in my very early years here. A lifetime friendship has evolved from it. He edited, along with Jeff Alexander and Christine Williams, *Self, Social Structure, and Beliefs. A festschrift* for me. And so I was happy with my role with students. I felt I was really chosen to direct their—and I've been told subsequently by former students who always tell you something twenty years later, they never tell you at the time, that I had the reputation of being a student friendly faculty member. Not everyone was. There were selected members of the faculty that were regarded as sort of martinets or ogres. Well, Davis was one of them. He was very punitive. And Kornhauser. I mention those names as being people who had difficult relations with graduate students and didn't have too many of them.

08-00:10:30

Rubens: You've mentioned something on student files on you.

08-00:10:34

Smelser:

Oh, yes. Well, I was on so many orals examinations that over the years the students developed a kind of a book of the questions that I asked in any given year and they would pass it on to the next year as, "This is what this guy's thinking. This is the sort of thing you might be on the lookout for." So there was a circulating history of questions that I had asked on orals exams. Those students, they used it as a kind of a crib sheet almost, thinking that you might be better prepared if you know what's on this guy's mind. And then a few years after I got here I got wind of this. Some student told me that this was going around. So this motivated me to change my questions, playing last tag with this system, this filing system that students had built up on me.

08-00:11:25

Rubens:

I thought you were referring also to the Slate reviews.

08-00:11:27

Smelser:

No. Of course, my courses were reviewed by Slate, the Slate Supplement. And they were positive, generally positive.

08-00:11:40

Rubens:

I'm wondering if you want to talk a little bit about the political climate on campus. There was the development of Slate, the graduate student organization. There were lots of demonstrations that were anti-capital punishment. By '60 there'll be the demonstrations against the House Un-American Activities Committee. There just was a spate of things that are taking place that are different from the era that comes on with the Free Speech Movement.

08-00:12:10

Smelser:

Yes. There was a lot of political activity. Nothing as dramatic as the Free Speech Movement. And I did not involve myself one way or the other in most of it, although I was a very frequent and sometimes mandatory observer. One time in my theory course a student came up to me who asked me to postpone his final examination because he wanted to march in the [Caryl] Chessman protest. This was a male student in the class. Posed it to me. Basically he was saying this is a higher calling and you should honor my request to take the exam later. It posed a little dilemma for me as a faculty member. My response at the time was, "I'm very sorry. The exam is going to be given and you're either there or you're not there." So I took a professorial approach to that.

I'll mention another incident now in that we're talking about it. I was co-teaching this theory course with Art Stinchcombe at the time of the Third World Strike. It was '68, or '69. But anyway, by that time there were quite a few minorities beginning to appear on campus. And my theory class with Stinchcombe had maybe sixty or seventy students, of whom ten were minority. Mostly black. The strike occurred during this course. The minority students stopped coming. It was right at the beginning of the term. About three days after the strike began, a delegation of the minority students in the

class, and a very hostile delegation, came to see me. Stinchcombe didn't want to have anything to do with it, so I carried the ball on it. They wouldn't meet in my office because that was territory they wouldn't—wouldn't cross the picket line. They would meet with me in the student union. So I went and met with them. They said, "You can't hold this class on campus because we can't come to it." Said, "We demand that you move this class off campus." They were pretty ugly. It was a time when civility was really sort of down the tubes. And I got the idea at the time, without consulting Stinchcombe, I said, "Look, I can't do this. The majority of the class is taking this course under normal circumstances, and I'm not going to tell the whole class that I am going to change just because you're asking me to." Then I said, "I'd be willing to teach you this class separately off campus." They didn't know what to do. Took them completely by surprise. They said, "We have to caucus." So they left and then they sent a delegation back to see me late that night. "We accept it." They were, among other things, scared they would jeopardize their graduate careers if they wouldn't come to the class. So I told Stinchcombe about this. He was very happy that I had handled it in that way.

The atmosphere at the time was carried home to me when I walked out of the—they came to my office, the two told me they would do it. I was finished with my work anyway, so I walked outside with them and we walked out the door of Barrows Hall. And we were jumped by two policemen, armed policemen because of these two black guys. I said, "These are my students," and the policemen backed away. But that was the kind of tone that was being set there. So we met with these—and then at the end of that course—I'm sorry to be taking so much time but it was a very interesting episode. At the end of that course, a delegation of minority students said, "Look, we've been involved in protesting this semester." We want to get incompletes in this class and we don't want to take the exam." So at that point, again, I was negotiating without Stinchcombe. I said, "I can't do that." I said, "Tell you what. I'll give you two weeks grace but I want to get my grades in on time," and I could do that within the flexibility of the scheduling. So they said, "Okay." The day before the exam, two of them came, said, "We're not taking the exam." And it was kind of ugly atmosphere.

08-00:17:17

Rubens:

They were really pushing you?

08-00:17:19

Smelser:

Yes. And at that point I said, "Well, if you don't take the exam, you'll find an F on your record." And they came. They buckled at that point. I was quite scared. This was one of these ugly times. And six months later one of them came back and thanked me.

08-00:17:41

Rubens:

So you had, in fact, for most of the semester, or at least half the semester, taught them separately?

08-00:17:46

Smelser:

Yes. I dealt with this thing by doubling my teaching load. Basically taught the course and the class as usual and met with them in what they called liberated territory in the school of industrial relations off campus.

08-00:17:58

Rubens:

Well, it's a good story. It's a little out of sequence but it's speaking about the difference between the period—

08-00:18:03

Smelser:

Yes. Now, back to the earlier period. Yes, a lot of my students went to the HUAC hearings and came back and talked to me and they wrote term papers on it and so on. So I was getting involved in it. And I would follow this stuff but I have to say that I was more intellectually—my intellectual compass was not into the political situation on campus and I didn't really get involved until it became big and ugly and consumed a—

08-00:18:33

Rubens:

Well, and then you were placed into an administrative position.

08-00:18:35

Smelser:

Then I joined in in a very big way. And we'll have that episode later. But I would have to say I was interested and informed, not active and did not choose sides.

08-00:18:50

Rubens:

And did you feel pressure at all from anyone in the department or even people outside the department?

08-00:18:53

Smelser:

Not then. The department was not affected by these earlier—I don't know. Some faculty members were sympathetic to the sit-in and they would see the students. So it didn't become a collective matter. It was individual department members. And it didn't affect the culture of the department.

08-00:19:07

Rubens:

That's the question that I should have posed. Did it?

08-00:19:09

Smelser:

No, it didn't.

08-00:19:10

Rubens:

Because there were faculty who were involved in the Rumford Act, for instance. They were trying to get the elimination of discriminatory practices in housing, which still were in effect in Berkeley until '63, '64.

08-00:19:27

Smelser:

Yes. I would still have to say that insofar as any faculty members who were actively sympathetic with some of the movements that were going on, any participation in it was individual, not collective. The department didn't collectivize in 1964.

08-00:19:42

Rubens: Okay. And in terms of pressure that one would feel, that wasn't there.

08-00:19:46

Smelser: No. It was—

08-00:19:47

Rubens: It was collegial and—

08-00:19:48

Smelser: I was still swimming in an intellectually wonderful atmosphere which was vibrant politically but didn't hold a candle to my intellectual interests.

08-00:19:59

Rubens: You've mentioned about outside the department. Henry Rosovsky and David Landes in economics. The Society of Fellows connection with Henry.

08-00:20:21

Smelser: Well, yes. Henry and I were in the Society of Fellows together. We were the closest of friends. We were both slightly alienated from the Society of Fellows culture and we remained very, very close family friends with him and his wife. He took a job at Berkeley at the same time I did in the economics department. David Landes, who also had a Harvard connection, another economic historian, also took a job. We had a little group, Henry and David and I. Especially Henry. Very close, very good friends.

I had some friends in anthropology, Clifford Geertz, later a very eminent anthropologist. He and I were at Harvard together, though we didn't know each other very well. And then Lloyd Fallers I would mention and also David Schneider. They were both faculty members, young faculty members. Schneider was one of my teachers at Harvard in anthropology and he joined the department. So I had that connection with him. And then in the department I was very friendly with Bendix, of course, who was there, and Charles Glock and I were friends and Selvin, as I mentioned—and Erving Goffman in particular. I'll mention a little thing about Erv. He was hired the same time I was. When I came here, we had a lunch of the department over off campus. And Erving sat next to me. Erving was a very sharp and aggressive man. Began attacking me. Kind of a strange, funny attack, though. He began attacking Harvard, he began attacking Parsons and all that stuff. It was kind of to my mind, kind of childish. He was sort of doing this as a very caricatured sort of criticism.

08-00:22:10

Rubens: Had he been drinking?

08-00:22:12

Smelser: No, no. It was his style. He attacked everybody. He brought everybody to their knees. He was a very aggressive guy and he was so clever. He would discover a person's weak spot in a microsecond and go after it and he thought this might be my weak spot or something. I don't know what it was. Anyway,

I sort of disregarded this, what was going on here. Didn't fight with him or anything. This kind of baiting kind of went on for a while. Pretty soon I began, sort of said, "Erving, what in the hell's going on? Why you doing this?" I gradually broke through that and we then developed what turned out to be an extremely close relationship. I was really kind of devastated when he left right after the Free Speech Movement. He got a high professorship at Penn in 1965 or '66, I think, and took off.

08-00:23:01

Rubens:

And what was his specialty? *Asylums*. Wasn't that one of his—

08-00:23:06

Smelser:

Asylums was one of his books. He was a social psychologist known for the dramaturgical theory, framing analysis. Extremely brilliant guy. Admiration for his mind was unbounded. He was so clever. And he also would poke fun at Parsons and so on. But pretty soon it all faded away and we became friends on our own.

08-00:23:29

Rubens:

I meant to ask you about social psychology. That was kind of a strong suit at Berkeley, wasn't it?

08-00:23:37

Smelser:

But I didn't have much connection with the psychology department, then, except through Lazarus. He was the guy who was especially interested in the study of stress and anxiety and he was in my theory group. And because I was talking about collective movements, we really had a meeting of the minds because intellectually we were kind of in the same place, even though we were in different disciplines and addressing somewhat different problems. We really had the electricity in our interchanges. But even though my brother had gone through the graduate school in psychology I didn't have many connections with the department. They developed later.

08-00:24:12

Rubens:

But in the department of sociology, were there people who reflected—

08-00:24:18

Smelser:

Well, Herbert Blumer. Herbert Blumer I should talk about. I had a different relationship with him. He was one of the standing figures in the study of collective behavior and social movements and this is where I was at in my research.

08-00:24:32

Rubens:

And he had come from Chicago?

08-00:24:34

Smelser:

He had been at the University of Chicago. He'd come out here. He had helped build the department. He's a—

08-00:24:38

Rubens: Had Clark Kerr recruited him?

08-00:24:40

Smelser: I think so, as the initial leader of the department. And he did a fantastic job of bringing good people here. He and I had rather different approaches. He was a deep critic of Parsons. He didn't like the idea that the system was the determining factor of so much of life. Sort of rescuing the individual and he believed in this. However, I was working right in his field so I decided I would try to establish a relationship with him. So I would write my chapters and I would give them to Herb for feedback.

08-00:25:22

Rubens: You're speaking of the *Theory of Collective Behavior*?

08-00:25:23

Smelser: As I was writing *Theory of Collective Theory*, I would give Herb my chapters and then we'd have a lunch and he was extremely negative. Very critical, saying I was off on the wrong track intellectually and it was all this idea I was building up a system that was determining people's behavior and there wasn't any choice on their parts and what really is going on is the action about people reacting to their situation. He had this particular symbolic interactionism, this approach that was almost a religion for him and he would inflict it on me as being a representative of something that was untrue and unworthy. Well, we had a couple of lunches. They both went the same way. So I decided, well, because I presumed he didn't really find this very rewarding because it was so repetitive, and I didn't accept it, either. But then I continued to send him chapters and he would respond in writing to the chapters. But the same message. A ten page letter saying the same thing. Every letter was the same. So we had an exchange of 40, 50 pages and he would say always the same thing. I would learn nothing from it because it was all said before. It wasn't a productive relationship. He always expressed, intellectually, admiration for me but he had certain remoteness. I felt it was on his side because I didn't experience it with other faculty members. A remoteness, a distance, kind of standoffishness. We should have been friends. He was of German background from Missouri. [laughter] And he had been accepted as a Rhodes Scholar but decided to play athletics instead here. And so we should have had a lot going for each other but it never crystallized.

08-00:27:16

Rubens: And had he been a lefty also?

08-00:27:18

Smelser: No. Not really. I'd call him a softy but not a lefty. But he and Davis were the two people that I didn't really get on with. I wouldn't call him enemies. We saw each other socially. It wasn't an inimical relationship but it was a remote relationship in which I felt some discomfort.

08-00:27:40

Rubens: So let me check on how much time we have left.

08-00:27:45

Smelser: Let's look for any untied ends you might want to—We'll take up my research next time.

08-00:27:50

Rubens: I think so. I wanted to talk about your early service to the Berkeley community. You were placed on the chancellor's committee on discrimination.

08-00:28:04

Smelser: Okay. I'll talk a little bit about that. Basically, those first six years before 1964 I would say that I had two main audiences in my mind. One was my colleagues. Interdisciplinary relations in Berkeley have never been the strong suit, unlike Chicago, for example. So my department was my immediate culture, mainly, with the exception of the friends I had, tracings to Harvard and some other departments. And my second audience was the national sociological world. Those were the two main audiences with which I was conversing at that time. I liked the situation here at Berkeley. I was completely satisfied with my situation but I had very little sense of the institution as something you might be loyal to or love. I wouldn't call it an instrumental relationship but it was an unattached relationship with the university as a whole—a situation that changed dramatically beginning in 1964. And we'll talk about that personal change that I underwent.

So I guess this was instrumental on my part. The university was providing me with tremendous professional opportunities and I was taking advantage of them. There was also a university policy that you don't bother assistant professors too much. They don't put them on big committees and you don't ask them for these assignments and that sort of thing. You let them prove their worth and so on. You don't immediately come into this line of fire of this academic senate or the administration as being someone they're going to call on immediately to do important administrative work. So there wasn't too much demand. However, I was put on this very interesting committee, I believe it was two years after I came, called the chancellor's committee on discrimination. David Blackwell, the statistician, was the chair of it and it was a committee of eight or nine people. We didn't have any power.

08-00:30:19

Rubens: David Blackwell was one of the first African Americans at the Cal.

08-00:30:22

Smelser: Yes, he was. Very eminent statistician and wonderful man. I liked him tremendously. He chaired it. He was a gentleman. He wasn't a militant. But we would raise questions about admissions.

08-00:30:32

Rubens:

So did you volunteer for this or were you asked to be—

08-00:30:35

Smelser:

No, I was asked. I was asked. It was an administrative committee. So I was asked to be on it. I'm not sure why. They just heard of me, and wanted some social scientist, bright and young social scientist on it. And so we would raise questions about fraternities, we'd raise questions about admissions, we'd raise questions about—because there were rules against even asking people about their ethnic—there were color blind rules at the time. We raised some questions about that. We couldn't even find out what the percentage of minorities was on the campus because they were not recorded. We had it based on a campus survey by a survey research center to find out how many minority students there were on the campus at the time.

08-00:31:12

Rubens:

Hard to believe.

08-00:31:15

Smelser:

We talked a lot and we raised a lot of questions and I think Blackwell talked to the chancellor's office on the basis of our deliberations. It was a pretty weak committee. I would say it was probably not decisive with respect to any influence it might have had, even though I remember the work we did as being certainly engaging and important questions that were being—and I contributed actively to the intellectual work of the committee. But it didn't hold a candle to the forces in the area of discrimination. That came later. But that was my only real campus concern.

08-00:32:02

Rubens:

You also served on a couple of committees in the sociology department.

08-00:32:14

Smelser:

I was on the admissions committee. I kind of forget. One would come up each year. You'd be asked to be on it.

08-00:32:25

Rubens:

It shows up in your bio bib, from '59 on. I think we'll talk about admissions later when we discuss your book *The Academic Market*.

You were also a consultant to—

08-00:33:00

Smelser:

Oh, yes. Early on, Prentice Hall, with whom I published the *Sociology of Economic Life*, and then became a regular—I published other things with them as well later on. Yes. Blumer had been a series editor. In other words, he had approved every book that was published by Prentice Hall and then he stepped down a couple years after I arrived. And the series editor at Prentice Hall approached me and asked me if I wanted to take that job. Another big job, editorial job, to read every manuscript that they were thinking of publishing in sociology. And they put it in my list and I got a two percent

royalty on everything they published. I was in that position for close to twenty years with Prentice Hall.

08-00:33:52

Rubens:

Oh, goodness. I thought this was a year's—

08-00:33:55

Smelser:

No, no. I stayed them with a long time. Of course, it contributed to my ongoing education and when they were in the business of publishing more scholarly works than they later came to publish, I would send my graduate student dissertations to them and a lot of them are published by Prentice Hall.

08-00:34:14

Rubens:

Oh, really? That's important.

08-00:34:16

Smelser:

That was a nice little avenue that I had for my graduate students. So yes, an additional assignment that I took on. They finally excused me because they thought it was too big an expense on the publisher's part to pay me for this.

08-00:34:32

Rubens:

Usually we don't talk about remuneration. But the salary of a sociology professor couldn't have been too high at the point that you came in.

08-00:34:44

Smelser:

My beginning salary was \$5,500 a year. Of course, you got to correct for what time it was. They brought me in at not the lowest rank of assistant professor. But once again, Lipset began pushing. "Appoint him somewhere up the line." So I came in at kind of lower middle level of the assistant professor ranks. No, I wasn't terribly uncomfortable. Any bit of supplementary income that I got, which wasn't much—I got nothing for the editorship. I basically didn't start getting money from Prentice Hall until later as accumulation. However, this rapid promotion that I went through started shooting my salary up, as you can imagine, because salary was tied to rank. So I became increasingly more comfortable economically. I had a huge blow, temporary blow, in my divorce.

08-00:35:40

Rubens:

I imagine.

08-00:35:41

Smelser:

But otherwise I—

08-00:35:44

Rubens:

I imagine the book with Parsons, as well as *The Theory of Collective Behavior* must have had a good return.

08-00:35:42

Smelser:

They were not enriching but I got good royalties on both of those books, especially *Collective Behavior*, continuing for years and years. A kind of text. It was in print really forever.

08-00:36:07

Rubens:

Yes. Well, do you think that's where we should end today? I don't know if we did enough on your editorship. What a powerful position.

08-00:36:32

Smelser:

Well, I could give the day by day. One thing that's left out is the style I developed dealing with authors, because it had a rejection rate of 85 percent. And this meant that there was a very diplomatic dimension.

08-00:37:03

Rubens:

You talked about rejecting Blumer and Kingsley Davis. They were part of the 85 percent.

08-00:37:26

Smelser:

Yes. I used to say that the job brings with it an 85 percent level of enemies because you reject that many of your people. I began developing an art of rejection. In other words, a rejection is a rejection. Everybody's going to feel bad about it. But I would try to draw out some strengths in the articles when I would talk about why they weren't accepted and I would give helpful advice as to where it might next be sent and try to be as sympathetic as possible. I spent a lot of time on the diplo—rather than a form letter saying, "Sorry, goodbye." And then I would sometimes quote associate editors who would have something good to say about it. I developed a joke. I would write to these people and I'd say, "Your article is so brilliant that it would demean our journal to publish it." I never wrote that. That was my sort of semi-sick joke about being nice to authors. But I got flak. You never—

08-00:38:39

Rubens:

How could you not?

08-00:38:40

Smelser:

I got flak and I got some accusations of being ideologically biased. You'll get this with a number. You get more than a thousand articles over the period of time; you're going to ruffle a lot of feathers.

08-00:38:58

Rubens:

And do you think that that played into why you weren't head of the ASA until later in your career?

08-00:39:02

Smelser:

Don't think so. I think it had to do with the internal politics of the ASA, which I will talk about. I was in my sixties when I was elected president.

08-00:39:17

Rubens:

So should we wait to talk about it later?

08-00:39:19

Smelser:

Yes, we'll talk about it later.

08-00:39:19

Rubens:

All right. So I think we'll say goodbye for the day.

Interview #5 March 29, 2011

[Begin Audio File 9]

09-00:00:03

Rubens: Good morning, Neil.

09-00:00:04

Smelser: Good morning.

09-00:00:05

Rubens: Today is the twenty-ninth of March. This is our fifth session. I had one leftover from our session last week. We didn't talk about you being an advisor to *The American Journal of Sociology*.

09-00:00:20

Smelser: Yes. Not long after I arrived in Berkeley—1959, it was—I was asked to join the editorial board of *The American Journal of Sociology*, which is one of the two leading journals in the field. It comes out of Chicago and it's published by the University of Chicago. This is one of these precocious things. I was really known only for my dissertation. It was published by the University of Chicago Press, so there was some evidence of my presence there. The editorial board is a group of a dozen people, perhaps, that they put on and send out articles to, in your areas of expertise. This was just before my first publication, and really only one year after my Ph.D. So it was, as I say, a kind of precocious thing. I served reading, periodically—I couldn't say how many, but a significant number of articles for them, offering my opinion and advice, and went back to the editor-in-chief for work. I enjoyed that. I found myself always comfortable with doing critical reviews and critical analysis of other's works, so it wasn't a tremendous labor or learning experience for me. But that lasted only two years. I was, I think, a three-year appointment, renewable, but it lasted only two years because I was appointed editor of the *American Sociological Review*. I said, enough, and resigned.

09-00:01:52

Rubens: Sure. You reflected last time that you weren't exactly sure why a young professor just beginning to publish would take on the editorship of the journal.

09-00:02:04

Smelser: Yes. It was not from a kind of ideal-type point of view. It was not the rational choice for a person that young to get into heavy editorial duties, because, simply, from the standpoint of the kinds of criteria that academic review committees would have in mind, editing is not the top priority. Creative scholarship is a top priority, and this, of course, was a tremendously demanding job. I can't say how many hours a week I worked at it. Maybe twenty, twenty-five. Of course, that added on to everything else I was doing. I was already being recommended for tenure very early in my time here. I knew I was in good standing. I suppose, as I said previously, that played something of a role in my decision to do it.

09-00:02:57

Rubens:

I wonder if you felt a bit seasoned, or had you become comfortable in liking—

09-00:03:05

Smelser:

I felt confident in my own intellectual ability and confident in my ability to do the editorial work. I wasn't hesitant on those grounds. In other words, I wasn't scared of the job. I was just wondering whether or not it was the right time to put in that much work on editorial chores.

09-00:03:25

Rubens:

The question was really, did being an advisor to *The American Journal*, did that season you? Did that give you a sense of—

09-00:03:30

Smelser:

Oh, yes, it gave me some training. Yes, it had to be. It was a direct line, because I was in the business of evaluating and making decisions on manuscripts.

09-00:03:39

Rubens:

Sure, and a wider field than you probably had read previously.

09-00:03:43

Smelser:

Yes.

09-00:03:44

Rubens:

Let's turn to the research that you were doing.

09-00:03:55

Smelser:

Yes. I came to Berkeley having finished my doctoral dissertation. It was right in the process of being accepted by the University of Chicago Press and by Routledge & Kegan Paul, so I was confident that that was behind me and it was being published and would be out and received. So the question was, to this young scholar arriving on the scene, what was I going to do next? I actually had made my mind up before arriving that I was going to try a major treatise on social movements and collective behavior. There were three sets of intellectual influences on me, I would say, that led me to choose this. The first was a most inspiring exposure to the field from Gordon Allport, the social psychologist, when I was a freshman at Harvard. I was absolutely transfixed by his analysis of rumor, of which he was the leading scholar in the country at the time. He played the Orson Welles broadcast in 1939 that led to the panic, "Invasion from Mars." He played that in the class, and we analyzed it and so on. I was just totally taken in by these episodes and his analysis of them. I'd say that was the original influence, though I didn't say, "I'm going to write a book on that" at that time. I was only a freshman.

Then, however, when I was working on my doctoral dissertation and the British labor movements, labor protests and movements were the most engaging part of that research. I thought my interpretation of them was the most exciting and probably the most original. So I was already primed. Then in the meantime, I had made contact with a scholar, an economist at Harvard

in my graduate year. His name was Alfred Conrad. He was the husband at the time of Adrienne Cecile Rich, the poet. We became friends with them, in a kind of a couples' way. He was a kind of economic historian, and he introduced me to a certain way of thinking in economics called value added, which simply refers to the accumulation of inputs into products as they're being manufactured. You can treat each phase as a kind of causal moment in the production of some kind of process. I then began to think of it in a more analytic way, more abstract way, as a way of organizing any causes of social events. This was what became, ultimately, in that book, what is now always referred to in texts that summarize it and so on as the value added theory. What it really means is that I broke down the causes of these different kinds of collective episodes, panics, riots, different kinds of social movements, the development of religious cults, everything the book covered. I developed a way of dealing with multiple causation. All of these are multiply caused. What I did was to frame causes of an extremely general and permissive character that will permit certain types of behavior to occur. For example, if you have a system of property, in which the only way that that property can be disposed of is to pass it on to an heir, it's a very un-fluid situation. There's no possibility of panic. No possibility of rapid disposal. No possibility for rapid movement under those structural circumstances. However, if you have a market—and I use this as an extreme contrast, the stock market, in which you can dispose of everything right away—that means it's a structurally permissive or conducive condition for something like panics to occur. They don't occur everywhere in society. They occur only where they're possible. So that was the most general. But then within that, you get all kinds of other accumulating factors, and so it was a funneling effect that I was trying to describe in terms of causes that operated within the conditions established by prior causes. I treated the analysis of panic and the analysis of hostile outburst and the analysis of crazes, the analysis of social movements, revolutions and so on, all within this common framework that was a way of organizing causes which had not been put together at different levels before.

Those were the three—as you might say, the intellectual bases for it. I also was dissatisfied with the state of the field of the study of collective behavior. I thought it was much too psychological. It started out by Gustave Le Bon, in the late nineteenth century, in his study of the crowd, which he treated as psychopathological expression of baser human instincts. That dominated the field for a long time. The irrationality was sort of being given up by, I'd say, more sensible scholars, like Herbert Blumer, but the psychological emphasis was still there. I wanted to develop a genuinely social psychological-sociological theory of the development of these classes of behavior. So that was my main polemic, and there's an irony that came out later in the reactions to my own book that I'll talk about. This was the kind of intellectual frame with which I began my work. I began it aggressively from the minute I arrived here on the campus. It was in progress for two or two-and-a-half years, and it was what I was working on when I went into the Theory Center that I mentioned last time.

09-00:10:02

Rubens:

And you talked about Blumer reading the chapters as well.

09-00:10:05

Smelser:

Blumer was one of my critics. I covered that in my relations with him.

09-00:10:10

McIntosh:

Can I ask a question about the *Theory of Collective Behavior*? You just mentioned the tradition that goes from Le Bon through Freud, E.A. Ross, and McDougal, people like that. But there's another tradition, or another sort of milieu, at this point in American sociology that's really focusing on communication as being an important focus point. I wanted to ask you if that was anything that you were responding to. I'm thinking, for instance, of people like Harold Lasswell, who was emphasizing propaganda as a method for social control because of communication being at the forefront of his mind. Was the *Theory of Collective Behavior* at all responding to that?

09-00:10:59

Smelser:

No. At a more macro level—that's moving toward the more macro level—I was more influenced not by the propaganda analysis and the communication analysis, even though I was much aware of it. I was really influenced much more by the classics. By Marx, by Tocqueville, who themselves were interested in revolutions, because I was going all the way up to the macro. Now, I got into the issue of social control as one of the determinants that was in my theory of value added. That is to say, how authorities react once a disturbance of this sort appears is very important in the directions that disturbance is going to take. But I didn't get into propaganda and handling and manipulation of information so much as the political disposition of authorities. So I'd have to answer your question, no, it was not a significant influence in my thinking at the time, even though I was aware of it.

09-00:12:00

McIntosh:

As another follow-up question on the issue of social control, it struck me, reading *Theory of Collective Behavior* again, that social control is sort of the last phase. I don't know if this was intentional on your part or just one of the outcomes of the development of the theory, but the book argues that you can't understand social control until you understand the other phases. Was that—

09-00:12:33

Smelser:

I'll put it a little differently. Social control comes into operation once some kind of movement appears and begins to be developing. That's why I treated it at the end. It's a response to an already accumulating and developing phenomenon. It overlaps—I subsequently reflected—it overlaps very much with conduciveness at a later stage, because one of the things that social control does is either crack down on one form of expression or permit other forms of expression or strategies that the authorities can foul it up and send it in a new direction, perhaps more extreme direction, so on and so forth. So I try to distinguish between the broad structural permissive features of the society and the social structure on the one hand. Then, on the other, once the

thing has accumulated into some kind of a delivery of movement on the part of people who are either protesting or calling for reform or revolting or something, then the police, the authorities, the politicians, the upper classes, their behavior becomes a very important determinant as to which direction this is going to take.

09-00:13:48

McIntosh:

Another development in American sociology, at least in my understanding, in the twenties, thirties, and forties, is the emphasis on small groups. I was hoping maybe you could just talk a little bit for the record on where you saw this work as speaking to that.

09-00:14:07

Smelser:

To my own work?

09-00:14:08

McIntosh:

Well, where you saw the theory of collective behavior within that tradition. Are you dealing with small groups or are you dealing with something—

09-00:14:14

Smelser:

No. I would say that was not a major informing framework for my work. A lot of it had to do with the effective performance of groups. That was a big thread, which I wasn't especially interested in this particular work. A lot of the work had to do with worker satisfaction. I was fascinated by the emergence of the efforts to identify the importance of informal groups and organizations. That's always been a kind of fascination of mine, and I've, in my most recent work, returned to that. But I would have to say that the small group perspective was not a significant element in my own thinking at the time.

09-00:15:08

Rubens:

Is it logical, then, that we talk about the reception of that book?

09-00:15:11

Smelser:

Yes, I'd like to talk a little bit about that, because it was a complex reception. It attracted a lot of attention. The editors of the major sociological journals sent it out to the major people in the field. The early bout of reviews, by Ralph Turner from UCLA and Lewis Killian from Florida State, both of whom were experts in the field, was to recognize the book. They'd both described it as being a kind of major book that was going to last a long time. Even the first initial reviews were enthusiastic. They had criticisms. Ralph Turner, for example was, more than anything else, a student of Herbert Blumer. He had inherited his perspective, and he was very much interested in that I didn't spend enough time on process and circulation of meaning and so on—the informal aspects that of course his own work stressed more than anything else. But he described it as it's a really major theoretical contribution. It immediately began to be a subject of some interest in different sessions in the American Sociological Association meetings. I appeared a few times in the following years, along with leaders in the field, like James Coleman. Were on the panels discussing it, and so on.

It had an early and visible reaction, but what I would like to say is that that reaction was mainly in the context of, here's an effort on the part of a social scientist to develop a new theory of this kind of behavior, and it's very exciting and it raises a lot of questions, it raises a lot of criticisms. But they were doing it within the framework of analysis—what kind of analysis I had done. Collective behavior was not a very strong field analytically at that time, and neither was the study of social movements. This was an effort to formalize it, to give it more beef, to bring it into more of the mainstream sociological analysis. That's the way it was treated and reacted to, which gratified me, because that's the spirit in which I wrote it. Now, after 1965 and into the seventies, when there was an internal revolution in sociology itself, and particularly a virulent and prolonged attack that more or less dethroned Parsons as the leading theorist, and his type of theory, functionalism, my book got picked up in the middle of that revolution. The thing they really picked up was this issue of social control. I began being criticized from the left as being managerial-minded. How do you handle these things? That I was an apologist for the system and that this was a handbook for the police, for handling riots and that sort of thing.

So I got pushed way over to the right, as far as this dominant reaction in the field was concerned. They put me back in the Le Bon tradition of treating these people who were involved in these as irrational. I never use the word "irrational" in the book. I talked about the nature of the beliefs and compared the structure of many beliefs to magical beliefs, for sure. There was one unfortunate phrase in the book—as it turned out, unfortunate. I described collective behavior as “the action of the impatient,” meaning they saw the world as being changeable and moving and they wanted to do it right away and so on. This got picked up as a condemnation of these movements as irrational. So I got pushed over in the irrational direction, which was also not my intent at all. I treated the behavior as purposive but having a guiding kind of ideology or belief system that I analyzed in one of the chapters for all these different types of movements, but I surely did not take the viewpoint of the authorities that these were things to be crushed, or that they were irrational. But that's the way the whole thing was treated, and much to my chagrin, really, because I didn't really see that as consistent with my effort, which I saw as neutral and analytic. But nonetheless, these larger political movements swallow everything up. I think in retrospect, it was very understandable in the context of that ideological ferment that my book would have been treated in that way.

I got stereotyped, actually. It's kind of emerged from that recently as the field has gone away from this particular ideological commitment. There's been something of a rebirth of interest as people have turned more to the study of beliefs and framing and the emotional sides of these movements. Once again, there was an article in the *International Encyclopedia of the Social and Behavioral Sciences* that describes this course of events, beginning with my work and then the series of intellectual developments that began treating

social movements as more rational and more purposive and more creative and to be identified with positively. Now there's a movement back towards some of the kinds of emphases that I chose. The author of this Encyclopedia article called this movement "Smelser's revenge," meaning that the themes that I had picked out were now returning as major threads in study.

09-00:21:20

Rubens:

In the seventies and on into the eighties—were there critiques of trying to get too general of a theory, in looking at social action?

09-00:21:35

Smelser:

That wasn't one of the criticisms, that it was too abstract and too general. I think the criticism was that it was wrongheaded, and it was just a continuation of this functionalist interest in social control and steadiness and the restoration of stability and so on.

09-00:21:52

Rubens:

Okay. Not that you were attempting to get a grand theory?

09-00:21:55

Smelser:

No, no. That attack on Parsonian thinking was never made on me. I used the framework of some of the theory of Parsonian thinking in framing that book, but it was kind of attacked on its own. As part of the functionalist tradition, it was attacked, but the idea that it was too abstract and too removed from reality never came up as a line of criticism, the way it did so heavily with Parsons.

09-00:22:29

McIntosh:

One thing that's interesting to me in the reception is that you were heaped into the Le Bon school of treating emotions as irrational and things like that, whereas, if anything, I saw the book as saying that, as a result of social strains, these emotions are sort of rational reactions.

09-00:22:48

Smelser:

They're reasonable. Understandable reactions.

09-00:22:54

McIntosh:

Yes. And so if anything, I saw it on the other side of the spectrum, which is that, if there is a critique to be leveled, it is that you're taking away the personal validity of these emotions, because you're saying that they're just responses to social conditions.

09-00:23:15

Smelser:

Well, I think maybe that might be an extension of the sort of critique that Blumer brought. He said these people are being pushed around by the system, right? He didn't like that at all. That's what he kept returning to all the time. It opened up the structure-agency issue, though it wasn't framed in that way at the time. The big irony, of course, is that my own mission was to make the study of social movements more sociological, and these later reactions turned it back into psychological direction, which I thought was just out of keeping with my own purpose.

09-00:23:50

McIntosh:

Looking back, do you think that any study from this period that used the term “social strain” and that focused on reactions to social strains was destined to receive the critical opposition that *Theory of Collective Behavior* received?

09-00:24:00

Smelser:

It was very widespread. The word "strain" disappeared out of the common discourse. Other words—contradiction, whatever—showed up as, in a way, you might say equivalent to it. But that particular line of analysis, which was associated with Parsonian language, was kind of—that's why people stopped using it, really, as a particular analytic category.

09-00:24:30

McIntosh:

U.S. intellectual historians like to turn to figures, like Mills or the presence of the Frankfurt School in the U.S., as being some of the sources of this reaction against the functionalist paradigm. Are those the figures that you would cite yourself as leading the charge?

09-00:24:50

Smelser:

Mills was a really major figure in terms of the articulation of an approach that young people took. There were various European scholars who were on the new left, and Britain was another source of influence on this. Barrington Moore was one of the major critics. Ralf Dahrendorf in Germany. It was sort of a rebirth of a Marxian-Weberian world view. Radical sociology really was closer to Marx itself, but these other elements were kind of derived from a wider range of social theory, but stressing above all domination and oppression.

09-00:25:41

Rubens:

What about E.P. Thompson? *The Making of the Working Class* came out which takes aim at you.

09-00:25:48

Smelser:

Yes. E.P. Thompson wrote the book called *The Making of the Working Class*. I think it came out in the mid-sixties, if I'm not mistaken. He, in his first chapter in that book, deals with an assault on two theories of the British Industrial Revolution—or of capitalism, really. Mine and Ralf Dahrendorf. Dahrendorf and I were looked upon as being, in a way, very opposite in our views, but he picked us both up as being mistaken. He picked up that left wing thread that appeared in the critical reviews, namely that I was treating the British working class as an irrational historical force. His own thing was the making of the working class. This raised the agency question directly, saying they made their own history. They were not misguided. They were, in fact, enlightened. That was the whole thread of his book, was to raise the whole thread of autonomy and independence and creativity on the part of the workers themselves in the making of their own history. Interestingly, he cited my book throughout, from the standpoint of the evidence that I had gathered. The book is full of footnotes and references to me with respect to this, that and

the other historical situation. The initial chapter is a very vigorous polemical attack on my work.

09-00:27:21

Rubens: I thought it was important to include, because he's such a darling of—

09-00:27:25

Smelser: I guess you'd call that one of the reactions.

09-00:27:31

McIntosh: Clearly, *Theory of Collective Behavior* is a break from what you wrote on the British Industrial Revolution, but I wanted to know if you saw any of that work as informing *Theory of Collective Behavior*?

09-00:27:49

Smelser: Well, yes. I was always, in the back of my mind, having this in-depth study I'd done of protest over a period of fifty or sixty years in Britain, right from the early part of the nineteenth century up through the Chartists. It was always in the background of my mind because I knew it, but I did extensive reading in wider historical sources on Nazi Germany, on Italian Fascism, on the French Revolution. I included a great deal of scholarship on these in my last chapter on value-oriented movements, which included the big political revolutions of the time. I guess I've answered your question. It was in my mind, but I spread out so much more that it could hardly be said to be the determining model that I had in mind.

09-00:28:45

McIntosh: It was interesting to me to see how well-received it was among historians, actually. I read a review in the journal *History and Theory* of it that was—

09-00:28:52

Smelser: You mean my thesis?

09-00:28:53

McIntosh: No, sorry, on *Theory of Collective Behavior*, which is sort of more of a theoretical work. There's an eight-page review in *History and Theory* that said this is really a chance for a bridge between history and sociology as well.

09-00:29:09

Smelser: I'm not certain I remember that article. I'm happy to hear about it. My work has always been quite highly respected by historians. That was a bit of a surprise to me, I think I mentioned last time, that the British historians turned out to be so interested and so complimentary to it, because I was an outsider in many respects to the interpretation of British history. I appeared on a couple of forums on sociology and history in its general terms. The *Journal of Labor History*, I contributed a brief article to, which was a reflection on my interpretation. When the department here was having all this turmoil, one of the departments that turned to me to ask if I might like to join it—get out of sociology—was the history department.

09-00:30:08

Rubens: Oh, really?

09-00:30:08

Smelser: Yes. History, political science, and psychology were the three places that showed some interest. If I were in a mind to move, as Bendix and Selznick and Wilesnky and Swanson moved out of the department, there was some thought that I might be alienated as well. Which I sort of was, but I wasn't alienated enough to want to change my departmental affiliation. But those were the departments that had shown an interest in my work. I was on a lot of orals examinations in the history department. Right from the very beginning, I was an outside member on orals and thesis committees in history.

09-00:30:52

McIntosh: You mentioned political science. I was wondering how the book was received by political scientists.

09-00:30:58

Smelser: I can't tell you. I do not have a crystallized view of that. Obviously, the study of social movements became much more salient in political science after that time, but I cannot assess any influence I might have had on it. A lot of interest went in the direction of rational choice later and so on. I can't give you a responsible diagnosis of this question.

09-00:31:31

Rubens: Now, do we want to talk about the theory of economic development?

09-00:31:35

Smelser: I'll mention that the work I did that came out of my dissertation. It was a study of economic development in many respects, but it was a historical study. I had mentioned the development in literature was beginning to crystallize after World War Two. The great concern with the developing countries and with their joining the modern world, and it was a heyday of modernization theory, beginning around 1950. Coincided with the death of colonial empires and with the aspirations of the new nations that were emerging from the colonial collapse. Of course, economists were deeply involved in the economic development literature.

As I discussed previously, I joined a faculty group at the invitation of Lipset; it was made up mostly of economists and political scientists and sociologists, through the Institute of Industrial Relations. It was on the subject of economic development, and I was asked because I had done this historical work. So we met on a monthly basis. We wrote papers for each other and so on and so forth. In the meantime, Wilbert Moore—I mentioned him as one of my theoretical kinsmen, I guess you'd have to say—through the Committee on Economic Growth, which I was serving with in the SSRC at the time—there's a major conference the SSRC put together on industrialization. They invited me to come and give a general view of the structural and sociological aspects of development. These were the things that fed into it, my work here and on

this Committee on Economic Growth and with the SSRC. So I put together an essay. It came out under one title, but the major title that it later assumed when it was reprinted—it was reprinted a lot—it was called "Towards a Theory of Modernization." It was a general statement of what are the structural changes that really go on in society when modernization takes place. Modernization as industrialization, as urbanization, and so on. What are the major institutional changes? It was an abstract and general statement, and got a lot of attention. It was translated into many, many, many languages and reprinted a lot of times. I became one of those theorists, even though it was only based on this one article, one of the theorists of modernization. Taking it not from the economic point of view but from the changes in stratification, in family, in political structure, and so on, that typically go along with modernization. It also fell under the attention of those in the 1970s, particularly dependency theorists, as being a wrongheaded view of the nature of development as well.

09-00:34:42
McIntosh:

Just for the record, what were some of the basic concepts that you were using in order to create your theory of modernization? I know differentiation—

09-00:34:50
Smelser:

Differentiation was a big one. Unlike many modernization theorists, who saw this as a kind of ongoing process in which everything changes at once, I said the two main processes that are going on in modernization are the differentiation of social structures from one another, and I included education and family and religion and the major institutional complexes as well as stratification. I said differentiation is the main story, but at the same time, it is irregular. There are leads and lags and tensions and distortions of life in many quarters of society, so the second big contingency in modernization is a redefinition of what the integrative aspects are in society because of all the disturbance that's created by these lead/lag irregularities. So it was this articulation between differentiation and integration and disturbances that constituted the main analytic elements of that statement in that particular essay. So it broke from some of the modernization emphasis of being a steady, smooth process in which everything changes kind of more or less at once. I saw it as a much more fraught process leading to a lot more instability than the other modernization theorists were emphasizing at the time.

09-00:36:16
McIntosh:

That explanation caused me to see some connections with Gerschenkron. I don't know how to pronounce his name correctly. You mentioned that you all had a relationship way back in the Society of Fellows days. Was his work on your radar screen at all during this time?

09-00:36:38
Smelser:

I knew about it. His main contribution to the study of modernization appeared in an essay called "Economics of Backward Society" or something of the sort, in which he stressed a lot more the variability of the modernization process,

particularly with the role of government and banking, in addition to entrepreneurs and other processes. I read Gerschenkron and he influenced me. I was particularly interested in his interpretation of the politics of development more than anything else in his work. Gerschenkron, I think I mentioned in my earlier remarks, wasn't very friendly toward the kind of work I was doing. He didn't like Parsons at all, and he got into big fights with David Landes, for example, who was friendly to the Parsonian cultural approach. Gerschenkron would always tease me a lot when he would come to Society of Fellows. He used to say, "You're a smart young man, but you're misguided." Sort of teasing that he would give me all the time.

09-00:37:42

Rubens:

The political science department here at Berkeley had great admirers and promoters of modernization theory. I wonder how they responded to—

09-00:37:53

Smelser:

Well, I had a close relationship with David Apter. He was a student of African societies. As a matter of fact, he turned out to be a big sponsor of mine. He ran a Peace Corps class here in the late fifties, early sixties. He always had me around to lecture on modernization to Indonesian and other scholars who had gathered on the Berkley campus to come and study social science here as a basis of their—they returned to their own countries. So I was involved in that, and Apter was my main supporter. Apter had got me involved in a group called Theory and Method of Comparative Studies in the Institute of International Studies. He asked me to do this. It was handsomely supported by a Ford Foundation grant. I think it was '66, maybe. Then when I was being courted by Harvard in 1970, he was the one who set in motion the possibility that I could join the Institute of International Studies on a half-time basis, meaning that for the rest of my career, my teaching would be half-time, as of 1970 on. This was a great plum that the university could offer you at the time. Can't do it anymore. From that point on, I was half-time research professor, being funded by monies out of the Institute of International Studies, and teaching only half time. It was a response to Harvard and a result, in large part, of Apter's saying, "I'm willing to do this. He should stay here. We want him in the institute on this basis." He organized the financing of that—

09-00:39:58

Rubens:

But initially, based on Apter's appreciation of the work that you were doing?

09-00:40:02

Smelser:

Yes, he was a fan of mine in terms of my scholarship. He thought I was one of the people that would be important in his particular area of the field. Apter's an entrepreneur. He was an academic entrepreneur and he was always arranging things for people and himself. He ultimately went to Yale, I think in the early seventies, if I'm not mistaken. But nonetheless, I am very glad that came up, because I had neglected to say anything earlier about my relations with Apter.

09-00:40:33

Rubens:

Good, good. You mentioned last week that the economics department had sent a delegation to challenge your—I wonder how they responded to the book.

09-00:40:44

Smelser:

Well, they were selected economists who liked my work. Gregory Grossman, who was the comparative economic historian, I guess you'd call him, who was active for his whole career in the economics department, really liked my book on economic sociology. He went out of his way to compliment me on it and to, I think, assign it in his own courses on comparative economic structure. Walter Galenson, who was an institutional economist who was here for quite a few years before he went to Cornell, was also a fan of my work. Interestingly, so was the man Papandreou. He later became the Prime Minister of Greece. His whole family ran Greece for about—I don't know how many decades. He was chairman of the economics department at the time, and he took an interest in my work. That episode where they didn't want me to call my course Economic Sociology was just a little blip. In general, I got on pretty well with some economists. Most of the economists weren't interested in my type of work. They were in their own technical fields. It didn't make any impact on their thinking. They probably didn't even notice it.

09-00:42:04

McIntosh:

Before moving on entirely, I just wanted to talk a little bit more about this article, if that's okay.

09-00:42:09

Smelser:

Yes.

09-00:42:11

McIntosh:

First, there's a lot of literature being produced now that's trying to understand how, if at all, modernization theorists, or people who are working in the field, actually grappled with practical issues of developing nations abroad and policy issues. After publishing this article, were you offered any practical projects to work on by either the university or foundations, like the Ford Foundation, or the government?

09-00:42:44

Smelser:

Not beyond my continuing work with the Social Science Research Council. The work that came my way, of that character, was more in response to collective behavior. This was the era of civil defense. A lot of people in the government were interested in reactions to disaster and threat. Of course, I put the reactions to disaster kind of front and center in my work on panic and scapegoating and different short-term responses. This got the interest of some people in what became DARPA, the research arm of the defense department. I was commissioned to write a major article on reactions to—it was originally going to be reactions to disaster, but they really wanted to make it a much larger working essay that I was not really intending to publish, to be submitted to them. Got an honorarium for it and said a lot about the possible reactions—it was an applied essay, in part—reactions to atomic attack and issue of mass

panic and that sort of thing. But I also included a much larger framework. It was a general study of all kinds of social change, and it appeared in that book of essays in 1968, called *Essays in Sociological Explanation*, under the title, "Toward a General Theory of Social Change." It was altered. I didn't use the same document that I sent in to the government. It was an outcome of further work I had done based on that study. But on development in general, no, I was not called in as consultant.

09-00:44:57

McIntosh:

But did it put you in dialogue with other people who were working in a similar field? Or are you now in dialogue with Rostow again or people like that?

09-00:44:08

Smelser:

I met people casually. The article was highly noticed, and people would talk to me about it. I talked about my linkages here on the Berkeley campus with economists and economic historians and so on. The linkages came out of that special interest in modernization, and I certainly didn't consider myself a major theorist. This is just one little essay I had written. It had a lot of impact, but I didn't really see myself in that movement. I differentiated myself from people like Rostow and Lerner, who I thought had much too kind of a mechanical view of what modernization was like. I was standing in the middle of these people who were talking about this general process of modernization that's going to take over the world. Then on the other side, I got criticism from Clifford Geertz in the anthropology department, saying, "You're talking at a much too general level." He was interested in all the cultural idiosyncrasies that were going on all the time. My dialogue with Cliff—he came from the other side. He thought it was too general, and the general modernization theorists who were writing at the time would have that—I never really argued with them—that it was too contingent. So I was in the middle again.

09-00:46:26

McIntosh:

I'm sorry to ask you so many questions about this one little article.

09-00:46:28

Smelser:

No problem. It was regarded as fairly important.

09-00:46:34

McIntosh:

No more questions about that specifically. I guess a connection between that article and *The Sociology of Economic Life* would be—

09-00:46:47

Smelser:

That article appeared, in highly modified form, as the last chapter of *Sociology of Economic Life*, on processes of growth and change.

09-00:46:57

Rubens:

Okay. Do you think it's—

09-00:47:01

Smelser:

Enough on the intellectual side of my life at that time? Yes.

09-00:47:07

Rubens: Now to the personal side. What was noticeable is that, in the collective behavior book, that you dedicated it to your wife.

09-00:47:18

Smelser: Yes. That was in 1961, when my marriage was still intact. Tensions already, of course. But it was a meant dedication, for which I became embarrassed later, of course, when the marriage ended. It seemed kind of silly or ill-advised.

09-00:47:43

Rubens: The times had changed.

09-00:47:45

Smelser: My times changed, yes. That book was dedicated to her, and I sort of vowed to never dedicate a book to anyone else. Though my most recent book, on the odyssey, I dedicated to Sharin. It took me forty years to get over that error. [laughter] My marriage—I can't date when it began to unravel, but it was about two years before the separation, which was in the spring of '63. I will not go into the details of the conflict or anything, but it became extremely taxing. Very full of conflict and very personally demoralizing and depressing for a long period of time, until it was in the spring of 1964 that we separated. It was that late. So there was a significant period of turbulence and rescue activity and conflict that preceded the separation. It was very unpleasant. Certainly with two young children, I was extremely guilty about that aspect of it, all through the conflict and the separation.

09-00:49:01

Rubens: Was it the occasion of the conflict that you turned to the Psychoanalytic Institute?

09-00:49:07

Smelser: Yes. I of course had this great background in psychoanalysis. It was a big part of my personality, and I sort of had a kind of gleam in my eye that maybe one day I would go in, but it was quite obviously the personal disorientation and disappointment and bitterness and conflict and depression that went along with the end of the marriage that led me to apply to go into the Psychoanalytic Institute. Now, that's an interesting aspect of this. Almost everybody who gets into this kind of psychological distress will just simply go into treatment and try to bail yourself out through help. That, of course, was a very big part of that motivation. But at the same time, I had had this long intellectual interest in psychoanalysis. I decided that I wanted to do more than just get personal help. I wanted to go into the psychoanalytic training program that was available for non-physicians at the time. That arrangement had evolved over time. All my mentors at Harvard who went into the Psychoanalytic Institute in Boston couldn't do anything but get psychoanalyzed and go into courses. They couldn't practice one bit. But gradually, the psychoanalytic institutes began to open themselves up more to lay participation. Step by step by step, they

permitted more practice, more actual psychoanalytic practice, to enter into the training programs.

When I entered the Psychoanalytic Institute, after a series of interviews with them—I applied and I was interviewed by four or five analysts, and admitted after sort of a—I guess it was not an arduous test, but they wanted to make sure they were getting—I was the only non-doctor, except for one other person at the time, so they were beginning to open their doors to non-medical people. I had to sign a document when I was accepted, that while my training was going to be no different from the medical candidates, I had to pledge that I would never practice afterwards. This was still a restriction. It was a guild issue for the American Medical Association, that control of the supply of psychoanalysts. By the time I graduated, however, they had lifted that. So I was not only permitted to practice if I wished, but also, in a move that was done by the California legislature, I became certified to practice by the state of California. They added to the list of people who were certified to practice: non-medical graduates of psychoanalytic institutes. I actually could have taken up psychoanalytic practice if I wanted to afterwards.

Just about the time I separated, my analysis began. Once I was admitted, I went around and interviewed all the training analysts. You got analyzed only by training analysts in the institute, not by any old person. There were four training analysts in the East Bay—I didn't want to travel to San Francisco for my analysis—four in the East Bay. I interviewed all of them, and in the end was taken on by Stanley Goodman, a training analyst in the institute. Practices right here in Berkeley.

09-00:52:50
Rubens:

Was this expensive?

09-00:52:54
Smelser:

It was expensive at the time, particularly since there's always a burden that goes along with getting divorced. In the divorce processes, I was to pay a fairly large amount of child support and a very small amount of alimony. That amounted at the time to \$625 a month. That was not a very large sum by contemporary standards, but it was a dent in my income at the time. Then the analytic fees—my training analysis, I had to pay for it. It was not subsidized. I didn't have a grant or anything. My fees were \$25 an hour, and it's four times a week. That added up to be a considerable expense, even though it was tax-deductible because it was part of my further education. It was done on an educational advancement. I had a couple of years of—I wouldn't call it hardship, because I never went hungry, but I had years of stringency. I borrowed some money from the university. They had a kind of emergency loan of a couple thousand dollars that I paid back within a year. These things were coming together at that time, between '63 and '65, I felt it.

09-00:54:18
Rubens:

Sure. Had *Theory of Collective Behavior* started to—

09-00:54:23

Smelser:

Royalties were coming in. The main thing that eased my situation was my rapid advance in the ranks of the faculty. Every time I got promoted, my salary went up and up, so that's what wiped out the hardship, was the increase in income. Mainly. Royalties were part of it, but they were never the biggest part. The big thing was my salary

09-00:54:45

Rubens:

And how did you fit it into your work schedule? Was that part of your morning ritual?

09-00:54:55

Smelser:

The analysis, I went whenever it was scheduled. Four days a week, Monday through Thursday. And then seminars began, more or less immediately, and they took place on Friday afternoons and Saturdays. There were maybe five or six hours of seminars a week that went along with it.

09-00:55:13

Rubens:

How many people were in those seminars?

09-00:55:15

Smelser:

My group? Five people. I was in a class of five. Four doctors and myself. We formed a tight little group at that time. The analysis, I just went to when it happened. I couldn't schedule it at the time of my classes, but my schedule was otherwise quite flexible, and I would go when I had to. It was only a five-minute drive to the analytic office. I'd run over to San Francisco. But in the meantime, when I separated, I wanted very much to continue a full as possible relationship with my children. I worked out the schedule that I would pick them up every Wednesday afternoon. They'd stay overnight with me, and then I would take them to school in San Francisco, where my former wife moved. Then I would pick them up on Saturday morning, usually after a seminar at the institute, and take them to school Monday morning. So I had a Wednesday and weekends consistently, which I managed to keep very faithfully, even during the very rugged period of 1965, when I got into the administration.

09-00:56:36

Rubens:

The actual separation was in '64, so you weren't having to do that while you were traveling east for the SSRC or the—

09-00:56:43

Smelser:

Once the analysis started, that's a very inhibiting thing on your travel. Most of my meetings on the East Coast were on Friday, Saturday, or Sunday, with the American Sociological Association, where I was on the governing board. There were always weekend meetings. I managed to not miss very much of either the routine that I had set up with my children, and it didn't interrupt the psychoanalytic schedule significantly. So it was squeezing things in.

09-00:57:21

McIntosh:

Can you reflect a little bit on how the psychoanalysis just changed you? Both intellectually, in terms of the concepts, but also in terms of your interpersonal relationships?

09-00:57:34

Smelser:

Well, yes. I didn't know at the time, but psychoanalysis is very much beyond your own reflection at the time it's happening. You're just kind of going through it, and you're going from meeting to meeting. It has a certain chaotic character because you are free associating and you go from subject to subject or feeling to feeling that you didn't think were related to one another. You're sort of learning—disturbing learning process, because you'll find out a lot of things about yourself you had not acknowledged before, and a lot of these things are ugly. It's a disturbing experience, particularly in the early parts of it. It's not that you go in and you start getting better. It's a very chaotic, up and down, irregular process, and you go through moments of enthusiasm and moments of despair all the time. That's a bit of the process, that I'm discovering things that I didn't know about myself. There's a lot of analyses and therapy that lead to a lot of somewhat crazy acting-out outside of the treatment. Very fortunately—I didn't plan this out—but I didn't get into a lot of crazy behavior that many people who go into training, or go into analysis, get into. The routines of my work life didn't seem to be disturbed. That was really not the major source of conflict and perplexity in my life at the time. I obviously spent a lot of time on things that were related to my work. My perception of my father and my relations with Parsons played a very big role in my own analysis and what I was doing and how that reverberated in my life and a lot of other relations with colleagues and so on. It touched on my work but never disturbed my work. I kept a steady flow. I almost had to, given the number of involvements I was in. But that could have fallen apart. It's altogether possible that could have fallen apart, but it just didn't happen to.

[Begin Audio File 10]

10-00:00:4

Smelser:

When I went into psychoanalysis, I had absolutely almost zero support from my colleagues at the university. My department, in particular. There were some individual friends who encouraged me with it, but my colleagues in sociology had absolutely no idea that I should be doing this at all. No support. First of all, it was another distraction from my career. This psychoanalytic process took twenty hours a week, given the analysis and given the classes. Then when I took on my own patients about a year after I started my own analysis, then that added up to twenty hours a week. That's a very big commitment. So my colleagues thought I was going in the wrong direction. They weren't particularly sympathetic to psychoanalysis as an intellectual endeavor, and they said, what are you wasting your time for? I felt pretty solo in what I was doing, from the standpoint of my colleagues. However, from the standpoint of my relations with my colleagues, I felt a lot more comfortable. Whatever competitive feelings I had, or any—I'll call them competitive in

other aspects—kind of got more under—I didn't worry about it. My collegial relationships improved a lot. I never really had terrific hang-ups about the authority in the university, but I was kind of more easygoing with any kind of authority relationship, like deans or others that I may have had something to do with. Of course, my relations with women were revolutionized. It was the core of my problem in my marriage, of course. Of course, my relations with women the rest of my life have been really vastly improved, and I give the majority of credit to what I was able to figure out and how I was able to change during the psychoanalytic experience.

10-00:02:12

Rubens:

You had no inhibition about telling people this is what you were doing?

10-00:02:18

Smelser:

No, I didn't keep it a secret that I was in the Psychoanalytic Institute. There was an intellectual side of that, too. In these seminars, I read practically everything Freud ever wrote in the course of this training, as well as all kinds of contemporary—I became a really expert in the field because of the seminars and my own intellectual interest. I found with my medical colleagues in the institute that I was much more theoretically and analytically prepared to think about the intellectual subject matter than they were. I have this theory that medical schools kill people, from the standpoint of intellectual and analytical ability. They get into these clinical cases and don't get out. I became a kind of intellectual leader among my colleagues over there, because I had a much more sophisticated sense of analysis and theory than any of them did. It was just easy for me to enter into that dimension of it. That's kind of an aside there.

I was sufficiently taken and committed to the psychoanalytic perspective that, from time to time throughout my life, it has influenced me in what I've written. One of the essays in that book, *Essays in Sociological Explanation* that came out in 1968 was a turn to psychoanalysis, to say what can it contribute to the study of collective behavior? I didn't treat that very extensively in the book. I referred to Freud and other strands, but I didn't really do it deeply. Then I went much more deeply into the personal and in-depth dimensions that were influenced, obviously, by my—I wrote a couple of theoretical essays. I was asked to give an address to the American Psychoanalytic Association in the 1970s. It was a kind of playful essay. I wrote an essay and delivered a talk that was called "The Myth of the Good Life in California." I approached it mainly from the standpoint of ambivalence. The myth has got a happy side and it's got a dismal side, and I tried analytically to then link with the themes of the happy and the dismal sides together within a psychoanalytic framework. It was very popular, that lecture. I was Phi Beta Kappa lecturer to eight universities around the country that I wouldn't otherwise have gone to. At every one of them, I gave this lecture on the myth of the good life in California. Of course, everybody in the

rest of the country lapped it up, because they thought I was bashing California. That was a psychoanalytic effort.

I later wrote a technical essay on the application of psychoanalytic thinking to the study of art, to the study of social organization, and again also to the study of social movements. That is to say, applying psychoanalytic thinking to non-clinical settings. That was a large and major essay. The idea of ambivalence became a core theme in my own thinking, and it was at the center of my presidential address to the American Sociological Society in 1997. In that year, these articles had accumulated to the point that there was a real book there. I approached the University of California Press in 1998, about the time I was going to give my presidential address, and asked them if they would be interested in a collected book of essays that had accumulated over time. I had a close relationship with Erik Erikson, and I had written about his work as a social scientist. So all of these things fell together, and they fell together pretty well into a book, and that was published in 1998 by the University of California Press. *The Social Edges of Psychoanalysis* is what it was called. It added up to be a book-length series of contributions, though I had scattered these things everywhere throughout the literature. They hadn't ever been brought together. I had done them, in a way, opportunistically. But together, that showed the collective and longstanding influence of psychoanalysis on my thinking. Then of course, with my book *The Odyssey Experience*, that came out in 2009, that was psychoanalytically much informed.

10-00:07:08

Rubens:

Erik Erikson—so you had met him at the institute?

10-00:07:13

Smelser:

He and I met during his trip to California, when I was still in analysis, but we didn't really register much relationship. Then, after he retired from Harvard, he moved to the Bay Area. He lived in Tiburon or Belvedere, and he and I joined a psychoanalytic discussion group headed by Robert Wallerstein of the San Francisco campus. These were intellectually-inclined analysts. Wallerstein was close to Erikson. He invited Erikson. He invited me. I had collaborated with Wallerstein on an article on psychoanalysis and sociology earlier, so Wallerstein and I had a close relationship. We became friends. There was this group of about eight people or ten people who joined in this seminar that met evenings over in San Francisco for a year or two or maybe more. That's when Erikson and I hit it off and began to see each other independently. He would come over to the Berkeley campus. We'd have lunches in The Faculty Club and we'd talk. We developed a beautiful relationship. It was very interesting, because I knew his son, Kai, before I knew the father. Kai and I were good friends before I met Erik.

10-00:08:48

Rubens:

And where was this?

10-00:08:49

Smelser:

He was at Yale. But at the sociology meetings, at different places, we found each other and sought each other out. I was almost exactly Kai's age, so I was kind of the son figure for Erik. My father had died just a couple of years before. One of the psychological effects of my analysis was to bring me to realize I always had a good relationship with my father, but it had some distance in it. I came to realize how much I really loved my father through the process of the analysis. Of course it was a bit late for me to express this to my father, who was very old. He was failing at the time and he died. I always thought that my relationship with Erik was a way of expressing that positive side that had been somewhat inhibited in my own relationship with my father. Erik kind of liked it, because he didn't have the same problems as he had with Kai as a son. It was the right moment, I think, for both of us. We actually collaborated in a book. We put a conference together. I took the leadership in this, because Erikson was fully retired and was beginning to get less active. It was also a beautiful experience to work with him on that. Then he left. He went back to Massachusetts. The last few years of his life, I of course diminished the contact I had with him. Shortly after that, he began failing himself and died several years later.

10-00:10:32

Rubens:

Your father died?

10-00:10:34

Smelser:

In 1975.

10-00:10:38

Rubens:

And so you had already met—

10-00:10:40

Smelser:

I was already with Erikson at that time. The results of the conference were published in a book called *Themes of Love and Work in Adulthood*. It was on adult development. That book appeared about 1980 or '81. My friendship with Erik flowered in that five years between my father's death and the appearance of the book. It continued a ways afterwards, but as I say, he moved away.

10-00:11:14

McIntosh:

Love and ambivalence are two concepts that have come up as a result of discussing your work in psychoanalysis. Are there any other—

10-00:11:26

Rubens:

I was going to ask—I'm interrupting you Jess—if you'd just articulate a little bit, particularly what you meant by ambivalence.

10-00:11:33

Smelser:

All right. Ambivalence is the feeling of love and hatred toward the same object, simultaneously. And above all, the fact that it's never really resolvable. That it switches back and forth, back and forth. Any time that the love comes up, it often excites the opposite tendency. Any time the hatred comes up, and is of course rooted in the children's attitude toward their parents. And in

particular, Freud made it a center piece in his discussion of the Oedipus complex, and the attitudes of the son toward the father, the love and the hate, and the competition and the striving and the affection and helplessness all went into that. That was, of course, the place where he made most of it, though he saw it as a universal and permanent feature of life. That anything you got close to, you also would come to hate. Many aspects of Freud's theories have been more or less definitively discredited. His theory of sexual development has been much criticized and vastly reformulated. While it's still a part of the corpus of psychoanalysis, it's not taken as seriously. His theories of dreams have been challenged. His classification of neuroses and disorders has been seriously challenged. His writings on history have been more or less discarded. Then, of course, his writings on people like Leonardo and Dostoevsky—a lot of very critical attention to that.

One thing that has really not been disturbed is this idea of ambivalence, in terms of outside criticism. It's really been a standing ingredient. It's actually spread. It's still, I'd say, a sort of core ingredient as it was originally expressed. It showed up in my work, even before I got into the analytic world. If you look at the introduction to my thesis, I talk in the very first paragraph of the introduction how ambivalent we are toward the idea of developing countries. We have all these awful names for them, like backward and barbarian and so on. At the same time, we love the idea that they're moving ahead. I really began to exploit this idea of ambivalence. It appeared, though not so much under that name, throughout *Theory of Collective Behavior*. Then—again, I didn't plan it—when I was working on this book on comparative methods in the early seventies, I began talking about how ambivalent we are toward peoples who are different from us. That theme just kind of kept coming back and back and back. It kept reappearing in my own thinking. Then, of course, when I was trying to decide some kind of statesmen-like theme for my presidential address, I thought, I could have talked about functionalism, I could have talked about the history of sociology. There were five or six topics that were on my mind. I actually called in a former student, Christine Williams, who was a fellow at the Center of the time, and I went down this list with her. She said, "You've got to talk about ambivalence." She said, "There's no question about it. You've got to talk about that." Not that I decided at that moment, but in the end, I said, that's really the right thing for me to talk about.

10-00:15:00

Rubens:

The other word that comes up over and over again, and is a fact of your life, is collaboration. That you have really worked with so many different people. It seems in the main, most of those have been very fruitful and productive and enjoyable. Would you say there's something in your nature that facilitated this? Well you talked also about being a loner in certain ways in your college years.

10-00:15:20

Smelser:

One of the interesting threads in my psychoanalysis—and in fact, my analyst brought it to my attention in one of the rare comments he made in the first months of the treatment. He was wondering about whether I could cooperate with people, particularly younger scholars. Well, both older and younger. The older being feelings I had about my father and brother, and the younger being feelings I had about my younger brother. You're always ambivalent toward every member of your family, even though it was a pretty good and conflict-free family life. He actually raised that. It kind of startled me that he should raise it, because in fact I had collaborated with my brother already. We edited a book together. We wrote the introduction together. So we had collaborated together. Interestingly, I sort of took the leadership in that piece. Bill had written some scholarly work, but he was basically a therapist.

10-00:16:40

Rubens:

Just for the record here, will you just make a statement about what that book was?

10-00:16:43

Smelser:

It was called *Personality and Social Systems*, Published by John Wiley and Sons in 1963, and then there was a second edition in 1971. Once again, interdisciplinary impulse, bringing together work by psychologists and work by sociologists that tended to articulate with one another. The book got a lot of attention. There was a very funny ad that came out that John Wiley, the publisher, put out an ad—it was published in a journal—that said "Smelser and Smelser. That's good news." [laughs] That became a family joke from then on. We'd joke about this a lot. But anyway, going back to the collaborative issue, of course I collaborated with Parsons, too. I was a bit perplexed as to why my analyst had brought this up, though it provided the provocation for a lot of exploration of my feelings about my brothers and my father.

10-00:17:43

Rubens:

Now, Parsons had collaborated with a lot of people as well.

10-00:17:46

Smelser:

He'd collaborated with Bales and Shils and some others of his students, never to quite the extent he did with me. Never jointly co-authored a book. Parsons had a lot of collaborative energy. He was a man who was very remote, but he was easy to collaborate with because he was receptive to people's ideas. He was a dominant person, but he was always very supportive of me in everything that he might have been supportive of. I've collaborated with students in writing a certain number of things, and certainly editorial collaborations of many, many sorts. No, it's been less fraught than my analyst's question would have suggested. I've never analyzed it. I've never thought it through completely. Nonetheless, it's not been a problematic area. When the prospect of collaboration has come up, I've never had a problem.

I had an episode with Parsons later on. Perhaps I can discuss it right now. This was in the late seventies. The student movement had been active for a long time. Parsons took an interest in higher education. I had already begun my own research in higher education. I'd written the book on the University of California and its history during conflict periods. Parsons was collaborating with another scholar, Gerald Platt, a former student of his, on a book that ultimately became *The American University*, which I think came out in 1982 or 1983. I don't remember the exact date. Parsons and Platt approached me as a collaborator in this book. I had been already in an administration. I had written my book on conflict and change in California history between 1950 and 1970. I had been given a grant to do that book by the Ford Foundation. Parsons was turning in that direction, so they called upon me to write this theoretical analysis and recent history of the American university. I agreed. Parsons and Platt and I began having meetings together. Parsons and Platt were taking a certain line toward the university. They were focusing above all on the unifying influence of the academic culture on the university system, whereas I had been through all this conflict and all this knowledge of the contentions and infighting. I was really interested in it. My book was entitled *Conflict and Change in California Higher Education, 1950-1970*. We were already beginning to pull in different directions. He was taking a kind of smooth, transitional view of the student movement and how it was being absorbed into the system. It was kind of a symptom of disturbance, if you will call it, whereas I was really much more heavily into the structural contradictions in the higher education system. Group conflict and proliferation of groups that were at loggerheads with one another.

The collaboration began to get increasingly difficult. At a given moment, I was feeling extremely uncomfortable about being in this particular line of analysis. I took the leadership in suggesting to Parsons that I really didn't agree with the dominant emphasis that he and Platt were putting into the book. Perhaps I should maybe just not be part of the operation. I suggested that. It was the worst moment I ever had with Parsons—the second worst. I'll mention the first worst in a minute. Platt, to whom I'll be forever grateful, said, "We don't want you to leave this enterprise. Why don't you write an epilogue, putting your perspective into it?" That saved the day. It permitted me not to be just full co-author with a line of analysis that I thought was skewed. I didn't totally reject it, but I thought it was one-sided and skewed, and my own views were different. I wrote the epilogue to that book, and I wrote about conflict and change and tension. I sent that chapter to Parsons, and here was the worst moment. Parsons really didn't like it. He thought it gave away too much to his critics. He had had this twenty years of being bombarded with the ideas that he was a political conservative, that he was too much interested in the integration of society that he didn't really recognize conflict and change, and so on and so forth. Here I was, right in the middle of this book of which I was a big part of, writing along lines that were openly critical of a lot of the tenets that were informing the rest of the book. I was very polite in the way I phrased things, and always gave deference to his points of view. I said, well, here's

another additional dimension, and then I would go on writing about something that was in fact pretty destructive, though I never advertised it as such. It was just a different emphasis. After he got this letter, he wrote me a long—I don't know how many pages—letter, which got quite emotional in places. He said, you are giving away too much of what he considers my or our theory. You're joining the critics. You're joining Coser. You're joining Dahrendorf You're joining Mills. You know, the whole gang that was criticizing him for so long and so severely. He really leaned on me to change it, change my epilogue, which I wouldn't do. I just said, "No, this is my analysis." It became personal at that point, though Parsons, in the end, graciously said, "Well, let's put it in. Let's have it." So it comes in as not exactly heretical, but very different, and in some ways, contrary to the general theme of that book.

That personal episode never afterwards threatened our relationship. I still maintained a relationship with him. In fact, just before he died, he sent me a copy of the new book he was writing. One of the chapters, he wanted me to react to it. I was living in England at the time. I wrote a long, long reaction to the whole thing. He was going to come and visit me in London in 1979. He died the day before he was to come to see me. As a matter of fact, it was sort of spooky. I got a postcard from him the day after he died. We kept a personal relationship, even through this very difficult episode. I may be ahead of myself a little bit here. It's on the theme of collaboration.

10-00:26:04
McIntosh:

Very interesting. I just wanted to get back into the sixties and your psychoanalytic experience. We know about the personal relevance for you, but conceptually and intellectually, anything other than ambivalence or love or attention to personality that you felt equipped with after?

10-00:26:28
Smelser:

Well, I struggled. I did some theoretical work on psychoanalysis and other lines of inquiry. This began with Bob Wallerstein, in which we wrote this early collaboration called *Psychoanalysis and Sociology*, which appeared in *The International Journal of Psychoanalysis* and got some attention. It was always a good essay, but still I hadn't thought through a lot of the things. In a way, I was living a double life as a sociologist immersing myself in psychoanalysis, because their whole world view of what makes the world go round is so different. Internal conflict, impulses, defenses, coping. The view of reality is regarded in large part as projection or interpreted in terms of one's own neuroses and conflicts and problems and defenses. I was always interested in how emphasis on structural and larger forces could interact and produce anything coherent with this fundamental emphasis on inner emotional and conflict of dynamics. This was an ingredient, this theoretical synthesis, that informed a lot of my writing on psychoanalysis.

A certain influential essay I wrote, actually as my, quote, "thesis" for graduating from the institute—you had to write a little article-length thing. I

wrote a theoretical piece on the mechanisms of defense. The mechanisms of defense are an absolutely core feature of later psychoanalytic thinking of Freud, and especially Anna Freud. They were all directed toward defending against inner conflict, that some impulse, some disturbance, some unacceptable impulse would make its appearance and come in conflict with others, and then rationalization and diverting and projection and so on were all meant to deal with this inner dynamic and turmoil. So I wrote an essay in which I said, look, defenses are equally relevant to the dealing with the external world. Conflicts, anomalies, difficulties, suffering imposed by others. I systematically reclassified the mechanisms of defense according to a scheme that I had worked out, in a way that they had never been systematically classified before, and said, this is a way they apply to—you might say internal reality. This is a way they apply to external reality. I developed a kind of synthetic view of how, in a way, there's a kind of negotiation between the different kinds of defenses, inwardly and outwardly. That appeared as one of the essays. Then I also got interested in the application of psychoanalytic theory to social issues and social creations, art and social movements and so on. Then I also wrote a couple of strictly theoretical pieces on the difficulties and prospects of linking these two different world views with one another. So I'd say, in answer to your question, that there was a specifically theoretical series of issues that grew out of my interest in psychoanalysis and, you might say, the double life I was leading between a lot of my work, which was mostly macro-sociological at the time, and then immersion in this completely different intellectual tradition.

10-00:30:23

McIntosh:

It's a double life, but at the same time, what strikes me is how they both get at a similar issue, which is something that you've been engaging in throughout your career, which is looking at different ways of understanding human action and human behavior. Right? By which I mean, other than just a rational choice model. It's interesting that, of so many people in the milieu in which you're operating, you're the only one, most often, who has real economic training. You seem to be so eager to not heed the doctrine of economics.

10-00:31:02

Smelser:

It's been a lifetime polemic on my part against constricted economic thinking, yes.

10-00:31:09

McIntosh:

And so, at the time, was psychoanalysis another avenue for exploring different aspects? Was that intentional on your part?

10-00:31:25

Smelser:

Intentional or accidental, I would have to say that the latter played a bigger role. I didn't have a big plan about this. It was a part of my life history that I got involved in the psychoanalytic world. It was part of my life history that triggered the event of entering into psychoanalytic treatment and training. I did not have a master plan of any kind of intellectual integration. It kind of

became part of the picture as I went on. It's very consistent with my whole intellectual career. I'm always looking at some other pond in which I can put my oar and mix it up. It's thoroughly interdisciplinary. Economics, history, political science, anthropology, and psychology, including psychoanalysis, have all been in my radar, in different emphases, different weights, all through my career.

10-00:32:25

McIntosh:

Back in the sixties, when you were dipping your oar in different ponds and equipping yourself with all these different models for understanding human behavior, when you would sit down to study an event, how would you choose which one to go with?

10-00:32:44

Smelser:

Having studied, and in some degree mastered, a variety of different disciplinary ways of thinking, and even substantive research in those disciplines, this all becomes internalized as intellectual tools which you have available. A toolkit, if you will, available to approach a given situation. I think I also have had a consistent strain of letting the material speak to me, and not coming in with this kind of top-heavy intellectual framework and saying, something is going to fit into this. There's always been a certain openness toward empirical data, information, the flow of history, to which I then get sensitized by these different frameworks that have always been available in my own mind because of the accident or the plan of my own training, or my own inclination to arch over and diversify my own approach to things. That's the way it's worked out. It's a kind of confusing process rather than any kind of master plan that can be subsumed under the idea of deduction, or figuring out what's important and then applying it to the world. Everything unfolds as the project has gone along.

10-00:34:09

McIntosh:

There's a fellow by the name of Howard Brick who's an intellectual historian. I don't know if you're familiar with his work at all.

10-00:34:15

Smelser:

Not really.

10-00:34:16

McIntosh:

He's written about mid-century sociology in the United States. His argument is that there is a movement to subsume economics under sociology.

10-00:34:34

Smelser:

Parson's a big part of it, of course.

10-00:34:35

McIntosh:

That's what he was trying to do. Since we're on the subject of looking at other ways of understanding behavior, who else in the sixties was taking a similar tact to yours, if there were any other figures? Trying to get away from rational choice models, I mean.

10-00:35:00

Smelser:

There was a kind of intellectual movement, of which Parsons could be said to be a part, though he didn't use the words so much. He always referred to his own work as a general theory of action. That, of course, subsumed a lot of different approaches, but was distinctively his own stamp. There were others writing in a similar vein, and it went more often under the heading of systems theory. The figure who was most prominent in that was Kenneth Boulding, the economist who wrote a great many works outside the kennel of economics. Those works were fairly Catholic. They weren't the effort to superimpose an economic world view on everything else. He wrote a lot of books that were extremely interesting, and he was a very interesting man. I met him on a number of occasions at different conferences, not by accident. We'd both show up because we were both kind of generalists. He was kind of a funny man. Arrogant, self-centered guy, but he had this openness about him that I always thought was welcome. There were other people that called on systems theory. The cybernetics people were getting into it. A man by the name of Walter Buckley was very much interested. The idea of bringing work in from all the social sciences, and also biological sciences, that a systems approach was going to integrate—going to be a unified kind of science. That was quite popular. From the fifties up through, say, 1970 or something, this was a noted bit of literature that died suddenly. That kind of level of general theory generally fell off, and has never returned. We kind of turned sociological theory over to the Europeans. With the great parade of names, including—well, maybe Foucault, but certainly Habermas and Giddens and Bourdieu. The whole parade of French and continental and English intellectuals more or less took over thinking in general theoretical terms. Grand thinking in this country has retreated into a more modest series of interdisciplinary projects rather than general theoretical encompassing efforts.

10-00:37:38

McIntosh:

One last comment is that what's interesting to me is that a lot of U.S. intellectual history that focuses on this era focuses on people like Parsons, or people like yourself, or even Boulding and Norbert Weiner and people like that, who aren't working in this rational choice paradigm. Was rational choice a bit of a straw man that you all were using in order to define yourselves, or were there actual people who were pushing it at this time, in the fifties and sixties?

10-00:38:21

Smelser:

Pushing rational choice theory?

10-00:38:23

McIntosh:

Rational choice theory, and who were sort of formidable opponents.

10-00:38:26

Smelser:

Well, it was just an operating orthodoxy in all of economics. It was just assumed as part of economic analysis, and any time you undertook the analysis of a problem, you built models, often quantitative models, based on a

series of rational assumptions. It was being secretly challenged by a lot of people in economics, who were now introducing qualifications on the assumptions of full knowledge and perfect fluidity of resources and so on. In the meantime, since that time, rational choice theory has undergone an internal revolution in economics that they will not admit. There's still an article of faith in rational choice. It's not only the way that people in markets will behave, but people like Gary Becker argue that this is the way people outside markets will behave. It's a kind of continuing orthodoxy, at the same time being modified kind of from within. Certainly psychology, particularly the heuristics tradition in psychology, has led to serious challenges to the cognitive and evaluative and decision making aspects associated with rational choice theory.

You asked if it was a straw man. Well, we thought it, genuinely, intellectually mistaken. Intellectually limited, and simply didn't solve problems it was meant to solve. I've written two lines of thinking. I wrote a little article around 1989, '90, called "Economic Theory as a Religious System." I analyzed it in terms of a parallel between what's happened in economic theory and doctrine, with what happened in Catholic theory and doctrine. That is to say, it kept adapting to the world but still insisting on continuity of faith. I saw great parallels in what formal economics has done. Most recently, I've tried to analyze why, in spite of this parade of destructive attacks and full discrediting, basically, of the really orthodox economic rationality, why has it persisted and why has the influence of economics persisted. It's a bit of sociology of knowledge as to where they're located in society—their own, you might say, public relations stance, and exactly how they treat deviations to their own theory. Rational choice theory is still a formidable piece of the intellectual armor that the society has around it to deal with problems, but at the same time, intellectually, it's somewhat in shambles.

10-00:41:12

McIntosh: That is a very striking analogy between Catholicism and economic—

10-00:41:17

Smelser: They've continued to adapt to the real world, but maintain a historical and world continuity that there are fundamental truths and that's what we're still living by.

10-00:41:27

McIntosh: Was game theory at all—that made a big splash in the fifties. Was that at all anything that you saw a need to react against?

10-00:41:35

Smelser: I always had the same skepticism toward game theory as I did toward rational choice theory, insofar as I thought it was a vastly simplified, and in a sense artificial, series of assumptions, even though it dealt with conflict situations. It deviated in large degree from classical, rational choice theory. I still maintain that it had suffered some of the same intellectual narrowness. It's still very,

very much in our world, game theory, in business administration and political science.

10-00:42:14

McIntosh: International relations theory.

10-00:42:16

Smelser: In international relations theory, foreign policy, et cetera.

10-00:42:23

Rubens: Just to finish the thread of your turn to psychoanalysis and psychoanalytic training, I wanted to ask you about one more concept that I guess comes from Durkheim, but you've used *anomie* in many of your talks and writings. I wondered if you could articulate how your concept of that changes as a result of your study and experience of psychoanalysis.

10-00:42:46

Smelser: Not really. Durkheim's notion of anomie is fundamentally a social concept. It has psychological postulates connected with it, but it's basically the kind of disorientation that comes with rapid social change, in which the former rules of society no longer apply. He got it going most of all in the division of labor, in the irregularity of the processes of change, but above all in his notion of suicide, in which one of the causes of suicide was the disjunction between people's expectations and their actual life experiences. It was on the basis of anomie that he made the prediction, or thought it made the claim, that both in times of rapid economic decline and in rapid economic growth, you're going to get increases in the rate of suicide, because both of them are confusing. Both of them are socially confusing and the old rules don't apply. The limits of life get disturbed, and then, of course, the psychological consequences of this were confusion, despair, uncertainty, and so on. That's the whole psychological link with suicide that he made. It's since been deeply challenged, that theory. It's a concept that naturally kind of appealed to me, going back to the connection between the disorderliness of processes of change. That's exactly what Durkheim had in mind. That element of Durkheim has always struck with me as an analytic feature of my own thinking.

10-00:44:31

Rubens: So it's not particularly altered—

10-00:44:33

Smelser: No. The psychoanalytic thinking and anomie, I've never made much of an effort to link the two.

10-00:44:40

Rubens: Should we conclude with your practice of analysis, or do we—

10-00:44:46

Smelser: Yes, I can do that. Yes. The rules changed—a little anomie in the middle of my training—that said, now you can practice. But my senior people over at

the institute said, "You should start practicing." They were really very, very encouraging that I should take on a clientele. During the course of my training, they didn't want me to take on psychoanalytic patients right away, cold. I was not a psychiatrist. I did not have psychiatric practice in my background. They said, "You should get a little advanced clinical training before you start taking on psychoanalytic patients." Very graciously. Cowell Hospital at UC Berkeley let me join its staff. I requested to join its staff as a therapist. At the very beginning of my analytic training, it was suggested by the institute, and so I joined that staff and worked up there a number of hours a week—eight hours a week, maybe, or variable—seeing students.

This is beginning in '64. I was seeing students through FSM. I was seeing these students coming in. I was on the Cowell staff for two years then, getting some clinical training that I discontinued when I started taking analytic patients. That was about two-and-a-half years into my training. I began taking non-patients through the Psychoanalytic Institute, who'd applied for low-fee analysis. I began analyzing these patients. The rule was, in order to graduate, you had to have a certain number of hours of analytic practice, and you had to bring one case to successful conclusion. One of the cases, I analyzed for three-and-a-half years, and that was the one that was judged by me and my supervisors to be completed, but then I had four or five other cases that went on anywhere between one and two-and-a-half years I had met them up in Cowell Hospital. They gave me an office that I could work in.

After I graduated, I really had a dialogue with myself. Do I want to start taking on analytic patients? It would have been continuing this as a second career, almost. It would have been a different flow of income for me. A lot of different aspects of it. I said to myself, I really don't think I want to do this. The reason I didn't want to do it was because of the infringements on my travel. If I took psychoanalysis seriously and saw patients on a four-times-a-week basis and had a number of them, I couldn't do it. I would just wreck the whole process by being away so much. It's one of the biggest disruptions of psychoanalytic treatment, absences. Of course, you're really bound to the locale in which you're practicing. By that time, I think my life was flourishing. I was traveling a lot. It just wasn't going to fit my general academic and related career at that time, because I was already on a lot of national things, getting involved, and so on. I said, well, I'm not going to practice psychoanalysis. But I did join the Cowell Hospital staff again, in which you could gear the vicissitudes of the academic and my personal calendar easier. I was able to carry on a limited practice there for several years more. Then I became a supervisor of students in the clinical psychology program on the campus for ten years. I supervised their treatment of patients as they were beginning their training. I would have a supervisee every year among the clinical psychology students over at Tolman Hall, and would guide their psychotherapeutic efforts during their own training. I kept my hand in, but in this very limited way that did not really disrupt the other aspects of my career.

10-00:49:12

McIntosh: We're almost on two hours now.

10-00:49:14

Rubens: I think we should wrap it up for today. I think next week, we'll start with the heady fall of 1964 -how that affects your department and then you.

10-00:49:25

Smelser: Yes, and then '65 was just a great turning point in my career.

10-00:49:29

Rubens: That will be a fascinating, wonderful session.

10-00:49:31

Smelser: That's good.

Interview #6 April 5, 2011

[Begin Audio File 11]

11-00:00:10

Rubens: Good morning, Neil. We're here for the sixth interview. I'd like to see if we could locate and map out where you were in the fall of 1964.

11-00:00:24

Smelser: I was beginning my seventh year of membership on the faculty. I'd been teaching steadily and regularly, mostly my required course, but on other areas as well. I was teaching a course in the fall of 1964, Collective Behavior, I believe. My book had been finished a couple of years. I had begun my psychoanalysis. I was just on the verge of beginning classes at the Institute. I was already seeing people at Cowell Hospital on a one-day-a-week basis, four or five hours a week, as part of my apprenticeship into the analytic institute. Four or five patients a week. It was a short amount of time, but definitely clinical experience, and it was being supervised by staff at that hospital. And I was continuing my editorship of ASR, entering the third year of my three years as editorship of the ASR. So my plate was normal and full that term.

11-00:01:32

Rubens: Plus you were also separated from your family, and you were picking up your children—

11-00:01:38

Smelser: I was taking care of my children two-and-a-half days a week, carrying them back and forth to their school in San Francisco and having them on weekends.

11-00:01:47

Rubens: A little more than normal.

11-00:01:50

Smelser: Obviously, I was very dedicated to my children. Obviously, I was always traumatized and very guilty by the separation at the young age. So I made a point—not in a mechanical way—but I made a point of wanting to keep my fatherly relationship with them alive and well. This was agreeable with my former wife. In fact, she probably would have asked that I take them more if I had had more time. Anyway, yes, my days were full. Of course, the semester had begun when the first revocation of the activities on the strip were done. The semester just really began with a bang, because it was only a few days after the semester that Alex Sherriffs and other staff members— Katherine Towle—actually laid down that prohibition, and that's what started all the—

11-00:02:53

Rubens: Prohibition for the students to set up tables along—

11-00:02:55

Smelser: Yes, set up tables to collect money and organize students to engage in political activity off campus. They were sending buses over to the Republican National

Convention, which was being held in San Francisco, where Barry Goldwater was nominated. So that was a political moment that was significant nationally as well. But that was then prohibited, and then began the fireworks of the demonstrations, the arrests, the disciplining, and so on and so forth. That drama began then. By October, with the police car episode, it was fully in swing.

11-00:03:28

Rubens:

Do you want to begin first with reactions of members of your department and how it tapped into—

11-00:03:34

Smelser:

I will give you first my own personal first impression of when the troubles broke out. I was frankly perplexed. I was surprised. I did not have a prediction of this. Very few people did, even though there had been a good deal of political action on the campus the previous three to four years, on a variety of different things—civil rights, hiring policies of local firms, sometimes the visiting presence of a controversial foreign leader. There were demonstrations, and one saw this as an active campus for sure, but nothing of those dimensions. I early remember thinking it was very foolish on the part of the administration to prohibit this activity—but it was a somewhat academic reaction—because I had come across in my own reading on social movements and revolutions and religious movements that these precipitous crackdowns after a period of leniency are an absolute key to very explosive reactions. It's one of the formulae. I knew abstractly that this was a wrong thing to do, and there I saw it kind of happening before my own eyes. I didn't feel inclined to do anything. I was a young faculty member, not exactly in a position of leadership, and no access—I didn't know [Chancellor] Strong at all. He didn't know me. Sherriffs, I knew about, because he was a psychologist, and I knew he was vice chancellor. Didn't know him. Perhaps casually, just to say hello. I was a faculty member, probably somewhat typical across the campus, who was basically confused and perplexed as to why this thing had exploded.

I'll talk about my colleagues. It was a department that had an interesting composition, because it had grown rapidly. It wasn't really formed until 1952 as the sociology department. It was called the Department of Sociology and Social Institutions. It was sort of a diverse department, really. Clark Kerr's decision to give it the real go-ahead came in 1952, and it was in that great wave of sort of bliss, between '52 and the early sixties, that there was a tremendous amount of hiring of both senior and junior people, and the quality of the department got established then. One of those years in the early sixties, we were ranked as number one in the country because of the really explosive development of talent at all levels. One of the very interesting features of the department was that two of its members, Philip Selznick and Marty Lipset—they were both about ten to twelve years older than I, and they were in their late teens in the Depression, whereas I was less than ten years old. They were in from New York, and they were in the radical left of New York and they

were politically seasoned because of that particular history. Then there was a man named Lewis Feuer who was over in the integrated social science program, who has also been active in that period and had renounced it. Everybody in that thirties New York scene was very active at the time. There were divisions there. They all went in different directions. But Lipset and Selznick in particular were very active in the far left at that time. Neither one of them were in the same radical frame of mind in the 1960s. They had taken different routes in their own personal careers, but that element of political sophistication and savvy that came from their involvement in that early period was a very important feature. They were very active. Lipset was somewhat to the right of Selznick. There was a certain amount of personal competition between the two, as sociologists. I knew about that. They weren't enemies, but they were chilly in their relations with one another. Politically, Selznick was more sympathetic with the student protests than Lipset, but they weren't at extremes.

At the extremes, on the right, were Kingsley Davis, William Petersen—both demographers—and Feuer who wasn't in the sociology department, but hobnobbed a lot with the sociology people. I got to know him because he was kind of a social philosopher in his own work, and of course he was politically very engaged. I would say Lipset and Selznick constituted a kind of middle, even though there were differences between them. Blatantly on the left were William Kornhauser—not at the time of the Free Speech Movement. He was rather right wing. He was sort of a hawk before he went far left. He got won over by the student movement, in a fairly radical way. My book review editor, David Matza, who was my age, also was inclined to be more sympathetic than I to the movement. I'll say what my orientation to the movement itself was in a moment. Then Robert Blauner, who also had a radical background. He'd come back already, in '62 or something like that. He'd gone out to teach at another institution. Chicago, maybe. I'm not sure. Came back. Had just been made a tenured faculty member, certainly on the faculty. He was left-inclined. So you had an array of people already, though they did not crystallize.

During the last months of 1964, the department did not polarize. It was inclined in these directions. After the academic senate meeting of December 8, then the cleavages really began to set in, and they got hardened and generalized during the whole period of protest from 1965 up through 1971. It was a gradual process, though it hardened, I would say, in the mid-sixties, into a very divided department. I was, I'd say, confused. I didn't have what you would call a completely articulated political reaction to the events. I felt rather saddened that my university was afflicted by this kind of conflict. It would be nothing but mischief as far as a university was concerned. I thought the costs were going to be great, being involved in this kind of conflict, and particularly as time went on, damaged by evidence that they couldn't handle it. That was the worst part of that conflict, was that the administration was rather helpless in knowing how to deal with it, and of course, ultimately, paid the price of having to—Strong was let go by the regents.

- 11-00:11:04
Rubens: You're following this from *The Daily Cal*, from certain observances?
- 11-00:11:10
Smelser: Following it through *The Daily Cal* and conversations with colleagues at Cowell and the department and so on. Nothing heated, no conflict over it.
- 11-00:11:18
Rubens: I wondered also if your students were asking of you, especially, someone who's written on collective behavior—
- 11-00:11:24
Smelser: No. I was teaching this course, but it's quite interesting that the classroom was not infused at that time the way it was some periods later, when actual protests got into the classroom —marching in, disruptions, and so on. I can say a little bit more about that later, in my own graduate classes, which did get politicized.
- 11-00:11:46
Rubens: But not at this time?
- 11-00:11:47
Smelser: Not at this time. All this turmoil was going on, and a lot of continuity was going on as well, especially outside the whole complex of Dwinelle and Wheeler and Barrows Halls. The social science and humanities complex was the main source of action. If you'd go over to the engineering school, up on the other part of the campus, it was a long ways away.
- 11-00:12:14
Rubens: How long had you been in Barrows? I thought it opened in—
- 11-00:12:19
Smelser: When I first came, South Hall was the central sociology office, and individual studies were in Wheeler. I believe Barrows Hall opened '62 or three.
- 11-00:12:33
Rubens: It was new.
- 11-00:12:34
Smelser: I was in South Hall for two or three years.
- 11-00:12:37
Rubens: Sociology was on the second floor?
- 11-00:12:38
Smelser: Second floor. We called it the bullpen because we all had a desk at which you could sit—it was one huge room. It was a pretty primitive—
- 11-00:12:49
Rubens: Oh, not all strung out along the—

11-00:12:52

Smelser:

No, it was one big room. We had this nickname, bullpen, for it. I would see students in there sometimes. It was kind of a nice arrangement, really, because it was more public and more interactive.

11-00:13:10

Rubens:

We're talking about South Hall, is that right?

11-00:13:11

Smelser:

South Hall. It was more public and more interactive among faculty than the Barrows, which tended to be separated by offices. The central administrative office wasn't much in terms of functional interaction and so on. But anyway, South Hall was kind of a favored place, I thought, because it was the oldest building on campus and so on.

But anyway, I did get involved early, even though I described this tentativeness and this confusion in my own personal reactions. I got involved by a kind of fluky accident. I was walking, I think, over toward the student union to get a cup of coffee or something one day, and here comes Marty Lipset, my colleague, and he intercepts me on the little bridge going over there, and said, "Look, we're going to see Strong. We're really upset about this situation." It was in the middle of the police car episode. This group of faculty had demanded a meeting with Strong. It was Lipset. I think Bill Peterson was along.

11-00:14:21

Rubens:

Nathan Glazer?

11-00:14:22

Smelser:

Nathan Glazer was there. It was called the Glazer Group, I think, by some of the people who have written it up. Henry Rosovsky from economics, and I think David Landes was there, too, if my memory is correct. Anyway, they picked me up as a kind of straggler. I didn't know exactly what the meeting was about.

11-00:14:41

Rubens:

And you hadn't spent time observing this mass demonstration?

11-00:14:46

Smelser:

I'd gone by it and I'd gone to it. I didn't get totally fixated by it, but I knew it was going on for sure. Hard to miss it if you were living in that part of the campus. But I wasn't inclined to do anything about it. I was uneasy about that whole situation. Anyway, we went and we waited in Dwinelle Hall, waited outside Strong's office. The chancellor's office was in Dwinelle at the time. He finally came out, and it turned out to be a completely unsatisfactory interview. These people had come with some effort and some suggestions as to how the situation might be resolved, and Strong simply took a straightforward and traditional and antagonistic attitude toward the students. I remember a vivid phrase that just curdled my blood. He said, "These students should go home."

That was his diagnosis of the situation. An old-fashioned dean-of-students-like mentality. He was sort of short with us and impatient. He was already under great stress, I suppose. I know he was under great stress. The interview with him was not helpful.

In the meantime, Henry had been taking down points. He sort of turned out to be the secretary of this group. We were sitting there feeling kind of hopeless, because this interview with Strong had been so unproductive, and we felt that it achieved nothing. So it was Marty and Henry who got the idea—and Nathan Glazer—to put in a call directly to Kerr, and to talk about our disappointment and to read him these notes about what we thought should be necessary for some kind of settlement of the impasse over that police car. What Kerr might be willing to agree to, what might dissolve it, and so on. Henry crept over to the edge of the room, quite accidentally, and called up Kerr and got him. Was able to get him on the phone in his office. I remember he was just reading off these points that—I hadn't really been a big part of it—been put together by these people who were politically more savvy and politically more centrally involved at the time. Kerr, at the other end of the line, he later told me, was frantically taking notes on these notes, and in fact they were the points that got into that settlement with Savio and the free speech steering committee that led to the giving up of that holding of the car and a temporary peace. It wasn't a permanent peace. It was a temporary peace, because subsequently, Strong and the administration tried to discipline all these students. That blew up and that got discredited as well. But that was a very interesting episode, and really my first, what you call, official involvement, on this completely accidental basis, in the thing.

11-00:17:50

Rubens:

Do you remember the tenor of Lipset? What his demeanor was? The reason I'm asking this is because I've read quotes from some of the students involved who said he was really spewing. He was sputtering. He was mad.

11-00:18:05

Smelser:

Yes, Lipset was highly involved sort of thing.

11-00:18:09

Rubens:

He had written this big book, *Political Man*.

11-00:18:12

Smelser:

He was a political sociologist. He was a seasoned political actor. Lipset was never a diplomat. He spoke his mind. On political subjects, he would get quite impassioned. It all came out of this politicization of the world that he lived through in his youth. Even though he was not committed to a specific left orientation anymore, he was passionate about politics.

11-00:18:40

Rubens:

So this was just made for him.

11-00:18:42
Smelser:

His disappointment was also very keen when we failed as a political effort. There was a second episode that I should report, also tied to Lipset. There was a delegation of people from the Berkeley campus, and I cannot tell you anymore about its circumstances of forming than that. It was some kind of very informal delegation, much like the one that picked me up to go and see Strong. Kerr was meeting on the Davis campus with interested political groups. I think a couple of little groups from Sacramento were there. The academic senate was there. Maybe it was at the time of a senate meeting. I don't know. Anyway, Kerr was there, and a number of different groups had converged to have some kind of urgent meeting with him. The whole campus was beginning to form into groups of lone rangers and small groups who all thought they had something to say about the settling of the big developing controversy, and this was sort of one of them. We went up and talked with Kerr. I don't remember much about the meeting. It was only my second meeting with Kerr. Kerr had greeted me as a young assistant professor. I didn't know him at all, and he didn't know me at all.

11-00:20:01
Rubens:

You were in that theory group he created.

11-00:20:06
Smelser:

He had formed the theory group. It was, once again, Lipset who pushed me. Kerr didn't have anything to do with the recruitment. He formed it and I was the beneficiary of it, but I never met him in connection with that theory group. Again, it was a stormy meeting, where Kerr kept his cool the whole time. I'm not sure if anything came out of it. It was one of these expressive meetings in which a lot of people expressed strong feelings, I recall, but it was somewhat indecisive. It was not a decision-making group. It was a group, I think, that Kerr had expressed a willingness to listen to at the time. I would guess it was around Thanksgiving—a little before Thanksgiving. For the major sit-in itself, I was not present. I had gotten an invitation to give a sociological talk at the University of Arizona, and I'd made the occasion to visit my parents in Phoenix at the time for a couple of days. So I was away for four or five days, say between the first and the fifth of December, and it was on the second that that sit-in occurred—and I read about it in the paper.

11-00:21:21
Rubens:

The students occupied Sproul Hall.

11-00:21:24
Smelser:

Eight hundred students moved in and sat in in Sproul Hall. There was a certain amount of roughhousing in getting them out of there. Six hundred of them didn't leave voluntarily. They had to be dragged out. There was a lot of talk about police brutality and so on. That really brought the campus into chaos. The next day, there was a huge meeting in the Greek Theater, where Clark Kerr tried to come and actually talk to the gathered community, and there was a presentation of a resolution by the chairs of all departments. Philip

Selznick was then chair, and he signed this resolution. That big episode in the Greek Theater, that farce when Savio came out to the stage to try to take over the microphone and then he was dragged away by the police, just made a shambles out of that, and that whole effort turned into nothing. That was, I believe, on the third of December, just after the sit-in had been broken up. Nothing creative or definitive came out of that big Greek Theater meeting. Everyone was still kind of pulling their hair.

11-00:22:38

Rubens:

I think it led the students to realize they really had to escalate their—

11-00:22:44

Smelser:

The students were heartened, really, by the failure, I suppose. The activists were all hanging around the stage there. I remember seeing Bettina Aptheker right on the scene just after the police came out. I came back, I guess, on the second. So I was at the Greek Theater meeting, sitting with Marty Lipset, and he was giving me a running diagnosis of how the far left was manipulating this meeting. [laughter]

11-00:23:20

Rubens:

Meaning the students?

11-00:23:22

Smelser:

The whole thing with Kerr, and then how they broke it up, how Savio came on, how it all got turned into a farce.

11-00:23:29

Rubens:

I'm just wondering, who does he think is manipulating it?

11-00:23:32

Smelser:

Bettina? She was the closest to being still a Communist. She was the daughter of a Communist, and still the furthest left in that whole group. She was lurking at that point. Now, he made much more of it than I think he should have, but nonetheless, that was his interpretation. He was, of course, distraught that all this political chaos was transpiring in front of his very eyes. I was distraught, too. Not in the same way he was. I was beginning to feel the political hopelessness of the situation. There was a big group. It's called Committee on Two Hundred, in which Lipset and Selznick and Kornhauser were active at the time.

11-00:24:18

Rubens:

This was the faculty group within the—

11-00:24:19

Smelser:

It was a faculty group called the Committee of Two Hundred. I don't know whether it had two hundred. It was mostly social scientists, but there were others in it as well. They were the ones that came up with the kernel of the faculty resolution that was to pass on December 8. That got introduced. The faculty sort of really mobilized with this general huge meeting that they scheduled then for December 8. It was attended by twelve, thirteen hundred

people. There were only two thousand on the faculty at the time, so this was a really—

11-00:24:54

Rubens: Usually the meetings had been—

;11-00:25:00

Smelser: Academic senate meetings—to get two hundred there is an accomplishment. When I was chair of the academic senate, a certain portion of my meeting didn't have a core. This was of course, obviously, a state of mammoth crisis on the campus. I went to that. Sat next to Erving Goffman. I should have mentioned a neutral group. There was Erving Goffman, Herbert Blumer, and Hannan Selvin, a young faculty member, who didn't figure into politics of the department at that time.

11-00:25:44

Rubens: There's one other person I wanted to ask you about in sociology, John Leggett?

11-00:25:49

Smelser: Oh, yes. He was a junior faculty member. He was an activist. He was an assistant professor at the time, and he was out there with the students. I should have mentioned that. He was right out there in the middle of them. He was indistinguishable from a full activist.

11-00:26:04

Rubens: He is in the center of that iconic picture of the FSM leadership marching under Sather Gate.

11-00:26:09

Smelser: He was among a small minority of faculty who could not be distinguished from the student activists in terms of his orientation.

11-00:26:17

Rubens: Not someone you knew particularly or had conversations with?

11-00:26:19

Smelser: I played softball with him, out on Strawberry Fields. I was a member of a group, and I was a member of a basketball group, too, that really crystallized later. It had my brother in it, it had David Matza, it had Leon Wofsy, the very far left—

11-00:26:37

Rubens: Biologist?

11-00:26:37

Smelser: Biologist, who had been a member of the Communist Party. And Henry Miller and a few others. We played basketball every Sunday during that period. In '65, when I was in the chancellor's office, Wofsy used to try to work on me [laughter] while on the basketball court, from the political point of view,

which was kind of interesting also. In any event, I remember sitting through the big faculty meeting on December 8. Followed the arguments with keenest of interest, because it came from all directions, and there were a couple of former German scholars. Karl Landauer spoke, and he was really worried about the presence of police on campus. He was thinking of European episodes in the previous Fascist era. The discussion was very impassioned, not always clear, but the vote, which basically rebuked the Strong policies about political action on campus and called for liberalization on campus, called for a reassignment of disciplinary activity to the faculty, which the regents rejected. I know that's an administrative responsibility, not a faculty responsibility, but the faculty wanted to take over the disciplinary role. They also specified that political activity should be unregulated, except for time, place, and manner. This was the resolution of the Committee of Two Hundred. I voted for it. Goffman voted for it. It was a huge majority for it. Something like ten to one, the majority for that resolution.

I remember one specific disturbing moment for me, is when we all left Wheeler Auditorium and went out the front steps, and there was this huge mob of students, of activists, waiting for us, because they knew that this was going to be a decisive meeting. As we emerged from Wheeler, there was this huge clapping and applauding and yelling and cheering and whistling for the faculty. I remember feeling almost nauseated at this. I wasn't opposed to the students, but I was sufficiently alienated from some aspects of that whole episode that I didn't want to be given credit for supporting it. That's the source of a little bit of shame, discomfort, that I felt in being cast in that role. I wasn't that enthusiastic about the student movement, even though I voted for the resolution. The excesses, the near-violence, and the fiasco-like quality of the whole thing had—in a way, one side of my reaction was to be turned off by the whole thing. Then here I was, cast in this kind of unequivocal, partisan role of endorsing the student movement, and I wasn't comfortable with that, though I was not, either, politically opposed to its aims.

11-00:30:18

Rubens:

I'm having a hard time trying to see why coming up with the resolutions was perform an endorsement of the students.

11-00:30:25

Smelser:

That's what the students interpreted it as. That's why they were cheering. I didn't think it was all that. It was certainly regarded widely as a victory for the students, and they thought it was a victory for sure. Within a matter of two weeks, Strong had been asked to leave office—not to resign, but to take a leave from office—by the regents. The other thing I did during this period was to, on my own hook, pay a visit to Regent Donald McLaughlin a very influential regent on the right-hand end of the political spectrum, though not an extremist. I knew McLaughlin socially, and it was also by accident. I had known his son, a historian who was in graduate school at Harvard at the same time as I. I had gotten to know him and his wife as a social friend. The son

had already come out to Stanford, I think to do postdoctoral work, if I'm not mistaken. I continued to see the son during this period. But I took it upon myself to go see McLaughlin, because I was already chagrined with the ineptitude of the Strong administration, and I actually went up and talked to him openly and straightforwardly about this, and actually said that he should not be chancellor of this campus, because I had seen such bungling that had been going on, and vacillating and changing of position, and uncertainty, that it was totally ineffective governance that was coming before my eyes. I said, "You just don't have a choice. This man can't handle this kind of situation." It was on December 2 that he was discharged. It was on the same day, I believe, that Martin Meyerson was named acting chancellor for an indefinite period. It was two days later that Meyerson asked me to come work with him as his specialist. The title was going to be Special Assistant to the Chancellor for Student Political Activity. That name designates exactly what he wanted me to do in the office.

11-00:32:56

Rubens:

You told us last week that it was not on the basis of your book, necessarily, that he brought you in. I can't see how that couldn't might have been—

11-00:33:08

Smelser:

I can't reconstruct this. Because of my role that I have described, I was not publicly identified in any way with any of the factions. I had this relationship with Lipset, that was the closest with anybody who was so involved, but I never was public in that visibility and I never gave speeches. I never went to these meetings. I was not a member of the Committee of Two Hundred. As far as public view is concerned, I didn't have a role. I didn't have a position, even. I think that might have played a role in Martin's choosing me. If he had chosen some hardheaded partisan from anywhere on the spectrum, that would have complicated that appointment. Possibly compromised it in some way from the beginning. I believe that I was told by a person I cannot identify—not because I don't want to, but because I can't remember—that Martin Meyerson was initially inclined to appoint Philip Selznick to that position, and that Marty Lipset talked him out of it and recommended me as a figure for that position. Marty knew me, my style. I never verified this. I never asked Marty if he'd recommended me, but I got that story. The role of my own research on collective behavior and riots and panics, which had come out in 1962 and was already a recognized contribution—I have absolutely no idea what role that played. The press made a great deal of it when I was appointed, and describing me as an expert on mobs and that kind of thing.

11-00:35:16

Rubens:

And liked your youthfulness.

11-00:35:20

Smelser:

The press thought I was young enough—I was thirty-four years old at that time.

11-00:35:25

Rubens:

They said young, athletic, and they commented on your bowtie.

11-00:35:32

Smelser:

I guess that was my heritage from Harvard. I was wearing bowties at the time. I also looked—can I use the word "clean-cut"? I didn't invite much stereotyping. I suppose those who didn't like short-cut hair—one particularly appreciated that. I was, in a way, a kind of mystery man. Students, after I was appointed, went around to my colleagues—the activist students went around to colleagues that they knew—Blauner and so on—and asked them what my political line was. Here was a person who was helping out, who was given a lot of responsibility in the area of political activity when that was the number one item on the campus. Where does this guy stand and what's he going to do? And so on and so forth. They went to Blauner. Blauner told me this story. He joked with them, actually—or teased them. He said, "Well, you know, he's been editor of *The American Sociological Review*" He said, "I would suggest you read the last three years of *The American Sociological Review*. You'll find out what his line is." [laughter] Which I thought was a very comical comment.

11-00:37:01

Rubens:

Well, also a kind of—is sanguine the word? He wasn't going to get too involved.

11-00:37:08

Smelser:

He didn't give them some kind of lowdown. I'm not sure Blauner knew what my politics might have been, even. He was just playing games with the activists.

11-00:37:19

Rubens:

I want to ask you, what are your politics at that point, but I want to ask it also in relationship to, did you have any graduate students who were involved in any of the meetings, who were on the GCC [FSM Graduate Coordinating Committee], or who were part of the Steering Committee?

11-00:37:39

Smelser:

Most of my contact with sociology activist students came later. I did not have any students who were centrally involved in it. I was the mentor of one student, by the name of Max Heirich. He was coming up with dissertation plans, just after the Free Speech Movement, and he decided to undertake a kind of sociological analysis of it as a phenomenon. He chose me as his supervisor. I directed that thesis. He wasn't really an activist. I believe he was sympathetic. He observed canons of objectivity and he didn't make judgments in his book [*The Spiral of Conflict*]. That was the closest I came as far as student links to the Free Speech Movement.

11-00:38:32

Rubens:

But Marty is not hounding you, is not trying to really convince you?

11-00:38:39

Smelser:

No, I didn't feel any pressure to be anywhere one way or the other. If you ask my politics on that—I mentioned my initial confusion. Then I had this absolutely thoroughgoing ambivalence about the movement itself, which I thought was somewhat justified but also getting into excesses, and I couldn't join sides with the administration, because I saw that as being so incompetent and wrongheaded in many regards—repressive, followed by being—

11-00:39:13

Rubens:

Provocative.

11-00:39:14

Smelser:

Provocative and vacillating. I hate to disappoint you, but I cannot articulate a definite political position that I had at the time. I suppose that was helpful to me because I joined the Meyerson administration. I didn't have many preconceived ideas about the angels and devils in my environment. I had a thoroughgoing kind of skepticism of all parties, by virtue of what my politics were. I suppose that turned out to be an asset once you entered a new scene that was open and confusing and no one really knew what was going to happen after that fateful December series of events.

11-00:39:58

Rubens:

Were there any of the four or five major leaders that you found particularly—if not attractive, at least impressive?

11-00:40:08

Smelser:

Of the student movement?

11-00:40:09

Rubens:

Of the student movement. And conversely, ones that you thought were just provocative or—

11-00:40:13

Smelser:

Yes, I had. My role implied that I had frequent meetings with the FSM Steering Committee, with individual representatives of the student activists.

11-00:40:25

Rubens:

As of '65, once you're in the position?

11-00:40:26

Smelser:

As of '65.

11-00:40:26

Rubens:

We'll get to your position, I think formally, in just a minute. Before that, did you have a strong opinion about Mario Savio and how he was able to electrify students? Or Goldberg, who I think you mentioned, was—

11-00:40:44

Smelser:

The ones I knew about were Savio, Goldberg, and Weissman -he was the graduate student kind of leader.

11-00:40:51

Rubens: Head of the coordinating committee?

11-00:40:53

Smelser: Coordinating committee. I guess that theme of ambivalence came in there as well. I saw and appreciated Savio's charisma, but I saw little hints of demagoguery that I didn't like in '64. Weissman, I thought, was a very clever politician. Sort of a Leninist type in the movement, and very clever. I saw him as probably calling the major shots strategically for the steering committee, just because he is a diagnostician of this and he was clever. He kind of had a good sense of the administration's weaknesses. I wouldn't say I had an admiration for him, but I had an appreciation for him. Then the third one I mentioned was—

11-00:41:55

Rubens: Art Goldberg, then earlier, you had mentioned Bettina Aptheker.

11-00:41:58

Smelser: Yes, I didn't know her at all. Lipset always tried to poison my ear about her, because he was so anti-Stalinist and anti-Communist at the time.

11-00:42:11

Rubens: He must have known her father, too.

11-00:42:13

Smelser: Yes, probably so. I didn't have a full diagnosis or a picture of all the activists. Later on, I came to know them all.

11-00:42:33

McIntosh: We've talked about how allegiance to Strong was eroding rapidly, but what about allegiance to Kerr at the time among faculty members?

11-00:42:47

Smelser: It was a complex situation. Kerr's reputation on the Berkeley campus during the years of his presidency had undergone some deterioration. A lot of it had to do with the Master Plan. The whole history of Berkeley faculty, and to some degree, administrative orientation, has been to regard itself as the special place in the whole system. Somebody once told me, "We would never have had the FSM if UCLA hadn't been formed." It was some kind of a joke, but nonetheless, Berkeley was put in the shade by the development of multi-campus system. Of course, the master plan added three new campuses. Consolidated system-wide, in a much firmer way than it had ever been done before. They superimposed the quarter system on the Berkeley campus to make it uniform system-wide. That was a huge source of alienation on the part of faculty. They had been dictated an academic schedule. It was symbolically huge. Kerr had used the unfortunate phrase, "Berkeley is only one jewel in the crown," and that, of course, rankled with the prideful Berkeley faculty. There was a kind of disaffection with Kerr after his years as chancellor, and then he went to the president's office. He had a different role. I think fully expectable,

given the exigencies of the two offices, that he would experience some decline in appreciation on the Berkeley campus.

However, there was an interesting phenomenon that was already there but became intensified during the Free Speech Movement, that individual and groups of faculty members would bypass the Berkeley administration and go talk to Kerr separately. This, of course, was mischievous as far as the local campus administration was concerned. But Lipset was one of the big ones. He was a buddy of Clark Kerr, through the Institute of Industrial Relations originally. He had his ear, and he talked to him all the time. There were others, too—groups and individuals who went around and—and Kerr was criticized, certainly by the Berkeley administration—Strong and the rest—for meddling during that whole time. There was a great deal of tension and division between the campus administration and the university-wide administration.

The whole thing broke down because different parties were going to the regents that shouldn't be going to the regents, given the line of authority of the institution. They were calling their favorite regents and complaining about this and that, and the regents were contacting the administration here with complaints. That shouldn't have been done. Should have gone through the president's office, but it didn't. Everybody sort of became individual agents, and that was one of the banes of my existence in working with Meyerson. The regents were forever placing these hostile phone calls and asking for this, that, and the other thing to be done. Just as I say, this whole lone ranger complex just kind of took over the institution during '64 and '65.

11-00:46:27

Rubens:

This is a banal question, but where was the president's office during this period?

11-00:46:32

Smelser:

University Hall.

11-00:46:32

Rubens:

That's when they established it.

11-00:46:36

Smelser:

Yes. It moved much later to Oakland.

11-00:46:43

McIntosh:

Just to stay on Kerr for another question, he's often understood in the context of the FSM as kind of cracking down unnecessarily. It sounds like, from your portrayal of him, that he is actually receptive to faculty concerns and maybe even student concerns as well.

11-00:47:10

Smelser:

There was a great discontinuity and contradiction in the popular attitude toward Kerr as either a liberal or a disciplinarian. He was widely cited as both.

He did engineer some relaxation of the rules about political expression, but at the same time he kept other rules. So he got it both ways. The Free Speech Movement regarded him as repressive enemy, whereas in fact he was a straightforward, thoroughgoing liberal who was trying to handle a difficult situation on campus. I always saw Kerr as being overly tainted by this authoritarian picture that was made of him, mostly by activists on the left. On the right, he was suspected—J. Edgar Hoover had a whole record on him as being too liberal his whole life. He was one of these tragic characters in the middle that got burned from both sides.

11-00:48:13

Rubens:

There's something about a kind of removed nature, at least his presentation in a time when things were so inflamed.

11-00:48:21

Smelser:

Kerr kept his cool all the time publicly. In subsequent interviews with Kerr, I learned, because it was after the whole series of episodes, and afterwards, Kerr brooded a lot about this period, and what he should have done and what other people did. It was not a very calm view of that crisis period in his own mind, even though he was able to keep his cool. He never blew it during the entire crisis, and he was masterful with the regents during that period. Deep down, he was really—rewrote that history hundreds of times in his own mind. I later became a friend of Kerr's. I became a bit of a therapist for him. It became almost—

11-00:49:22

Rubens:

A sounding board, at least.

11-00:49:25

Smelser:

Sounding board. He respected me. He thought I was a loyal person, which I basically was. But I found it, after a while, rather difficult to be with him, because he would continuously brood about the past and regrets and blames and other things that he experienced, in retrospect and probably at the time as well.

11-00:49:52

McIntosh:

In a way, it's sort of understandable, because so much of the history of the FSM is celebratory—really uncritically celebratory of the FSM. From the picture you're giving today, it sounds like there were excesses on both sides.

11-00:50:10

Smelser:

Yes. I didn't pick sides in that whole conflict, because I had impatience with everybody.

11-00:50:17

McIntoh:

Right. I can imagine being in Kerr's position, seeing how this is being entered into the history books, and being distraught at that.

11-00:50:29

Smelser:

Events like this don't end when they end. The controversy continues in how to write the history.

11-00:50:41

Rubens:

You talk about that Meyerson asked you within a couple of days to join.

11-00:50:53

Smelser:

Yes. The way it happened was that I was in Washington, D.C. on that weekend before the fifth. I was on the Council of the American Sociological Association, because the editor of the ASR was automatically a member, so I sat in on all the meetings of the governing board of the ASA at that time. On Sunday evening, I got this frantic phone call from Erving Goffman, saying that my children had been in a fire in San Francisco, but he immediately said, "They were saved and they're not hurt." Nonetheless, I left immediately. I got a plane that night and came the next day. They came to stay with me because they didn't have any home. Temporarily, they were out of a home, so they came to stay with me. My son nearly died in the fire. A very heroic fireman rescued him from inside this fiery building and carried him out on a ladder. As he was coming down the ladder, he fell backwards and broke his neck and died—the fireman. The story was written up the next day in the *SF Chronicle*, with pictures of my kids on page one, mostly because the fireman had died in his heroic act.

11-00:52:23

Rubens:

This was pretty traumatic for your son, let alone you and his mother.

11-00:52:25

Smelser:

Well, both of them. Daughter, too. She was disturbed by it. They were six and four at the time. It was a very near-calamitous situation all the way around. Anyway, it was that day Martin called me up at home. Didn't tell me what he wanted. Said, "Could you come and see me?" So I was able to go in that afternoon, and he didn't mess around at all. He'd read about my kids and was sympathetic. I didn't know him very well. I only knew him from that one trip up to Davis, and I didn't get to know him then. It was a bolt out of the blue. He just said, "Would you be my special assistant for student political activity?" You can imagine my reaction. I was totally blown away. I regarded myself as relatively invisible in this whole period of conflict, even though, as I've reported, I had some little accidental roles from time to time. He simply put it to me, would I come help him in the chancellor's office in this position? Didn't describe what it should be. There was no job description that he handed me, or anything like that. I said yes. I didn't think it through. I think if I had thought it through, I might not have.

11-00:53:45

Rubens:

Because?

11-00:53:54

Smelser:

It's not exactly what you'd call a career-advancing move for a professional academic. That is worse than going to editorship. You're going to be swallowed up by administrative assignment, an unknown duration, and unknown consequences. Secondly, if I had thought about it, I would have seen how many of my colleagues had gotten tarnished during this—Lipset was a very controversial figure. He was really blasted and tainted and cursed, especially by the left. I think that was one of the reasons he decided to go to Harvard. He had been so wounded on the local scene that he would, in a way, get out of that particular controversial role that he had taken on in the campus. And others, left and right, who had made the wrong move at the wrong time—you get seared. Here I was, taking on this job that was right in the center of everything, blithely assuming it wasn't going to happen to me, which is a really stupid assumption. But I didn't have any idea that I would be damaged. Unrealistic assumption on my part.

11-00:55:08

Rubens:

Was there a specification of how long the service would be?

11-00:55:12

Smelser:

No. He was acting chancellor.

11-00:55:14

Rubens:

So he had to wait, too, to see what—

11-00:55:15

Smelser:

He was on a string. It was just assumed that I would come in. There was no indication whatsoever as to how long it would last.

11-00:55:26

Rubens:

Were you still teaching then?

11-00:55:29

Smelser:

I was supposed to teach heavily in that term. I was going to teach that undergraduate theory course, and then I had already been drawn in to be a regular teacher of the graduate theory course. Those two together were a very substantial teaching load. Meyerson asked Philip Selznick, the department chair, to excuse me from teaching the graduate course, and Herbert Blumer took it over. Blumer actually got me back in it for some guest lectures, but that was only a tiny commitment. I had one course off, no stipend, and no other changes in my circumstances. It was one of these informal commitments. I didn't object to not being rewarded in any way or for doing it. It just seemed natural that I wouldn't receive extra administrative pay or something like that.

11-00:56:25

Rubens:

How did you go about preparing yourself for—

11-00:56:28

Smelser:

Didn't have any time at all. Had no preparation whatsoever.

11-00:56:30

Rubens:

Once you were there, though, how do you start thinking about what you're going to do? You must have had the disposition of the students that had been suspended, and the others that were facing court trial. That must have been one of the major—

11-00:56:51

Smelser:

Several things happened in rapid order. First of all, the press got interested in this appointment, and I was scheduled to give a press conference almost immediately, which I did. That's where all that talk about my being young and bowties and whatever came out of. I was totally unprepared for this press conference. You can imagine I'd come in there, and four days later, here comes the press corps, asking—

11-00:57:21

Rubens:

Dick Hafner [Richard, UCB Public Relations] didn't sort of field this for you?

11-00:57:24

Smelser:

No, no. He was sitting there. Hafner and Ray Colvig were there. I'm sure they were nervous as anything about what I might say. Apparently the way the press wrote it up was favorable and benign, and so it passed. But it happened to me. I didn't have a chance to think it out. We had a meeting with the steering committee of the FSM within a week I was in there. The time, place, manner rules were already being almost formulated, and I got involved in those. In a way, it began necessarily as a reactive role. I had to be introduced to the senate. Everything happened all at once. I certainly didn't plan these things. They were all part of the picture. I was just in the fray immediately.

[Begin Audio File 12]

12-00:00:02

Rubens:

So you're positioned inside the administration, with no preparation. How were you going to position yourself?

12-00:00:26

Smelser:

I believe I told you the anecdote in my seventh grade, in which my teacher called me aside and said I should become a diplomat. Well, this was a kind of test of that strange comment, because the administration was held in very low regard by virtue of its behavior and collapse. Meyerson was taking a much more sympathetic public line toward the students, without giving specific promises of any sort. He was making favorable noises. That was the tone that was rather set. I was certainly highly attuned to the need for not being provocative and not being preemptory in anything we did. We were walking on eggs. I knew that immediately, that the administration was walking on eggs after this period of crisis. The big question was to have an orientation to the students that would be to listen. Not necessarily to give, but to listen, to have an open door and an open ear. To meet when they wanted, and how often they wanted, on any subject they wanted. This was how I kind of defined it. As it turned out, I had as much contact with other faculty and regents and some

political figures around the state as I did with the students because of the nature of the interventionist forces that were operating on the university at the time.

From the standpoint of the students, the thing was, it was partly a containment operation, partly a diplomatic role, partly an assignment not to get us involved in the same kind of mistakes we got involved before, and partly to respond to their provocations insofar as they were there, and they were, in a way that did not get us involved in this heavy, confrontational, rigid posture with the students. I cannot say I worked these goals as a written-down philosophy. That whole series of attitudes evolved on my part, but it was also partly my initial disposition of an even-handed person who would show a certain amount of sympathetic attention to a great diversity of demands that were going to come our way.

12-00:03:03

Rubens:

Did you meet with Meyerson in those early days?

12-00:03:06

Smelser:

All the time. I was kind of sitting on the edge of his office.

12-00:03:11

Rubens:

They gave you space?

12-00:03:13

Smelser:

I had a space just next door to his. He defined the role—I defined the role—I opened the door any time I wanted. It was that day-by-day. So I was very close to him.

12-00:03:28

Rubens:

Similarly, I would think you would have quite a bit of relationship with Hafner and Colvig. They were monitoring, at least, or trying to put out the fires about how the university was perceived.

12-00:03:40

Smelser:

I developed an instant, positive relationship with Hafner. It was just one of those things. It immediately became a kind of humor-filled relationship as well. We were both sweating all the time, but we kind of knew what was going on. I, one day, sort of said, "This isn't a chancellor's office. This is a military operation. You're the general." From that time on to this very day, I always call him General. There was a little teasing. We had a lot of respect for each other. He was a very sensible man. I considered him my great ally and friend and supporter in the chancellor's office. And Colvig, but not to the same degree. I later became closer to Colvig. During that time, I got to know him, and liked him and got on well with him as well, but didn't become personally as close as I did with Dick.

- 12-00:04:44
Rubens: One of your initial concerns was how to—or should you—intervene on behalf of the students with their upcoming trial.
- 12-00:04:55
Smelser: The students had been arrested on December 2. A lot of them had been put in Santa Rita Prison and then released, but charged to appear for a trial under various charges of disruption.
- 12-00:05:08
Rubens: Trespassing.
- 12-00:05:10
Smelser: Resisting arrest. There were all kinds of charges involved. Six hundred were charged. Two hundred were released because they either didn't resist or they walked out. Six hundred were on trial by Judge Crittenden. Two hundred and forty-five faculty members signed a petition to the judge, "Drop the charges." That was in front of us. We also had—
- 12-00:05:38
Rubens: I assume you didn't sign this, and most of your colleagues you talked about didn't?
- 12-00:05:45
Smelser: I didn't sign it.
- 12-00:05:46
Rubens: I assume Lipset and—
- 12-00:05:49
Smelser: It was the left. The left faculty signed it. Crittenden rejected the petition. The FSM raised funds for defense on the part of the students, which I don't think got very far. There was a group of parents called the Parents Defense Group or something of the students. It was a political thing. The students, the activists, at a certain point, as one strategy, decided to work on the chancellor's office, to get us to influence the court to drop charges. That was where we came in. Within a week after I was in office, they called up to schedule a meeting with me to bring two lawyers and a group of the activists. Savio was there. Two other of the steering committee were there. They came to put pressure on us to either contact the prosecuting attorney's office or the judge, or both, to get the charges dropped. That meeting occurred two weeks after I was in the position. I did have apprehension. I did have the good sense to ask a lawyer, a member of the law school faculty who was on the emergency executive committee, to come to that meeting, because I didn't want to be there with a bunch of lawyers and not have a lawyer present.
- 12-00:07:16
Rubens: Do you remember who that was?

12-00:07:17

Smelser:

Yes, Arthur Sherry, senior law professor in the law school. Very sensible man. It was a tough meeting. First of all, the lawyers talked all the time, saying they were not going to plead guilty and we're not going to plead nolo contendere. "We consider this as a non-guilty situation, and we're going to plead not guilty. Furthermore, we've come to appeal to you to influence the judge and the prosecuting attorney to drop charges." My main strategy there, I guess was in keeping with what I said a few minutes ago, was to listen. To not promise anything, but not to reject anything either. Just saying, I'm here as an agent of the chancellor. I'm talking with you. I'm going to take seriously what you have to say and I'm going to talk to the chancellor about it.

That did not stop the meeting from becoming very abusive. The second half of that meeting was extremely abusive, with not the lawyers so much as the students telling us that if we didn't do this, the movement was going to gain strength. That they were organizing nationally and even internationally to protest this. That they were going to bring countercharges, they were going to sue the university. Just a whole massive range of charges. I sat there listening. Then at the end, I said, "I've listened to you. I'm going to talk to the chancellor. The chancellor is promising he's going to consider seriously what you've brought to me. I'll talk to your lawyer when I have something to say." That was the end of that meeting. Then I wrote a long and quite decisive memo to Martin, saying we ought not to intervene here. We let justice take its course. I do not believe any of these threats. I assured the chancellor I thought all these threats were empty and nothing was going to happen. That this was, in a way, a kind of desperation move on the part of the students at the last moment, that they hoped to avoid a trial. Martin accepted.

12-00:09:30

Rubens:

Why were you so convinced that nothing more would happen?

12-00:09:36

Smelser:

I have to say in response to your question, it was a kind of gut feeling. I got a sense of hollowness about all this. I knew enough about movements to know that they don't electrically magnify into an international reaction, and particularly after the big drama has already happened. There was a kind of unarticulated sense on my part that we were being bullied by these threats. It was a chance guess. I couldn't be 100 percent certain. Your question suggests that. I couldn't be 100 percent certain that we weren't going to precipitate a crisis, that we wouldn't get another sit-in, that we wouldn't get some kind of an action. There was always that possibility. That was one of the uncertainties of that office. But at this time, I just made that bet.

12-00:10:28

Rubens:

Plus you were looking at instances where you could have acted differently, and so were proposing the administration not play a provocative role.

12-00:10:38

Smelser:

I was sort of living out that philosophy in that situation. I don't want to communicate to you that I was being calculative at every stage. You're always feeling your way, and there's always uncertainty, and you're always uneasy, and you always have second thoughts. This decisive memo I wrote to Martin was a distillation, but a risk. As it turned out, it went into thin air. All the threats went into thin air. The trial took place in a routine way. I also had the view that it would have been mischievous on the part of the university to embroil itself in the justice system. That was the most general conviction I had. Of course, you had all these other strategic and tactical things going on at the same time, and the threats and everything. It was a very complicated little episode. I don't want to make it seem as though it were simple, despite the fact that I was pretty definitive in my reaction to it.

12-00:11:40

Rubens:

Sure. Beyond that, you had to face very early on the Filthy Speech Movement.

12-00:11:52

Smelser:

That was in March, so a month-and-a-half later. I was sitting at home, at evening. I believe it was an evening my kids were over. I got this phone call from the office of the university police. They said, "We've arrested this young man on the steps of Sproul Hall, who's carrying this sign." It said, Fuck, but he said: "It's a verb [laughter]—on it. And he's under arrest." I cannot describe how mortified I was with this news, because I knew how explosive it was at the instant they told me. Because it had to do with disciplining, cracking down on some—he was a kind of—

12-00:12:42

Rubens:

He was a non-student.

12-00:12:43

Smelser:

Non-student. He was from some other part of the country. He was a kind of pathetic-looking little guy. They landed on him with the full force of the law. My feeling at the time was why didn't they just take him off the campus? Don't come back. If you do this tomorrow, we're going to throw the book at you. That was the way I would have wanted to handle it. Instead, we were faced with this situation. A certain group of the activists immediately took it up. Art Goldberg was the guy on the steering committee, and a few of his buddies took it up. He came storming into my office the next day with his friends, really frothing at the mouth, saying, "What are you doing? This is a crackdown on everything we've stood for," etcetera, etcetera. Giving me the whole abusive line, with threats, all kinds of threats. He said, "We're going to have a group of a thousand people carrying those signs tomorrow on campus. What are you going to do about that?" This kind of stuff immediately, all of which would be terribly embarrassing to the university. Here he was, ranting and fomenting at us for having done this, and challenging, threatening. I remember only two things I said to him. The first one was without any confidence whatsoever. When he said, "What are you going to do if we send

the thousand students carrying these signs?" I said, "Well, I can't tell you." I said, "There could be a thousand arrests."

12-00:14:24

Rubens:

But he wasn't saying, under the time, place, and manner, we'll have a rally at Sproul Hall?

12-00:14:31

Smelser:

No, no. It was just going to be an event. It was just going to happen. A student protest against this heavy-handed, totalitarian act that the university had organized. I finally got him in a conversation. I just sort of asked him, I said, "Do you envision any limits on public behavior at all?" I just put it to him. He wouldn't discuss it. He was just mad. Unorganized. My sense is, in retrospect, not that that threat to arrest everybody had much meaning to him, but they couldn't get the troops. I didn't think a thousand students, or a hundred students, would—is this really worth doing? To most students, even activists, it seemed kind of trivial. He picked up this ball and he embarrassed the Free Speech Movement terribly by making this a matter of speech, freedom of speech. He wanted this obscenity thing seen as a matter of free expression. He put it under the cloak of free speech and embarrassed everybody else. Instead of the thousand sign-carriers, they had this obscenity rally, in which they behaved like cheerleaders, spelling out the words and yelling and reading out loud from *Lady Chatterley's Lover* and all kinds of things. Got several of the students arrested, including Goldberg, and charges brought by the university on the basis of the day. We were still in a state of chaos. I didn't know what was going on and what to do about this. It was a completely anomalous sort of situation as far as I was concerned.

Martin had the correct idea immediately, intuitive idea, that if the university didn't move in to discipline these students, we were finished, from the standpoint of attacks from the right. That was just the easy political inference made. The right was quite hungry. They didn't like Meyerson that much. They thought Meyerson was not a good appointment because he was too soft and he didn't make the right kind of noises. Strong was still sitting in the wings. We started the disciplinary machinery immediately. There were two committees who were disciplinary committees. One was the longstanding beer and sex committee that often went after fraternities when they had parties that were too wild and so on. Then we had a special committee that was set up under pressure from outside, for political conduct—a disciplinary committee for breaking the time, place, and manner rules.

12-00:17:17

Rubens:

These were academic senate committees?

12-00:17:18

Smelser:

These were academic senate committees. They were faculty committees. We sent the charges to those committees, and neither one of them would take

them. They said it's not our bailiwick. The faculty at its worst, I said. [laughter] A phrase I thought was—lecturing on principles of navigation while a ship was sinking. Figuring out these jurisdictional niceties was not what we needed at the time. So Meyerson did a clever thing. He formed a new special committee for this case alone, made up of two members of each of those committees and a new chair, and said, "You're responsible for this." That eased the pressure from the right. The regents had gotten on to Meyerson and Kerr to either discipline these people right away, unilaterally—kick them out—or else the regents were going to do it themselves. That's what precipitated Kerr and Meyerson to resign. Said, you're messing in our affairs. But setting up that disciplinary committee was an important part of that picture, because it meant we were taking some responsibility for this.

Now, what happened is this Free Speech Movement began denying that they had anything to do with these episodes. Savio was continuously on the plaza, denying that they liked it or anything. The faculty met en masse and denounced the whole thing. About a thousand members of the faculty denounced the whole thing, and supported Kerr and Meyerson in their role and requested they not resign. The other side of what happened was that three days after the filthy speech demonstration there, the *Oakland Tribune* came out with a furious attack on Kerr and Meyerson, including a long, really vicious article by Strong, attacking the administration since he'd left, and a strong editorial from the *Oakland Tribune* to reinstate Strong.

12-00:19:38

Rubens:

So Strong actually had an op-ed piece?

12-00:19:42

Smelser:

Yes, that whole blast that came out. It was a couple of days after that obscenity rally. We were getting squeezed hard from the right. The left was objecting to any kind of discipline of any students in the whole thing. The only thing for us to do in that circumstance was to make sure that committee did its work right. I was in contact with that committee all the time. The head of the committee was John Whinnery, the engineering professor. Eminent man. Beautiful man to choose for the position, that delicate position. March was the roughest month, because I was getting bombarded from all sides on this issue, mostly from the side that wanted to string these guys up, from the right. I even got a series of telephone calls from the governor's office, Governor Pat Brown. It was one of his assistants, and he would call me every few days. He says, "How are those hearings going?" He didn't say to convict these guys. He just said, "The governor is very interested in this case, and he's wondering how"—he gave an indication he wanted expeditious resolution of the issue. He didn't prejudge or anything. I think it was that Pat Brown, a friend to the university, he was interested in encouraging us, I believe, to save our necks by disciplining, but he didn't say it, and his assistant didn't say, "Get them." He said, "We're interested in the progress of the thing." His call was identical every few days.

If the university gets embarrassed in those days, then Brown gets embarrassed, because he's such a noted friend of the university. That was the political backdrop to it, I'm absolutely certain. But I would always say, "Thank you for your call. I'm monitoring the situation." I did keep in touch with Whinnery that I was getting these calls. I kept him informed about this, though I didn't try to influence. I didn't try to say, "Convict these guys." I was interested in two things. One is that they move fairly rapidly, because the turmoil was still going on. The campus was under bombardment during that month for its moral laxness. You can imagine the number of different accusations and complaints. I was with the governor that I wanted it to be done expeditiously, but at the same time, make no mistakes with regard to due process. In a way, they're kind of contradictory messages that I was giving to Whinnery. This has to be clean. Has to be absolutely pure. We cannot be provocative in this thing that was extremely delicate. But we have to be fast.

In the meantime, FSM steering committee, which was still somewhat floundering in its organization, was not endorsing the Filthy Speech Movement, but they were saying, "Do not discipline these students." They didn't want discipline. They saw what was happening. To be able to discipline these students and get away with it was an enormous symbolic move for the campus, because they had been helpless to discipline anything for a long time because of their being so discredited. This was a move toward reestablishing the campus authority in some minor but important, symbolic way. The students knew that. Maybe they didn't put it that way, but they knew it, and so they were bombarding the regents with communication to say, "Stop this travesty" and so on and so forth. They could not escape the taint of the Filthy Speech Movement, because they were saying, "Don't punish these people. Meanwhile, we don't agree with what they did, but don't punish them." They tried to bully the regents. Rallies and things were going on. But that is the time when the Free Speech Movement fell apart.

12-00:24:56

Rubens:

Dissolved, yeah. They were trying to set up a free speech union.

12-00:25:00

Smelser:

Subsequently, Savio resigned. Many of the members of the steering committee had already left town or resigned or done something. It was pretty much kind of falling apart, and this last episode of the Filthy Speech Movement—the nail in the coffin. Shortly afterwards, Bettina Aptheker and a few of the activists said, we want to set up a free student union, based on a trade union and endorsing the idea that when they had a political complaint, the only weapon was a political strike. It was kind of the European general strike model that they were building in. They charged twenty-five cents for people to join. They got a thousand members or something like that. It died within a few days. My own judgment of that movement was that it was a desperation movement, an effort to save some kind of face in troubled times. We didn't take any provocative attitude toward that. We said, "Okay, register

as a student group. If you want to have speakers, let's follow the regulations—time, place, manner. We'll do everything we can to help. We won't give you space in the student union, but we won't do anything to stop your activities." As it turned out, it lasted about two weeks, and then never was heard from again.

12-00:26:29

Rubens: Eclipsed by then the antiwar activity that—

12-00:26:33

Smelser: Well, then we had the—

12-00:26:35

Rubens: Just before we get to that—I'm sorry to interrupt you—when you talked about Brown's office calling you fairly regularly, wanting to keep abreast of what's happening, I was wondering if the administration was reporting to any other agencies. The FBI was known to have come during the rallies that fall and taken names and keep notes, but I've never heard whether the administration itself also gave information.

12-00:27:03

Smelser: To the best of my knowledge, we did not. There's always the possibility that I was not informed of that, because it is a subterranean activity. It would be criticized if known, and would have been carried out in secret, even from some of the staff members of the administration. To answer your question, I was unaware of any communication we were having with any federal or state security agency about the campus. They were active for sure. We did, to the best of my knowledge, not cooperate.

12-00:27:33

Rubens: I also say it because one of the memos you wrote in terms of how to handle discipline during the spring of '65 was to stop keeping as many records on students.

12-00:27:43

Smelser: Yes. That was not so much to federal agents as to employers that—we released their records, which may have had disciplinary action on them, only with the written consent of the student. That was something I believed very strongly in.

12-00:28:01

Rubens: Surely, there must have been some students who suffered for their activity from their professors' letters of recommendation.

12-00:28:11

Smelser: The answer is maybe, plausibly, but I say that without evidence.

12-00:28:16

Rubens: Okay.

12-00:28:19
McIntosh:

On that note, I'm curious how your appointment to this position changed your emotional involvement with students you're meeting with, if at all. Do you remember sort of consciously—

12-00:28:36
Smelser:

I'll tell you what changes in my emotional life on that topic and on a couple of others as well. I obviously had personal reactions to all these students—activists—who came. Mainly, they were negative, because I was being abused all the time by shouting matches and accusations about what the university was up to. It was a negative aura about the whole thing. They were playing their role, and I was as well. I didn't like it particularly. Some of the students, I actively disliked. Goldberg was an example. I saw him as abusive. Later on, when I met and dealt with at great length Jerry Rubin, the activist—not a student, but an activist in the antiwar movement—I took an extremely negative view of him as an opportunist and an exploiter as well. I kept these things under my pillow. I did not really express much affect toward any of these students. Interestingly enough, I came to like Savio. He gave me as much grief as anybody in terms of harangues and accusations, but somehow the guy seemed to have a sensitivity, a human side that he would let out. I sensed the man was suffering often in where he was. I was able to develop a positive identification toward Savio, despite the fact that the public confrontations were almost all just that—confrontations. It's very unlikely you develop a positive attitude toward someone.

12-00:30:29
Rubens:

Did you see him representing the ideals or being more idealistic than—

12-00:30:33
Smelser:

Well, actually, more capable of human feeling, I'd say, rather than getting involved in some abstract idea. I have to tell you the most dramatic change I underwent is that I developed a—I have to call it a love—for the campus and the University of California. Being in this fraught role, and seeing it in trouble, and expanding my horizons outside my immediate department, and seeing other people, and seeing how it worked, and liking a lot of people involved with it—previously, I looked at Old Blues as being kind of a quaint and somewhat irrational group for all their great proclaimed love for the university. I began to lose those kinds of feelings and my love for the institution became consolidated.

12-00:31:26
Rubens:

You're speaking of the alums?

12-00:31:28
Smelser:

Alums. Blues, Old Blues, especially those interested in athletics. I didn't exactly become an Old Blue, but I developed what I really called a love for the campus that endured for the rest of my career. It was this crisis atmosphere, the institution struggling for its life, and I was in the middle of it,

playing some kind of role that I had to regard as helpful in that regard, that I developed this real affection and love for the campus.

12-00:32:00

Rubens: That was just palpable. You felt it then.

12-00:32:03

Smelser: I felt it then. I feel it right now. I wouldn't have exactly predicted it would have happened.

12-00:32:10

McIntosh: This is very interesting to me, because as you've been talking over the last hour-and-a-half, I keep on coming to the conclusion that you are in such an unenviable position, basically, during this movement. Especially in your position that you're appointed to, where you're sort of taking it on all sides. I kept on wondering basically where you go for solace, or where the satisfaction is during this period in your career. It sounds like one way that you did that was by sort of manufacturing a love for—

12-00:32:43

Smelser: That's a very astute observation, and I think probably correct. I have to tell you that I was given fantastic support by Meyerson. He was really a wonderful person to work with, and he was always praising and giving me credit—more than I deserved. It was his own personal style. I didn't consider him to be faking or anything. He just was very dedicated. There was a lot of reward in that. I suppose if I reexamine my feelings at the time, that I probably developed a secret sense of being heroic in adverse circumstances. I never announced this to myself, even, fully.

12-00:33:34

Rubens: This was not something that came up in your analysis?

12-00:33:42

Smelser: After I left that office, I did a lot of reflection about it in my analysis. I found that that narcissism, if I might describe it that way, was very real, even though I didn't acknowledge it to myself fully at the time.

12-00:34:02

McIntosh: Just one last impression that I'd like to run by you and get your reaction to. It seems to me that one of the main moves that you made in this role is to push the university towards a neutrality, or basically getting it away from these provocative—

12-00:34:25

Smelser: This was a guiding rule of thumb philosophy—element of the philosophy I had, yes.

12-00:34:34

McIntosh: In a way, it seemed very effective, right? Get the university into a neutral position, allow the justice system to do what it needs to do, allow the

politicians to do what they need to do, and then the movement itself, the FSM, will essentially deteriorate.

12-00:34:50

Smelser:

Let it die was an element that was often in my mind. I saw that it was a confused movement. On a scholarly basis for this, I studied lots of movements that had gained their aims. For example, the Temperance Movement after the prohibition was passed. They got what they wanted. Other similar movements that had really gained what they wanted—and they all got confused as to what to do next. The Free Speech Movement, of course, splintered into a lot of other types of activity. Reforming student government. The Filthy Speech Movement was a little side issue. There were a few who were interested in reforming the large, impersonal lecture system and in other educational reforms. It went off in a lot of different directions. In a way, the best guiding philosophy is, let it happen. Don't get in there and provide a basis for a new unity for something that is, in fact, falling apart. Sounds too calculating on my part. I didn't have a Machiavellian outlook toward all this. You hardly could, as things were changing every day. But I semi-articulated a philosophy that contained those elements, yes.

12-00:36:20

McIntosh:

The splintering that's going on in '64 and on into '65—is the civil rights movement inflecting this in any way? Is there talk about race going around the campus, or is it—

12-00:36:36

Smelser:

Yes, yes. It was only a couple of years later that the campus really got into a thick problem over admissions and the Department of Ethnic Studies and things of that sort. It was kind of a constant pressure, not dramatic. The civil rights impulse was very, very strong in between, say, '59 and '63, when a lot of the demonstrations were over hiring policies in the Bay Area, and the students were actively involved in it. Concerns with civil rights got overshadowed by the student protests, but you also had coming in, in that very important transition into '65, the growing alienation with our involvement in Vietnam. You also had the beginnings of the more radicalized feminism that dominated the scene through the seventies. A lot of things were going on at the same time. Civil rights during my little episode was not anywhere near the top of my consciousness.

If you'd like me to talk about the antiwar demonstration—

12-00:38:01

Rubens:

I wanted to just return for one minute before we do that—are you starting to have defections in the faculty by the spring of '65? The sociology department, is that when people are saying, "I've had it, I'm leaving"? Isn't the number six that ultimately leave?

12-00:38:21

Smelser:

I haven't a count for you. I could probably add it up by remembering who left. '65, Lipset got an offer from Harvard at that time and didn't take it. He wrote them a heated letter about his commitment to public universities. He did take it the following year. They renewed it the following year. I think his commitment was weakened by the fact that the public university had turned on him. He was pretty alienated by that time.

12-00:38:52

Rubens:

He had gotten a book out with—it's with Sheldon Wolin, isn't it? "The Troubles at Berkeley." I couldn't remember.

12-00:38:57

Smelser:

Did he do that with Sheldon? I can't believe it. He was such enemies with Sheldon.

12-00:39:00

Rubens:

It seems so strange that the two would do it, but I thought they did.

12-00:39:03

Smelser:

Maybe an edited book. You can maybe get away with that. He certainly couldn't have written a book, because they were so at different views. Wolin was very far left on the campus scene. Let's see. Goffman left, fall '65 or '66. Goffman was an early leaver. Glazer went about the same time Lipset did, to Harvard as well. Peterson left later. He went to Ohio State. Those are probably the ones that went to outside institutions. Within the university, Bendix went to political science. Later, Harold Wilensky went to political science. Philip Selznick went to the law school, and Guy Swanson went to psychology.

12-00:39:54

Rubens:

I don't know why we haven't mentioned him.

12-00:39:55

Smelser:

He was a very eminent social psychologist. He was out there on the right. He came just after the Free Speech Movement, but he was on the right, along with Davis and Peterson.

12-00:40:08

Rubens:

And so all of these defections, do you trace them to an unhappiness with what was going on at the university?

12-00:40:16

Smelser:

I would say most. I think Goffman's was the least. He wasn't a very political man. He was given a fantastic offer by University of Pennsylvania—Benjamin Franklin professorship and they offered him half-time teaching for the rest of his life. He came to the department and asked for that, and we wouldn't do it. I was Goffman's best friend at the time. I told him he was crazy. You can't just come in and demand half-time. You can get money paid by some research agency or some institute or something like that, but you just can't come in and do this to your colleagues without any support outside. You can't just demand

it as a very special member of the faculty. Your colleagues won't stand for it. Wilensky and I, who were his pals at the time, said, "We're not going to support this demand on your part." He went. It wasn't a political indignation. It was a careerist move on his part. I know that Lipset and Glazer and Peterson were all frankly political. Davis, actually, sort of began spending all his time over in the demography program, so even he, in effect, kind of departed.

Most of the departures were from the right. We had an augmentation of the power of the left, and a shrinking middle, of which I was a part. This goes all the way up through the seventies.

12-00:42:00

Rubens:

All right. If you would, do you want to finish talking about the next challenges you had as an administrator.

12-00:42:06

Smelser:

Yes. The last biggest challenge I had in office with Martin was the Vietnam Day Committee. The war had escalated up through the late winter and the early spring, and we were sending troops over there and bombing by this time. An absolutely natural turn. One of the ways in which the movement, quote, "splintered" was to be drawn off into antiwar activity, and with a very strong impulse, even from an early period. That was when a certain number of faculty members—Stephen Smale was the most active, and there were others—interestingly enough, a lot of them from science were dismayed at the war—well, other people in the rest of the faculty as well, and students—turned more in that direction. There's no mystery about it. Obviously, the draft was on. This was an escalation of the war that was unpopular, and as it turned out, unwinnable. It was just a natural scene.

What was an interesting feature of this is when Jerry Rubin appeared on campus. He was not a student. He enrolled on a limited student basis. I was told he came to listen to my lectures on Marx in my theory course, though this was a class of three hundred and fifty people at the time, and I couldn't have recognized him. Then he got bored when I stopped talking about Marx and left. That was the story I got about Rubin. Anyway, Rubin and Stephen Smale formed an anti-Vietnam—called a Vietnam Day Committee, which was going to hold a really massive rally on the campus in early May. It fell to me to negotiate with them and another small group of faculty and students, activists, on exactly what this should be. They wanted an absolute mammoth sort of thing, in which we would suspend all political rules regarding time, place, and manner, and this was going to be a huge thing. They came at me with many, many demands on setting this part of the campus aside for them, setting that part of the campus aside, doing this, leaving the cafeteria open. Just a huge show, it was going to be. I was the sole person negotiating with this group. It was a somewhat overwhelming experience, because of the numbers of demands. We turned down almost all their demands for being treated specially, with a couple of exceptions. At the same time, bad blood began to

develop, with Rubin in particular, but Smale didn't help it out. They would come in with one demand and we'd say no, and they'd come in with a slightly different demand. No. "Well, we're going to do this anyway" and so on. All sorts of threats. You basically don't have terribly much power under these circumstances, when you're dealing with a group like this.

It got built up. Word got around that the event was being taken over by the left, and a number of the speakers who were more moderate or right-wing pulled out. McGeorge Bundy pulled out. Robert Scalapino on the campus pulled out. Eugene Burdick pulled out. So it did become left, and a protest rather than a teach-in. Then, on the day of the events, they proceeded to break every understanding and every rule that I had made. It turned into a circus with chaos. Press didn't like it. We got criticized for it. I was feeling very bad about this. The main result that came out of it was, besides my own personal disappointment that the university was being embarrassed by this—I didn't like the war, but I was not dealing with that aspect of it. I wrote a very long memo to the incoming administration on exactly how this had to be done, on the certain legal safeguards that we had to have, certain financial safeguards we had to have. We lost money on it. They used university resources and wouldn't recompense the university, that kind of thing. So I wrote a very long memo for the next administration. That was Roger Heyns' administration.

12-00:46:50

Rubens:

At what point was Heyns selected to be the chancellor?

12-00:46:53

Smelser:

There was a big meeting of the regents in June. The faculty loved Meyerson. There was a feeling of hope that he was going to be made regular chancellor. The regents did not appoint him regular chancellor. I never got all the reasons. I wasn't hanging around the regents at the time. Some regents thought he was too soft. They still had this right-wing frame of mind. There was some sense that he represented himself as being tougher than he really was in order to get support from the regents, who were not receptive. There was a rumor of anti-Semitism that went around, which I could never verify one way or the other. Maybe some regent made a comment. I don't know. But he wasn't appointed, and much to his disappointment. He was saddened by that, not being able to do it. He was unappreciated, I believe, as to the accomplishments that he made during his chancellorship. He was back on the faculty for a year, and then got chosen as president of State University of Buffalo, and then went on to a long presidency at Penn.

12-00:48:03

Rubens:

Did that mean you knew your administrative position was over? You could have been kept on by—

12-00:48:07

Smelser:

I had a sabbatical coming. I was going to get out of there anyway. I decided in my own mind that that year was enough—or that eight months was enough. I

declared my intention even before that whole decision about the chancellor was coming up, that I was going to go on sabbatical. I had been here six years. I did go on sabbatical that coming year. I didn't have any ambitions for office. In fact, I got a couple of administrative offers at the end of that period, based on the word that gets around. The vice chancellor at Santa Cruz.

12-00:48:44

Rubens: In June of '65.

12-00:48:47

Smelser: '65, yes. McHenry asked me to be his number two man down there.

12-00:48:51

Rubens: Was that attractive at all?

12-00:48:52

Smelser: No. I just said no. Afterwards, when I served in Heyns' administration, he wanted me to be dean of the College of Letters and Sciences, and I turned that down as well. When I come back from my sabbatical in '66. That might be a good place to finish my stint with the administration.

12-00:49:10

Rubens: I think we have ten minutes left today. I'm wondering if you'd be willing to talk about this trope. You mentioned that there might have been—no proof or evidence—that there was anti-Semitism against Meyerson. I know that, in the late fifties, the Department of History at Berkeley was not hiring Jews. Borah was in the speech department, and he was very upset about this. I'm not saying that I have evidence other than people's recall, yet the sociology department was heavily—

12-00:49:46

Smelser: Sociology is a field that is populated by ex-Protestants and Jews, largely. Not many Catholics in the field. It was not a visible fact in sociology that there were a lot of Jews on the faculty. I don't know about the history department. That's news to me, what you now say. I would have to just see who—people like Larry Levine, when they joined. They came a little later.

12-00:50:18

Rubens: Yes, Levine came in in '62.

12-00:50:21

Smelser: So no, I cannot comment. It could be that there was a bastion of that kind of conservatism, maybe.

12-00:50:30

Rubens: Was anything ever made of an excess of Jews in the sociology department? Did you ever hear any grumbling?

12-00:50:39

Smelser:

No. No. The department sort of hired people as it wanted. It, in some sense, reflected the field. I heard no sign of any objection, no sign of any special pride. These were mostly pretty secularized Jews. People like Lipset did important work later in relation to Israel, and his wife certainly did. There was no denial of it, but at the same time, no special either pride or protest. It was not a dimension that was anywhere on the horizon at that time.

12-00:51:35

Rubens:

I have found, in talking to students of the time, that there was also an observation that much of the leadership of the student movement was Jewish.

12-00:51:43

Smelser:

That's correct.

12-00:51:44

Rubens:

And that they were New York, East Coast—

12-00:51:46

Smelser:

Out of state, yes. That's right. Student activism was heavily populated with Jews. I once speculated that one factor in the collapse of effective protest against the Middle Eastern wars and the Gulf War was that this implicated Israel in a way that created some kind of conflict for Jews. That in fact to oppose that war would seem to expose Israel to more danger in the region. A certain factor of paralysis may have set in. In 1991, when the Gulf War broke out, I got calls—really international calls—"Why isn't Berkeley blowing up?" This was the question. I couldn't basically answer the question, but there wasn't much protest at that time. A very feeble one that failed. But that led me to thinking, and this was one of the things that came to mind. It could have been reluctance or a conflict on the part of the leadership that picked this up as a cause.

12-00:53:08

Rubens:

I'm not saying that there was a palpable anti-Semitism among students, but there were anecdotal observations regarding a disproportionate number of Jews in the student activist leadership.

12-00:53:23

Smelser:

If there were a few anti-Semitic people around, they would notice it, and they would probably read it in their own ways. I was aware of it. Didn't make too much of it one way or the other, except to indicate that, forever, since the rise of the left, Jews have been active. The Communist movement in Europe has been a threat in Jewish culture and politics. It wasn't very big.

12-00:53:55

Rubens:

I never asked you about that, also if when at Harvard you had ever seen any of that.

12-00:54:04

Smelser:

By the time I was there, the quota system had been kind of broken and there was a fairly sizeable admission of Jews. The anti-Catholicism was more evident at Harvard, particularly because the commuters, who would live in the commuter hall and were not resident students, were mostly Boston Catholics. There was a clear kind of social class-ethnic-religious hostility, as if these are an inferior breed of some sort. That's what struck me at Harvard.

12-00:54:40

McIntosh:

We have about five minutes. I just want to get the chronology straight here. When is it that you go on your sabbatical?

12-00:54:50

Smelser:

Beginning of the year of 1965. So I was there January through August, and it was quiet as anything during the summer. I was just on my way out.

12-00:55:04

Rubens:

And your sabbatical was a year long or a semester long?

12-00:55:08

Smelser:

It was a year long. I didn't leave, because I was still fulltime in psychoanalysis, and I still wanted to be near my children. I didn't leave that time. In the seventies, we were out of the country for three years. The seventies was kind of the time abroad. That was one of the threads in the seventies. Eighties was administration of all sorts.

12-00:55:36

Rubens:

Your bio-bib indicates that you presented a paper on the consequences of nuclear attack before your research group in 1964.

12-00:56:00

Smelser:

This was actually commissioned by a government agency. Probably DARPA or something like that—government people in civil defense. I wrote it, and I analyzed a lot of the disaster literature. I turned it into a general study of recovery, and ultimately into a long essay on general theory of social change. That appeared in my book called *Essays and Sociological Explanation*, in 1968. It was a long chapter on social change. I think I had a somewhat monetary motive in that. I was paid to write it by the government. I was in, as I mentioned last time, some dire straits associated with my divorce.

12-00:56:48

Rubens:

Very interesting in light of your work about preparing for disaster spawned by terrorism, after September 11, 2001.

12-00:56:58

Rubens:

Why don't we close it up for the day.

[End Audio 12]

Interview #7 April 12, 2011

[Begin Audio File 13]

13-00:00:00

Rubens: Today is the twelfth of April. It's our seventh meeting, Neil. We're beginning to look at a period that is just jam-packed and maybe even transformational for you. We ended last week with your leaving the Meyerson administration when Heyns came in as chancellor. I think we want to go back and look at a few developments that came out of that, especially which led to the creation of the Board of Educational Development, and you becoming Assistant Chancellor for Educational Development.

13-00:00:48

Smelser: Yes, yes. During my time with Meyerson, one of the threads of interest was ongoing themes of protest against the quality of education that a mass institution like Berkeley provides. Lots of criticism of the lecture system. A lot of criticism of the impersonality of the system, treating students like products and so on. It was one of the rhetorical elements in the Free Speech Movement. Not the major one, which was political, but always a thread of it, and always criticism of faculty and the institution. And a lot of rhetoric on reforming and making the educational system more personalized, more relevant, and so on. All these themes began coming up. Meyerson himself took the lead in this. Meyerson had commitments to educational reform and took it more seriously than many faculty did. As a matter of fact, it was his initiative that formed that Muscatine Committee which made a report on education at Berkeley. I was in his office when that was appointed. Played an advisory role in it. I wasn't central, though I was obviously very interested in it, and I was interested in the political dynamics of it. Muscatine sort of underwent a pro-student conversion.

13-00:02:17

Rubens: Was that a good choice, Muscatine? How did that come about?

13-00:02:20

Smelser: Muscatine? Well, he was a very eminent faculty member—English professor. He was nationally known. He was head of the Guggenheim selection committee at the time. He was a figure on campus. He also showed some sympathy to the students. In fact, the committee was, if anything, skewed in the direction of being pro-student and critical of the contemporary educational offerings. It picked up a lot of the student rhetoric. If you read that, the report, "Education at Berkeley", you'll see it has a lot of talk about alienation and different aspects of that and faculty responsibilities and so on. The one kind of completely square member of that committee was George Pimental. He actually wrote a kind of minority report defending the high premium on research, on the faculty's excellence and so on. A much more conventional sort of attention to the place of teaching in the university than that of the committee as a whole. But in any event, Meyerson appointed this committee.

Its report came out. It was criticized by the student activists for not going far enough. One might expect that. Savio gave a talk some months later, saying, we're not going to get any change, and still we have a student union that's going to force the university to do these things. He saw it as a weak report, whereas the majority of the faculty probably thought it was a little bit heretical toward the left.

But anyway, that report called for the appointment of a board for innovation in educational teaching and methods, and called for a position—there was some doubt as to what the exact position was going to be. I think they recommended a higher rank than assistant chancellor, but be that as it may, they recommended a structure that in fact Heyns picked up and actually created. The Board of Educational Development was a faculty committee specially designated to foster innovations and approve experimental courses and arrangements on campus. Corresponding to that was the position of Assistant Chancellor for Educational Development, and Heyns asked me to take that position. He had, I learned, considered asking Muscatine himself to do this, but Heyns explained to me he wasn't quite sure that Muscatine would be the kind of person to take the administrative responsibility of the position, even though he had fostered a lot of the ideas and so on. I have a feeling that Heyns chose me as a more moderate candidate. This is just my own opinion, my own post-hoc evaluation of it.

But anyway, my role was to be a member of the committee and help the committee make decisions on the academic merits of experimental courses, and vote on the committee. At the same time, I was the link to the administration. The committee's activities of approving experimental courses—it was kind of like a course committee, but an extraordinary course committee, dealing with group tutorials, dealing with summer residence programs, dealing with special courses that special faculty members wanted to teach, dealing with—you might say petitions—that came from student groups or individual students for a certain type of course, and so on. Then once the board had made a decision approving a course or an arrangement or experiment, I would then take that to the budgetary office, and basically beg for money for this. I always got the money once we approved it. Errol Mauchlan was my contact man. I sort of felt like Nora in "A Doll's House," going up and asking if I can do this and could do that. He was always very cooperative. Heyns had a lot of faith in me. He thought if I really approved of this sort of thing, we should do it. He more or less gave a very big blanket of coverage to the decisions that came out of that committee, but through me. In this particular role, I had Heyns' full trust in matters. We were able to do some things. However, that particular mode of approving discrete projects and then getting them funded and executing them proved to be, in retrospect, quite a weak mechanism, and a weak campus response to the problem of innovation, because it was situational, it was reactive. Very little innovation was done. The projects were all small and discrete. They would happen and then they'd go away.

13-00:07:59

Rubens: How about that it also resided outside the academic senate? Was there any tension between—

13-00:08:03

Smelser: No. This was a body which was pretty well accepted in the faculty. I never got any flack from the fact that we were doing anything irresponsible or so on. We had to do some policing. There were political ingredients in that job. In fact, the issue of innovation became quite highly politicized, because a lot of the courses suggested, or arrangements suggested, boiled up out of student activism and involved non-academic kind of advocacy. We really had to make a judgment on the academic quality of what came our way, and many things we vetoed. A lot of projects that came up, we just said, no, this is not it. We had a very sensitive issue as to who should teach these courses. We got a lot of volunteers from the community, from KPFA, from activist groups on peace, whatever—coming along, wanting to give a course on campus by one of their activist members for credit. So we sort of had to take the line that anybody who teaches these courses has to be academically certified through the processes of the university. There were a lot of people coming in off the street, wanting to teach a course because they had something important on their mind. We had to take that into account as well, and adopted basically a policy that only senate members—or senate-approved, temporary faculty could do that, of course, as well. It wasn't just senate members, but had to have academic certification by the university in order to teach. That was the way we handled that particular pressure toward politicization of the curriculum, because the BED seemed to be an avenue by which special things could happen. So there was a kind of policing operation there.

13-00:10:00

McIntosh: Can we just take a moment to talk about who was on the committee?

13-00:10:03

Smelser: Yes. I was going to say that the chair of the committee was a mathematician named John Kelley, a senior and well-known faculty member. David Krech was on it. He was a well-established senior psychologist on the faculty. James—what's his last name? Education school faculty member was on it. And a few others that we can fill in later. I felt I was sort of a tough cop by comparison with the mentality of that committee. Kelley in particular gave me a lot of grief, if I can say that. He had the idea, if anybody thinks of something they want to do, they should do it. He was extremely permissive. He was always open to everything, and I was always having to say, look—. I became a kind of academic conscience of this committee, which really took itself seriously, wanting really experimental things. I thought that I played that kind of role of cop on the corner, holding up the hand when something came along that wasn't academically appropriate.

13-00:11:23

Rubens: How were these faculty appointed to the board?

13-00:11:26

Smelser:

Let's see. It was the senate, yes, it was a senate committee. The administration took the nominations of the senate and made this committee out of it. I guess it was kind of on the senior side and on the liberal side, as far as its composition was concerned. I don't consider myself a rigid conservative, but I did get this role of being, as I call it, the academic conscience of the committee. Now, I did take a few initiatives. I was very interested in a nascent group of faculty who were interested in setting up a program of religious studies on the campus. Higher education has had a long history, of course, of separation of church and state. But we looked around as to the teaching of religion on the campus. We found that there were eighty to a hundred faculty members who included religious content in their courses in one way or another. History department, English department, in all kinds of ways. There was an initiative on the part of some of these faculty members to get a program, a group major, in religious studies set up. It wouldn't be a department. It would be a program—or a group, actually. We had a group, and then a program, and then if it matured into a department, so be it. If it didn't, it remained a group. That was one of the academic arrangements on campus. I very actively promoted and supported this initiative, and it happened. There became a kind of a group major.

13-00:13:07

Rubens:

Why was it that you were particularly an advocate of this?

13-00:13:10

Smelser:

I was sympathetic to the content, the scholarly content, and the usefulness with respect to undergraduates who might be interested in studying religion as a discrete scholarly phenomenon. It just happened to coincide with what I thought was kind of important. I thought it would be a good thing to have on the campus. That was the reason I resonated with the idea and actually took some initiative in helping that come about.

I was also interested in a second initiative that failed. That was to work toward a relaxation of definition of a fulltime student and progress toward degree. This was partly oriented to two audiences. One, veterans who come out of the service, who have to work. They've got family responsibilities. The lockstep progress toward a degree doesn't exactly apply to them, and also to women. I was very sensitive to the women who have a family, later entries who come back. The age of our student body was going up. I was interested in refashioning a time-to-degree and workload definition, and took the initiative to approach various kinds of administrators on campus. Deans, mostly. Summer school program head, others. I ran against a total brick wall. This failed completely. There was not any room to budge.

13-00:14:52

Rubens:

The departments were—

13-00:14:53

Smelser:

Well, departments—not so much as the deans of the different schools and colleges were the operative people here who would be responsible for the policy, for any changes. Talk about a brick wall. I really just didn't get anywhere on this, even though I was convinced that it was a valuable reform. But it didn't happen.

13-00:15:17

McIntosh:

What did these deans think that they had to lose?

13-00:15:21

Smelser:

They just had a model of the student progress. It introduced an element of chaos in the bookkeeping operations of the institution.

13-00:15:32

Rubens:

Was this for both undergraduate and graduate?

13-00:15:35

Smelser:

Undergraduate, mostly. But it could have applied to graduates. The graduate time to degree is a much looser thing, anyway, than the undergraduate four-year sequence. Student course load is much more meaningful and articulated at the undergraduate level. If you'd like a personal reflection on those years, I felt somewhat constrained, that it was a university response. It had a public relations element that we are engaged in innovation. Interestingly, the public relations office was interested in the role of this group. The centennial funding drive was going on at that time. Fifteen million dollars. Can you imagine how small? Got interested in communicating with alums in particular about this ferment on campus.

Well, I mention that because that's the way I met my second wife. Sharin (Sharin Fateley when we met, Sharin Smelser after we married) was working in the centennial campaign, and she was given an assignment by Joe Mixer to come and interview me about what was going on with the Board of Educational Development. She came to a lunch of the committee. That's how the spark was ignited in our romance. She was to come interview me. She did, on a couple of occasions, then wrote up accounts of some of the activities we were undertaking. The Board of Educational Development continued after I left in 1968, under different leadership. It also had rough sledding from time to time. It came into conflict with student-initiated courses. There was a kind of political tension there.

The big thing that happened and I should really mention this, is that I left in the summer of '68. I had agreed to be two years there. I served from summer of '66 to summer of '68. I left in July '68, which was the scheduled term ending. Just about that time, or just after I left, this proposal came—and my successor had not been yet named. This proposal came to the BED from two faculty members—one of my colleagues, Troy Duster—for that Eldridge Cleaver course, Sociology 139X, or Social Analysis 139X, which was

sponsored by my colleague and another faculty member, but Eldridge Cleaver was going to give all the materials. I didn't review that. Of course, the board says, OK, let's do it. Sounds interesting. I cannot say what I would have done. I mean, that's an after-the-fact construct. But I certainly would have raised the question about the faculty's participation, because they weren't going to participate. They were sponsoring it, basically fronting for Cleaver. That, of course, became a huge issue in front of the regents when it hit the fan. The whole idea was the faculty were not teaching this course; we're just getting this propagandist coming to campus. He was the Peace and Freedom Party's presidential nominee. This thing was huge in the papers and it really blew up. The senate got involved in it. The regents revoked credit for it. The regents got involved in interference in the prerogatives of the senate. It was a huge blowup that happened. I sort of said to myself from time to time, I got out of town just in time. I don't know what role I would have played. I just don't want to speculate that I would have stopped it or insisted on changes or whatever. Anyway, that was one of these bureaucratic accidents—or explosions, if you will—that happened by virtue of how the shop is being run.

13-00:20:05

Rubens:

During those two years, were you also teaching in the sociology department?

13-00:20:11

Smelser:

Oh, yes, I continued to teach in the department. I had now inherited the graduate theory course assignment, and I would teach that every year, along with other seminars and undergraduate courses. Regular teaching. I believe I got little time off. I got some time off because of the administrative appointment.

I should also talk about, a little bit, my larger role in the Heyns' administration, because I was a member of his cabinet and had a close and mutually trusting relationship with Heyns. It was a positive relationship. He knew me from before, because he was one of the people that tried to hire me to go to Michigan years before. He knew who I was even before he came to the Berkeley campus. I had met him back there. He was part of the interviewing group for me, to try to persuade me to come to Michigan. His confidence in me was such that he recommended that I take the position as dean of the College of Letters and Science just after '68. That came open. He wanted to appoint a new dean. He asked me if I would like to be dean. I said no, I wouldn't like to be dean at that time. I was already beginning to feel a little itchy that my scholarship had begun to be compromised by all this administrative responsibility, so I wanted to get back to the main line of my career. I helped him write speeches during the crisis—the whole time was a crisis period. '66 to '68, antiwar was the thread. Then in '68, '69 came the Third World College and People's Park. Everything was chaos in those years. I was kind of on hand to help him as he dealt with the senate, as he dealt with the public, and so on. One of his people. I wasn't in his inner circle, if you want to call it that. He had a group that was a kind of high command, if you

want to call it that. That was made up of him and Bud Cheit, the executive vice chancellor, John Searle, who was a philosophy professor who kind of took my place as political conflict person, and another advisor—I forget his title—from the law school, Robert Cole. He was a classmate of mine at Harvard, but a law professor. This was, you might say, the core solitary administrative group. I was on the edge of it but not in it.

13-00:23:02

Rubens:

Was that to your liking or did you feel—

13-00:23:08

Smelser:

I was glad I wasn't in it, because this group began to drift toward hawkishness in the face of all this conflict, and toward what psychologists sometimes call "group think." A mentality—you know who your enemies are, you know who your friends are. You have a strong conformity within the group. I remember one dinner I had with that group. I happened to be invited. I wasn't inimical to the group, but I just wasn't in it. At Heyns' house, I believe it was. The after-dinner talk all was poking fun at various activists and faculty members who didn't go along with them. I was very troubled by this, and left in a kind of state of, why has this happened? I can understand it perfectly, because they were under such bombardment. But somehow or other, I felt that I was a dove in the midst of this increasingly hawkish mentality. Now, this doesn't say they always behaved hawkishly, because they had enough sense in a conflict situation. They were intelligent people who rolled with the punches, but this was in their kind of private view of the world became somewhat insulated, I have to say.

This was Heyns' inner group. It was kind of like the equivalent of Martin and me. He and I formed a two-person group. We never developed this kind of semi-paranoid view of the world, even though we had strong reactions to what was going on. Somehow or other, that didn't happen. I was his single sounding board, really. Well, Bob O'Neill was, sort of, as well. He was legal advisor. But that was on a more selective basis, and he hired Dale Jorgenson, an economist, kind of to be the liaison with the Muscatine Committee in educational development. Those were not as central. But I was not in that intimate role in the Heyns administration.

Now, one of the things that happened—I can say that now—is that I developed a relationship with a reporter from the *Los Angeles Times*, Bill Trombley. He was the best reporter in the state. He covered higher education. He was a masterful journalist, and I came to like him and respect him. He formed a relationship with me, whereby he would call me or would come and see me for background. I was never quoted by him, but I gave him, under condition that he not reveal the source, a lot of general background that he would not have gotten from talks with the higher administration itself.

13-00:26:20

Rubens:

After you were outside the vice chancellorship?

13-00:26:23

Smelser:

No. Well, it extended a little afterwards, but it was during. It was actually during. Not that I felt I was being a traitor. I sort of had the idea that I was helping out the university by giving a more comprehensive background. He was absolutely and completely discreet in his own professional honoring of the communication that he and I had with one another. I didn't consider that I was a mischievous, Daniel Ellsberg-type leaker of damaging information to the university, but it helped Trombley a lot in understanding and interpreting all the events that were going on at the time.

13-00:27:08

Rubens:

So you were giving him a sympathetic look at the university.

13-00:27:13

Smelser:

Well, I wasn't criticizing the university, for sure. I was just giving him a layout of the kind of forces that were bearing on the chancellor's office, on the thinking that the general staff of the university was engaged in. I did say some things about conflicts within it when they came up, but he never betrayed any of this. I guess I could be criticized for having done that to a reporter, but because of the special honoring of the general background information I gave him, I didn't—if he had dishonored anything I had said, it would have been the end. It would have been the end of it.

13-00:27:57

Rubens:

It's interesting. So it was just a matter of his—

13-00:28:00

Smelser:

It was mutual trust that developed. He was sympathetic to the university. He wasn't a muckraker. He didn't want to expose scandals and conflicts and dirty linen at all. It was kind of an unusual relationship that we had.

13-00:28:18

Rubens:

Whereas the *San Francisco Chronicle*, I think, always was the lead in trying to expose—

13-00:28:23

Smelser:

The University was on the menu of the *Chronicle*. I had interviews with the *Chronicle* and *Oakland Tribune* and other reporters from other—and *Berkeley Daily Gazette* and others. They were much more—one might say official—in their content. I didn't have this respect for the local press that I developed for Trombley and the way he dealt with the university. I think, in a way, I might have helped by giving the larger context to him.

13-00:29:05

McIntosh:

I imagine that the people who would be most interested in Trombley's work would be UC Berkeley faculty and staff, right? So in a way, it sounds like you were kind of controlling the narrative through—

13-00:29:17

Smelser:

I was a big part of the narrative, because he came to trust me. Trombley liked the kind of conversations we had. Now, I never revealed this to my colleagues in the chancellor's office. It would have been very sensitive that I was talking to a reporter in an unauthorized way, which I guess it was. This is a part of the history I'm very happy to talk about, though I realize that the reactions to that role that I had would be very ambivalent and sometimes disapproving.

13-00:29:52

Rubens:

Well, I don't know if this is the time to ask it. We were going to ask if your administrative roles also engendered some kind of antipathy by members of your department. Did it harden the lines?

13-00:30:01

Smelser:

No. The issue of educational development and educational reform, interestingly, was a topic that not many faculty were interested in. It just wasn't high on their radar screen. Most of my colleagues thought I was doing something okay, interesting. It obviously was in keeping with the temper of the times to be interested in some kind of educational reform. Any friction and heat that I experienced in the department was all on the antiwar activities—that I wasn't dovish enough. I didn't join the shrill voices. Inwardly, I thought the war was a big mistake, but I didn't get into the big-time protest and commenting on it. Within the department, I was probably looked upon as center-right, even though I never uttered a word of approval about the war. That was the tone of the campus at the time. So the frictions were that, and also non-academic issues, like should there be graduate students voting on this committee or that committee, and departmental standards of evaluation. All these things were up in the air at that time. I tended to be a sterner conscience in this regard than many of my colleagues. That was another basis of where I would have friction. BED did not figure in my relations with them. It was something I was doing, and it was not controversial.

13-00:31:55

McIntosh:

I know we've diverged a little bit from the content of what the BED was dealing with, but I just wanted to ask, for the record, do any of the projects that you reviewed for the BED stand out, other than the ones that we've already focused on? Do you recall any—

13-00:32:14

Smelser:

Well, I think one thing that got maybe the most press was a thing called group tutorial. That's a kind of pre-runner to the freshman and sophomore seminars. A couple of faculty members in the English department were especially interested in sponsoring it, and I helped them carry out the group tutorial project, to hire graduate students to collaborate with them in group tutorials. That's, to give you an example, one of the most interesting things we did. A lot of the other things were discrete courses on discrete topics. Frankly, I forget the content of most of them. That was the biggest line of activity that

the BED did, was to approve specific courses that were one-shot, usually—didn't repeat—and came and went.

13-00:33:08

Rubens: Did anti-war politics figure in course proposals?

13-00:33:15

Smelser: Oh, well, a lot of the proposals that came up were inspired by antiwar animus. Most of them we disapproved because they were in the nature of, frankly, propaganda, and part of the movement. I did not see ourselves as part of any movement, and so these did not meet with my approval. And that would of course cast you in a conservative role as well, if you acted as policeman for things that people cared about very strongly.

13-00:33:51

McIntosh: Can I ask just one thematic question? I noticed, just in our discussion this morning, that you talked about yourself as being not on the periphery, but basically being isolated within the Board of Educational Development as the academic conscience. Then you also talked about yourself as being on the periphery of this inner circle with Heyns. Were there any groups that you really felt at home in?

13-00:34:19

Smelser: In the administration?

13-00:34:19

McIntosh: During this time, yeah.

13-00:34:21

Smelser: Not in that strong sense of the term. I was certainly a loyal and cooperative member. When there were heavy crises and my services were called upon—for example, in crisis meetings, in figuring out what to say to the press, in figuring out what Heyns would say to the senate—I was a full member of it. No, I didn't feel a sense of alienation. I didn't feel a resentment that these weren't like the good old days with Martin. I didn't have that particular personal line of reaction, no. I felt pretty much full member of the campus community, and even though my role in the chancellor's office, in all its regards, was not absolutely central, I didn't lose much sleep over that fact.

13-00:35:12

Rubens: How often did the academic cabinet meet?

13-00:35:16

Smelser: It normally would meet weekly. Sometimes it would meet more often, especially if there was something really hot. So it was an active group. His best advisory group.

13-00:35:31

Rubens: Do you have a reflection on his administrative style? You've ended up having a lot of experience with chancellors.

13-00:35:44

Smelser:

Given the circumstances of the time—his whole regime, '65 to '71, there wasn't a moment in which there wasn't deep conflict and in which he was not endangered. I have tremendous admiration for the man. It cost him his health, really, in the end. He retired because of some kind of a heart episode, though he was sort of forced out of office by the regents as well, as an aftermath of the reconstitution movement. The regents demanded that he give an accounting of what professors had turned their classes into active political groups or had not met their teaching obligations. He gave them a very benign report, which said basically the activities of the campus went on as normal. But they hadn't gone on as normal, and the regents knew that. They had their own sources of intelligence. So there was an idea that Heyns had engaged in some kind of cover-up. That was the basic endpoint or failing point of his administration that lost the confidence of the regents. He resigned not too long after that. He wasn't fired.

13-00:37:04

Rubens:

We'll get to that reconstitution movement a bit later.

13-00:37:08

Smelser:

Yes, I was in the center of it, because I was on the policy committee at the time.

13-00:37:11

Rubens:

Yes. But do you think now we should move to your scholarship?

13-00:37:16

Smelser:

Yes. I went back to my scholarship. I had been doing writing of individual essays over time. I put together this book called *Essays in Sociological Explanation*, which was an effort to bring my own sociological imagination to a range of different problems, including social movements and social change, and with a methodological essay or two. Put it together. People commented on the title of that book, because Parsons had written a book, a very famous book of essays, which he called *Essays in Sociological Theory*, and I titled mine *Essay in Sociological Explanation*. People began reading a lot into that, as though I was just more down to earth than Parsons was, and so on and so forth. That had a kind of interesting reaction, which I didn't particularly appreciate or not appreciate, but that's the way it came out. I was commissioned by John Wiley and Sons to write a text—or to edit a text, which I did. A high level text, in which I got really good scholars to contribute to chapters, and I put the whole thing together. It came out in '67 and then reissued in '73. I did the book with my brother—edited the book with my brother—called *Personality and Social Systems*.

13-00:38:58

McIntosh:

Now, that was originally in '63, right? Then the second edition comes out in 1970, is that correct?

13-00:39:04

Smelser:

1971, I believe. I can't remember the exact dates, but I, at that time, was working on that. I was converting my theory course into a book at that time. That came out in 1971. There were just a diversity of different lines of activity that were going on in scholarship. I decided also, under request—a lot of these were requests from outside—I decided to do this book on Karl Marx. It came out in '73, but I did the work on it around 1970. The University of Chicago Press had a series, masters of sociological thought, something like that. They would get people who were now currently active to bring together the most decisive writings of classics. Veblen was in it. William Graham Sumner. The big grandparents of the field were in it. They asked me to do the one on Marx, which was a very interesting request for me. It made sense in that I had read and given Marx really serious extended critical treatment in my book on the Industrial Revolution. I had read all the blue books that Marx read. I made jokes about sitting in Marx's chair. I made jokes about looking for angry German comments on the sides of these—because they're the very same blue books in the British museum that Marx had actually sat down and laid his eyes on as well. So I was already regarded as somewhat of an expert, and I'd used Marx in my course. I never gave that course without Marx's *Capital* being in it. So there was a reason for my being asked, though I was certainly not identified in any sense of the term as Marxist. As a matter of fact, if anything, I was looked upon as being in another camp from Marx. I wrote a long and very considerate essay on Marx as a sociologist. I still get royalties on that book. It's still in print. It sells a lot abroad. And translated into different languages as well. Another line of thinking. Again, responsive to the outside.

13-00:41:31

Rubens:

Your second serious look at Marx—second or third—did it change your thinking at all or affect—

13-00:41:39

Smelser:

There was one line of analysis in my introductory essay that caused a great deal of commentary, both positive and negative. I took Marx's view of society, and the components and the relations to one another, and I pointed out a number of circumstantial continuities with—it was known as functional analysis. That Marx in fact had a kind of functionalist view of the institutions of society and how they fit together. It's just that he introduced the thread of domination rather than integration. I tried to make theoretical sense of a variety of different approaches to social organization, and pulled Marx in, saying that there was a functionalist thread. Well, Marxists hated that. I really got it heavy from the left for that particular line of interpretation, which I still think had some merit to it, but nonetheless, you can see why it would be controversial in the context of the times.

13-00:42:46

McIntosh:

Understanding the context of the times, were there any other ways in which you felt constrained in your analysis of Marx or in publishing on Marx?

13-00:42:56

Smelser:

No, I didn't. I felt a little bit proud of myself that I didn't have Parsons' animus to Marx. He was very open in his rejection of the Marxian diagnosis of capitalism. I didn't have that animus. I think that, if I may be permitted a biographical observation here, one of the things I didn't like about my father's outlook on the world was the hatreds he had. I think I mentioned that in my reflections on my childhood. There were a lot of antagonisms in his outlook, and I thought they were ill-considered in many cases, even though I never argued that much with him. I just felt proud of myself for being a more tolerant person. I think maybe there was a little bit of recapitulation of that relationship with Parsons, who really was not tolerant of Marxian or Veblenian sociology or economics. I felt, in a way, I suppose, personally that I had risen above that and taken a much more catholic view of these very controversial figures.

13-00:44:14

Rubens:

Clearly the press chose you because you were not a Marxist, right?

13-00:44:18

Smelser:

I don't know what their reasoning was. That might have been one thought. I just think it would have been a mistake simply to get a confessed and believing Marxist to endorse what was already there, so maybe that figured in their decision to ask me. And as I say, I had already done some work on Marx.

13-00:44:39

McIntosh:

It sounds like a lot of these projects are commissioned from outside or have developed out of previous coursework that you've taught. As you're doing this work, are you feeling as though it's basically just kind of busywork that you're hoping to get through and get out?

13-00:45:02

Smelser:

No, I thought this work was worthwhile, even though it was not in the same sense original contributions, like my first three books. They were not this massive enterprise that were innovative in the same sense of the term. The one project that did fall into that category was the continuing evolution of my interest in comparative methods. As I mentioned, that had crystallized in conversations with Lipset. We were actually going to have a coauthored volume on comparative methods in the social sciences. I was the one who took the lead. Lipset was into a lot of other things. He continued to be committed to it, but I would write essays and memos. He basically wouldn't respond very quickly. He had other things going on. Then it all became a non-project when he went to Harvard. It just went up into thin air. But I decided I was going to go on with it. I delivered a couple of papers at conventions and published a couple of preliminary essays. I did an essay on Alexis de Tocqueville as a comparative analyst, and that was published, I believe, in '71. That was beginning to get crystallized. That became the basis of my major application for a Guggenheim in 1972, and a major focus of my research on the sabbatical leave in '73. Aside from all these other elements,

which were, in a way, pulling together things that had been going on in my life, and most of which were responses to outside requests, this one line of research was the novel one, which was to develop into that book.

13-00:47:08

McIntosh:

What did you see as the payoff for really investigating comparative methods at this point? What was your vision for this project?

13-00:47:17

Smelser:

It goes back to the influences on me that were part of my undergraduate experience, actually. Anthropology was very heavily into cultural relativism at the time. Every bit of exposure I had to cultural anthropology, relativism was an element. Relativism, in some sense, is anti-comparative. You respect other people's morals and values and cultural systems in their own context, by their own right, and you don't import any of your own biases in judging or interpreting culturally different points of view. So in that sense, everything is kind of unique. You don't draw comparisons. You're skeptical about cultural universals and so on. This made a big impact on me. I wasn't satisfied with cultural relativism, even though I was much influenced by it. On the other hand, you had a tradition of comparative studies in the social sciences, which was completely different. Like comparative voting statistics, which assumed that votes are the same in all countries and you can compare rates. Even economic statistics are considered as comparable between societies. So you had these two poles of comparative analysis: one scientific, dealing with quantified or empirically identified indices that we're applying across the board, and secondly, a relativistic point of view, which basically said you can't do that sort of thing, because the context and meaning is such that they are not comparable with one another. That was an intellectual dilemma that I was facing.

The thing that turned out to be controversial about that book was that I say, look, comparative methods in which you are really basically comparing a discrete number of societies with one another. And you can't use high level statistical analysis because of the nature of the sample you're dealing with. And you can't use laboratory methods, either, in dealing with these comparisons of large-scale social structures and processes. At the same time, there's a great deal of comparative work going on. How do you deal with it? How do you take into account that you've got a limited number of cases and many, many variables at work? What I did in the book was to try to establish a theoretical continuity on the laboratory studies, statistical analysis, comparative analysis, case studies, so on. It was a comprehensive, integrative book that treated research methods as one-of-a-kind, and you approximated the same efforts to come to explanation in all of them. That was the big message of the book. I said, how do you deal with different contexts? How do you hold things constant in comparative analysis? How do you approximate statistical reasoning, or how do you approximate laboratory methods of holding things constant and determining which causes are really at work? I see

the intellectual accomplishment of that work as this kind of integration of methods. It was an intellectually satisfying enterprise. When I came back from Europe, the first meeting of my comparative group—it was called Theory and Method of Comparative Studies—I presented the manuscript of that book to that whole group. There were about twelve of them in there.

13-00:51:17

Rubens:

They had not read chapters?

13-00:51:18

Smelser:

No, no. I did it all abroad. It was a finished product by the time I came back. I finished it off in Europe and I was getting ready to send it off to the publisher. [*Comparative Methods in The Social Sciences*, 1976] I put it into one of the evening seminars of this group. I was very gratified that Bendix who's my dear friend and also a comparative scholar, thought I was too, quote, "scientific." Giving too much away to the quantitative sides. Charles Glock, the methodologist in my department, thought it was softheaded. I guess I said to myself, well, that means something, that both these guys found it wanting. I must have done something right. It was, in a way, a middle-of-the-road, synthetic piece of work that I probably should have guessed would have not fit in the dominant views of comparative studies.

13-00:52:28

McIntosh:

I know that there's a lot of interest during this time, coming from foundations and universities as well, in exploring the idea of a social science that can be grafted onto other cultures, and creating kind of a way of using American scientific methods to understand the world.

13-00:52:49

Smelser:

Well, yes. If anything, that was the dominant mode. After World War Two, there was a sweep of positivism over European sociology. Rene Konig in Germany, he was indistinguishable from the positivism that was so dominant on the American side. That was a general movement that only got challenged perhaps fifteen to twenty years later, after a long period of dominance in Europe. So the dominant tone of the time was, quote, "scientific," even though all these threads of protest were going on. Late sixties was the beginning of the anti-positivistic revolution, simultaneously methodological and political in its import.

13-00:53:44

Rubens:

And positivism being used in what sense? How do you—

13-00:53:48

Smelser:

Positivism—the world is made up of positive facts, and you can identify these facts. They're in the same category of physical facts. The point of view of the observer doesn't have anything to do with defining them as facts. They're positive facts. That's what positivism really refers to. You can analyze them as though they were part of nature. Well, the objection to this, of course, that's not the case. These are human sciences. Human people endow these facts with

meaning. They're in fact creations. Later, deconstructionism was a more radical criticism of the positivistic point of view. Even at the time, phenomenology and some aspects of Marxism, which dealt with facts as apologies, were seriously challenging the straightforward, positivistic view of science and of social scientists as being neutral analysts of facts. So that turmoil was beginning, even though the dominant tone was still positivism. I gave a lecture in Germany in the late seventies called "The Persistence of Positivism in American Sociology." Despite the fact of all this turmoil, most work-a-day sociologists and social scientists still considered themselves to be positivists in that sense of the term.

The one side of my scholarship we didn't talk about at all was on higher education. That was already thriving. We may finish that. I'd like, if you don't mind, to talk about this role of academic statesman that I began to get involved in.

13-00:55:56

Rubens: Terrific. I think those two go together.

[Begin Audio File 14]

14-00:00:00

McIntosh: I just wanted to continue talking about your work in the sixties for a few more questions. The first is, you talked about the organic interest in comparative methods and where that came from. But I guess I wanted to also ask if you, at the time, were aware of the political import of your endeavor, and also if you were aware of the interest that foundations, like the Ford Foundation, would have in such a project.

14-00:00:35

Smelser: Are you talking about higher education?

14-00:00:36

McIntosh: I'm talking about the comparative methods.

14-00:00:38

Smelser: Oh. Oh, I was aware that the area was controversial. You couldn't get away from it. If you read it, you'd know it was right in front of you. I was very aware that I was taking a synthetic view of methods, trying to reconcile some lines of analysis that were considered to be irreconcilable. It's an intellectual style—it actually pervades my academic work—of synthesis and efforts at unity, which I think also, perhaps, is not foreign to my personal style in social and organizational settings as well. I've never found myself on an extreme end of anything.

14-00:01:30

McIntosh: This is jumping ahead chronologically a little bit, but since you mentioned the critiques of positivism, and the persistence of positivism as well, one of the lasting titles from this time seems to be Alvin Gouldner's *The Coming Crisis*

of Western Sociology, in which Parsons and you are both singled out for treatment. I wanted to get your perspective now, in retrospect, of whether you thought that was actually such an impactful critique.

14-00:02:13

Smelser:

I have to relate a personal story about Gouldner. He came out here to teach in summer school about three or four years after I arrived. He and his wife invited me to where he was living, up in the hills on the other side. Midway through the dinner, Gouldner became extremely belligerent toward me. Really hostile. It was me and Parsons. He was just lambasting and critical and personal. I was really so taken aback. It's the only social occasion I've actually walked out of. He was so mean and brutal in his attack. Totally unanticipated. Well, I knew he was already a steadfast and very hostile critic of Parsons, but why should this spill over onto me with such vitriol? It was very offensive. I basically decided I was going to never have anything to do with Gouldner again personally, which I didn't. He was a very aggressive man. Ended up in physical violence on his own campus at one point. A physical assault on a colleague later on.

In that book, he obviously is mostly interested in Parsons as a target. He refers to me as a kind of middleman, meaning I'm not a full Parsonian, but I haven't given enough due to the left, either. That was his reaction. His main reaction to my work was that I was not a full Parsonian and I was trying to—in that sense, he was maybe a little bit correct in his vision—that I was trying to occupy some kind of middle or synthetic position between the dominant paradigmatic conflicts of the time, between Parson's great emphasis on integration on one hand, and Gouldner's own interests, as well as C. Wright Mills and some of the British sociologists' emphasis on conflict. I was, somehow or the other, putting myself in the middle. He gave reference mostly to my book *Essays in Sociological Explanation*. I knew about that, and I read it in his book. In a way, it was a little bit insulting to be categorized that way, though it was not entirely wrong, either, in terms of what role I played. It's sort of in the style that he did it that I found most offense, and then I'd had this very negative personal episode with him that turned me off forever.

14-00:05:13

McIntosh:

The tone of the book is certainly very aggressive.

14-00:05:15

Smelser:

Shrill—a very shrill book. And it hasn't lasted. It's a product of the time. Gouldner just took an extremist view of the situation, and it didn't last.

14-00:05:32

McIntosh:

So at the time, it wasn't anything that really raised a lot of concern or interest on your part.

14-00:05:40

Smelser:

No. Everything was going on. One of a number of attacks from that side. No, I didn't lose any sleep over it.

14-00:05:54

Rubens:

This is an aside, but we never asked if you had a hand in replacing yourself as editor of the ASR. I know that—

14-00:06:02

Smelser:

There was no official role that I had. It was done by the publications committee of the ASA, and they chose a man named James Short, who was a sociologist from Washington State University. Outstanding criminologist. I thought he was a very good choice. I did not name him. He immediately came down to talk to me after he was appointed, and we had a long series of sessions together on editing the journal. No, that was a procedure. The editor was completely divorced from choosing his successor. They did come and ask me for suggestions. I totally surprised myself. My initial reaction to that was, shamefully, I couldn't think of anybody. [laughter] As I say, I was very ashamed of myself for that opinion, but it was a kind of self-aggrandizing sort of moment. That I know nobody can replace me was the message I gave myself afterwards.

14-00:07:16

Rubens:

The polemics, in and around Gouldner, for example, where did they play out? Would you see it in the ASR after you left?

14-00:07:30

Smelser:

Oh, well, sure. The ASR wasn't the place where the biggest activity was in this conflict, because it tended to maintain its sort of positivistic, scientific image. It subsequently has been criticized for being too, quote, "establishment" in its orientation. I think, in a way, I broke from that, because I included a lot of theoretical articles and a lot of softer stuff in there, and got some flack for it, actually. But this particular dialogue came out more in larger books or at annual meetings and that sort of thing. The ASR was a bit of a rock of stability in the middle of all of that.

14-00:08:19

McIntosh:

It strikes me that the Millsian and the Gouldner side of things, their critiques, because of the vitriol and because of the aggression, there's a spectacle aspect to it. They're exciting. It gives people something to talk about. But the positivism does persist, and if you're lecturing on it, when was that lecture you gave in Germany again?

14-00:08:45

Smelser:

Oh, mid-seventies, I think, if I can remember it.

14-00:08:49

McIntosh:

For those of you all in the discipline who aren't ideologically aligned with people like Mills or Gouldner, it sounds like stuff that you can kind of shrug off a little bit.

14-00:09:06

Smelser:

If you'll go back to the reactions that I described to my book on collective behavior, I was swept into the middle of that, mostly from the left, that I was an apologist for the system, as if I was writing handbooks for policemen and politicians to control social movements, and was treating social movements as irrational. So I got drawn into that, I thought illegitimately. I certainly was not uninvolved, but at the same time, I didn't sort of take any kind of strategic view as to how to shape my work or was I going to respond to these people. I was invited to get into a lot of exchanges in the literature, and with the exception of one article, I just declined to do it. It was not my style to get into these slugfests that deteriorated frequently and became ad hominem. I stayed away from that and felt myself—I wouldn't say above it, but not centrally involved. I think I had a certain feeling of continuity of myself as a working sociologist. A lot of this fighting, I personally felt, was a waste of time.

14-00:10:21

McIntosh:

Maybe that intellectual statesmanship is a good segue into your academic statesmanship as well.

14-00:10:27

Smelser:

Okay. A number of things began to happen in the late sixties that indicated that people were looking upon me as a kind of leader or statesman in the field. I was invited, in 1967, to a special conference of the American Academy of Political and Social Science in Philadelphia. I remember it very vividly, because it's where Sharin and I went on our honeymoon. We went to that conference and then spent a week or ten days in New York for our honeymoon. It was in December of 1967. It was called The Optimum Scope of Sociology. It was an overview of where the field was, what are the main issues, and so on. I was one of three people, three young people—or thought to be young people in the field. James Coleman, Peter Blau and I were the three main speakers. Most of the other attendees were the senior statesman of the field. The symbolism was, here are the three people we've chosen to make their statement about where the field is, where it's going, what its scope is, and so on. That was a sign, you might say, that a certain role was being assigned to me, to deliver the lead paper in this general conference, which came out in the publication of the Academy's journal six months later.

Then in 1967, I was also chosen to be a member of a special body created by the Social Science Research Council and the National Academy of Sciences. It was called Behavioral and Social Sciences Survey. It was meant to be a general assessment of the social sciences in general, and communicated to policymakers of different sorts with respect to what should be supported. It was a programmatic document directed toward funders as well as the intellectual status of the field. So I was chosen as a member of the sociology panel. They had panels according to all the disciplines and then some special ones, like social sciences and law. I was appointed as a member of the sociology panel, which was going to write a report on the current status and needs of the field of sociology. The chair of that committee was William

Sewell, a very senior sociologist from the University of Wisconsin. The co-chair was Otis Dudley Duncan, another senior empirical sociologist. As a matter of fact, they were close to each other.

At the first meeting of that panel, Sewell announced his resignation. The reason he announced his resignation was he was taking over the chancellorship of the University of Wisconsin, at which he lasted only one year. He really didn't find it to his liking. But he resigned as head of this committee because he knew he was going to be fulltime chancellor. Sewell's resignation was followed by Duncan's resignation, who said he didn't want to be in a leadership role alone. That committee lost its leadership at the very first phases. Its parent body, which was made up of the interdisciplinary representatives of the committee, the master committee, asked me to be chair, at a very young age—I was thirty-seven years old at the time—and a man named James Davis from Dartmouth to be a co-chair. Here was this, in a way, a kind of vote of confidence. You're speaking for your field. This is thought to be a definitive document to be communicated to foundations and government as to where the field is, what are the most exciting directions, where it should go, what the needs are for funding it and research, and so on.

I accepted that, and Davis did, too. It turns out that Davis did not have any real commitment to being a co-leader. As a matter of fact, he was, may I say, a bit lazy. He just didn't throw himself into it. So it turned out that I was fundamentally pretty much the leader of this committee, made up of eminent scholars from around the country, and took complete charge of the drafting of the report and representing it to our parent body. I was the one who came to the reviews that the National Academy gave of it. My first contact with the National Academy of Sciences—I went back to Washington and they had a panel of academicians who went over it and gave comments and reactions and so on. Same with the Social Science Research Council. Here was a, I thought, premature assignment as, you might say, a statesman in the field. A representative and so on. As it turned out, the report didn't have enormous impact because it was just at that moment when the expansive mode in terms of research donors turned into tough times. The seventies were a kind of downturn in this, so all our call for different kinds of research centers and other things that came out of our analysis went unheeded. In that sense, it wasn't a document that had an enormous impact, largely because it couldn't have an enormous impact.

A minor sign of the same thing was that the *Journal of Social Forces* asked me to review the entire *Encyclopedia of the Social Sciences*. Seventeen volumes. To write a big review essay for them. Once again, there's some kind of symbolic message communicated by this choice to do this sort of thing. I also already, at an early age, was beginning to be asked for some kind of autobiographical reflections on my work on collective behavior. Once again, all these things kind of came together. I didn't see it at the time. Each one, I experienced as a discrete event or assignment or involvement. As I look back

on it, they all had this kind of—well, here's a person who's speaking for the field, synthesizing it. Maybe it was that my style of being in the middle road played some role in that. They were not going to choose a C. Wright Mills to do this. They're not going to choose an extreme positivist like Coleman to do this. I imagine that played a role. There was also an idea around that there were three people in the field who were in the process of making the field *now*. This was Coleman, and Harrison White at Harvard, and me. Same generation. We were about the same age. I didn't exactly formulate it that way at the time, but rereading those involvements and those invitations, it struck me that that was a rather distinct change in my role, and of course that dimension of it continues right up to the present day.

14-00:18:36

McIntosh:

I must ask: if you didn't see reviewing seventeen volumes of the *Encyclopedia of Social Sciences* as a step towards being a statesman at the time, then what was the motivation behind taking on projects like that? Was it just, well, this is what a sociologist does? Because it sounds almost thankless in a way.

14-00:19:06

Smelser:

Let me try to reconstruct. I was an author for the encyclopedia. I wrote the entry on economic sociology. I was really close to the people who were really running the show for the encyclopedia. That was Merton and Sills. Sills was the editor. I knew the enterprise and I had talked with them a lot at meetings. The same time I was in New York, I'd see these people. I knew what was going on, so I was involved in it. I suppose when you get an invitation to something like this, you say, oh my god, who wants to take on seventeen volumes? What sense can you make of this whole thing? How are you going to say anything about any of it, because it's so complex and comprehensive? At the same time, I had the feeling at the time, though the degree of articulation is always highly variable in these matters that it was, in a way, an honor, recognition, to be asked to do this kind of enormous, gigantic enterprise. I tackled it and I did it. I did it in a way of trying to identify core themes, some biases that seemed to be reflected in the selection of articles and the tone. Later, I and Paul Baltes edited the entire twenty-six volumes of the next edition of the *International Encyclopedia of the Social Sciences*. In a way, it was kind of an interesting prelude to that later assignment that I'm sure we'll come to.

14-00:20:55

McIntosh:

Did this increased prestige that you're beginning to acquire during this time have anything to do with these appointments that you received in the late sixties? For instance, the National Advisory Commission on Civil Disorders is one, and the President's Advisory Committee on Development and Education is another. I just saw those on your C.V. and those stood out as—

14-00:21:20

Smelser:

The involvement with the commission on violence was a congressional committee that followed the violence in Watts and various eastern cities, and

the crisis of the Johnson administration. So they had a national commission of violence. Because of my work on collective behavior, I was a witness. I was just called to Washington. It wasn't a very big deal. I was one of dozens. I wasn't a planner, I wasn't a writer, I wasn't on the staff or anything. It was an involvement, and it showed that I was looked upon as a person who could comment on urban violence intelligibly and so on. It was, in fact, simultaneously a contribution and a recommendation. The other assignment you mentioned, it was—

14-00:22:20

McIntosh:

There's another one here. There's the Science Advisory Committee. It's a task group on research and development.

14-00:22:27

Smelser:

Oh, yes, yes. There was a commission established by the national scientific advisor to the president—Office of Science and Technology. They set up a national committee to recommend a long history of investment in educational development in institutes around the country; our Center for Studies in Higher Education was one of these institutes. There were more applied institutes, and other institutions who were interested in writing texts or teaching techniques and so on. The government threw quite a lot of money into this development of pedagogy and education. We were called upon as a special committee to go around and evaluate and make recommendations to the Congress via the National Office of Science and Technology as to the intellectual worth of these programs and whether or not they should be continued. I traveled all around the country. It was a big assignment. A man named Frank Westheimer from Harvard was the chair of this committee, and I was one of the members. I think I was the only social scientist on it. We actually made some very strong recommendations on the low quality of a lot of the research that was going on, which led to a discontinuation of a number of these. It had an impact for sure. I'm not really quite certain how I ever got involved in that. It's a total mystery to me why anyone would pick me out. It wasn't one of the areas in which I was being particularly active at the time. But somehow or other, somebody mentioned my name. That's the way these things go.

14-00:24:04

McIntosh:

And an interesting example of when those committees actually have an impact, right? It sounds like your recommendations were actually heeded.

14-00:24:12

Smelser:

Yes. That was rare. Usually these things get shelved, or they fail, or become—

14-00:24:21

Rubens:

This is in 1968, right?

14-00:24:23

Smelser:

'68. Yes, that's right. Traveling quite a lot during this period. I went to, I believe, Seattle. I went to southern California. I went to several eastern

centers. We just inspected the scene and we came up with our own recommendations.

14-00:24:42

Rubens:

Were you asked at all to comment on the events at Columbia College, and then the whole summer of '68, where there was the uprising in Paris?

14-00:24:56

Smelser:

Actually, I began to have a role around this time that continued a long time. It began with, I think, a decision on the part of Hafner and maybe Colvig here, that I was an appropriate person to speak with the press. I think, in their minds—I'm only speculating—that I was the kind of person who wouldn't say things that would get the university in trouble. The public information office had this view, and I was forever commenting on the local scene from the standpoint of the politics. Then I began to get a wider role of being consulted on things that I wasn't especially an expert in. I remember two interviews, I was asked to comment on why so many kids are cruising out in the streets of Walnut Creek. What's going on here? And then why have sales of jigsaw puzzles been going up in the past year? You know, these kind of fads and so on. When Humphrey the Whale was trapped in San Francisco Bay. I was interviewed on Humphrey and why there was a public reaction to it. I made some comment that got a lot of attention. I said, "Well, you know, people have really worried about latchkey children and lost children. The symbolism of this whale that had lost his way may somehow or the other resonated with these concerns." [laughter] So I began to be a commentator on both serious and trivial things. There gets to be a network of reporters that—they like to have people who will talk. For a large part of my career, I was kind of a conduit, sometimes for the university, but sometimes independently, on talking about a great wide range of issues.

14-00:27:04

Rubens:

But I was wondering if the student rebellion particularly, at Columbia and in Paris—

14-00:27:10

Smelser:

At the time, I was asked the usual, why did they blow, and what about the European academic system? I got a few interviews at the time. I was in London a decade after the '68 LSE—LSE was one of the big institutions that was involved in this, along with Paris and various German and Dutch institutions, as well as Columbia and Harvard. They had a commemorative session, in which I think I gave a very long interview on what this meant at the time, has there been any follow-up, so on and so forth. So yes, I'd have to say that I was identified as being someone that would be called upon to comment.

14-00:27:51

Rubens:

But not something that drew you as a follow-up to your research on collective behavior or your role as an administrator putting out fires?

14-00:27:58

Smelser:

No. I wrote an essay in connection with my joining with the Carnegie Commission on Higher Education. Kerr formed it after he was fired as president. I became a member of the Technical Advisory Committee of that about the same time, '68, '69. I can't remember exactly.

14-00:28:16

Rubens:

How does that happen? You're invited by Kerr?

14-00:28:19

Smelser:

Yes, Kerr invited me, largely by virtue of his becoming acquainted with my role in the campus administration. The Carnegie Commission on Higher Education issued all kinds of publications and pronouncements and so on. It was very, very active. He was right in the center of it all. He had, here in Berkeley, an outfit he called the Technical Advisory Committee, which he had notable people at Berkeley who do work on higher education. Martin Trow was on it. Sheldon Rothblatt was on it. Frederick Balderson was on it. Roy Radner was on it. I called them Clark's Boys, because he knew these people from his own involvements. And he invited me at that time to join that Technical Advisory Committee. He got me to do two articles. He and his colleagues who were doing the editing got me to do two articles in connection with that. One was a view of Berkeley ten years after the FSM. I wrote "Berkeley in Crisis and Change," an essay trying to assess what really changed, what hadn't changed, what were the causes of this, why did it go in these directions and that direction. Reflective essay that appeared in the book, edited by Verne Stadtman and David Riesman. Then there was a second essay I wrote, which was a prelude to my interest in general education, in a book edited by Carl Keyesen, on teaching the social sciences in college. It was an essay on what are the best ways to deal with the presentation on your fields, and strategies, and themes, and types of courses. It was really a pedagogical essay that sort of was a prelude to my work on the lower division and my work on general education later. Again, these were as requests. Kerr talked me into them. They were responses, like a lot of my writing at the time.

14-00:30:35

Rubens:

Would you say you became close with Clark Kerr at that time?

14-00:30:39

Smelser:

Close is hard to say. Kerr was a very cool cat. I certainly got to know him very well, and I certainly learned of his persuasive powers. I used to make a joke that I'd go have a meeting with Clark Kerr, and I'd spend the next two weeks figuring out what I had agreed to do. He was so smooth and so persuasive a man. Yes, I'd say I began to develop a personal relationship that continued. Went to his house sometimes and he would involve me in social activities and so on.

14-00:31:18

Rubens:

Of course later he asked you to write the introduction to his memoirs

14-00:31:23
Smelser:

Most of all, our relationship developed in writing his own memoirs. I was one of his main advisors. As a matter of fact, he singled me out as being valuable. I would read everything he wrote. I would say, "That is in bad taste, Clark." I was really honest with him. He made some comment about the turkeys of Tulare County. I said, "Don't talk about the turkeys of Tulare County. All you're doing is getting yourself some bad press." I did all kinds of things. I was really, surprisingly, honest, and I think he liked that a lot. Honest without being destructive about his work. He kept sending me every chapter over time, and I kept responding to them with long and critical memoranda. It had an influence on him.

I became a kind of therapist for Clark. We would have lunch together, for example. Mostly to talk about his memoirs, but he brooded a lot about his tough years in the university from, say, his chancellorship through his presidency, where he was in the eye of the storm and he got mistreated a lot. He did a lot of thinking about what he should have done and what other people did. He had a whole world view worked out, and he brooded a lot about this. I remember one special moment in my conversation with him. He was asking me how much detail he should go into on the student rebellion of '64 on. I was encouraging him to tell a story, because he had a perspective that nobody else had. Then I said, in a moment of bravery—which I cannot account for why I said it—I said, "Clark, you should be careful in writing this up not to engage in any brooding." And he sort of looked at me and he said, "Brood?" He looked completely unconscious of what I meant. In the end, he asked me to write the preface to it. I was totally blown away by this request, because there were thirty or forty people better known than I—statesmen of higher education. Derek Bok would have written it in a moment. There were just dozens of other colleagues, presidents, who would have given their right arm to write a preface to Kerr's memoirs, but he asked me to do it. I immediately accepted and also continued to wonder why he had asked me.

14-00:34:07
Rubens:

Did you ask him why he wanted you to do it?

14-00:34:08
Smelser:

Well, what happened is, after the memoirs came out, he was given a big interview on KQED. They decided that he was perhaps too old to carry this interview himself, so they brought me over to be part of the program, to comment on the times. I played a role in that interview, but not the main one, obviously, because he was the key author. Then we were given a ride back. The radio station hired a car to bring us back to Berkeley. We were sitting in the back. I screwed up my courage. I said, "Clark, I know that there are just dozens of people that could have written an interview and drawn a lot more attention to this work than I. I'd really like to ask you, if you're willing to say—if you don't want to answer this question, don't answer it—why did you ask me to do it?" I just put it right to him. He didn't hesitate very much. He

said, “Well, I wanted someone who would do some analysis.” By implication, you don’t ask people who will just rave about the book.

14-00:35:16

Rubens: Something too laudatory or hagiographic.

14-00:35:17

Smelser: Right. And many introductions to these are just that. In fact, I did a lot of analysis in the separate introductions to volume one and volume two. He said, you have a quality of objectivity that he really respected. Interestingly—this was after the introductions had been written—I had singled out Kerr’s ability to objectify himself and history as a special strength, completely accidentally. He told me, in a way, he chose me for the reasons that I had explicated in the book. It was a very interesting episode, writing that.

14-00:36:06

Rubens: That was in the late nineties?

14-00:36:08

Smelser: That was, I think, ’97. I was director of the center at the time. He called me up. I came to Berkeley. It’s as though he thought I wouldn’t do it. Of course, I would be persuaded with one sentence. He built up to it and then asked me.

14-00:36:29

McIntosh: The Carnegie Commission that Kerr was in charge of, what were the exact dates of that again?

14-00:36:36

Smelser: I believe it began in ’68 and it went on for—well, I rejoined it after I came back from Europe in ’74, for about a year. I think it was about a seven or eight-year project. Then it disbanded.

14-00:36:52

McIntosh: I think that overlaps with your time on the academic senate policy committee, correct? I guess, obviously, they didn’t see any conflict there at all?

14-00:37:06

Smelser: Yes. And no, no conflict. I think Kerr saw me as a sociologically-minded person who’d begun to take an active interest in matters of higher education. He respected things I said. I always felt kind of a good feeling after these Technical Advisory Committee meetings in terms of my own role. No, it was a side activity that didn’t seem to have any particular implications for my role on the campus.

The time on the campus when I was on the policy committee at the time—the policy committee was a very important committee. It was what was the Emergency Executive Committee in 1964, ’65, that was faculty’s, in a way, kind of organizational response to the campus crisis. They formed a very strong committee called Emergency Executive Committee. The title suggests

exactly what it was. After three or four years, that evolved into the Senate Policy Committee, but it was the same body in effect.

14-00:38:13

Rubens: And you're serving year '71, '72?

14-00:38:16

Smelser: Yes.

14-00:38:18

Rubens: How did that come about? Had you served on the committee prior to being the chair?

14-00:38:23

Smelser: I was a member of it, I believe. That may not have appeared in my list on my bio. I was a member of it in '68, '69. I was not the chair, but I was a member of it during that really heady Third World College movement—the strike, Third World Strike. And during People's Park. We were kind of helpless. The faculty was kind of helpless in that whole thing. I did get involved in the Third World Strike. When the administration was considering the establishment of a school of ethnic studies, I got assigned, by the administration with the agreement of a faculty committee, to telephone around the country, asking minority scholars if they would join such a department or such a school. Twenty or so. Just bring back a report. I called up fifteen or twenty black sociologists or social scientists and put this question to them. I said, "This is not a feeler. This is not a job offer. It's an opinion that I'd like from you. Would you consider seriously joining such a department?" I got a very interesting finding out of this. I got, unanimously, favorable views toward joining the Berkeley faculty, but they all wanted joint appointments with a regular department. That was a very interesting thing that I didn't exactly anticipate.

14-00:39:44

McIntosh: So nobody was willing to just strictly be—

14-00:39:47

Smelser: Well, there was a feeling, and the faculty was in large part responsible for this feeling, that these were going to be second-class institutions, academically. That feeling, I think, spread generally. That probably was what was behind the idea that they wanted to be full citizens of the campus. That's the impetus I attributed this quite uniform response to my questions.

14-00:40:12

Rubens: And the department still had—

14-00:40:13

Smelser: Department still had the clout. Still does. Applies to ethnic studies, applies, some degree, to women's studies. That was very hard to shake that imagery of second-classness, and that's the faculty's doing.

14-00:40:32

Rubens: Are you saying that probably through your role on the policy committee, you were asked to do this?

14-00:40:36

Smelser: Well, yes. This was the time when Reagan tried to close down the campus after the Reconstitution Movement. Reagan lashed out at the campus. Said, we're going to close you down. You're not behaving like a university. We got closed down for one day. I remember being the author of a very strong resolution that came out of the policy committee, saying that, in effect, we condemn the closing of this campus, either by radicals who want to change it into a social action group, or by a governor who wants to punish. It got highly quoted.

14-00:41:25

Rubens: Would you mind giving an overview of what the Reconstitution Movement was?

14-00:41:29

Smelser: Yes. The reason it was called the Reconstitution Movement was that it was right after the shootings on the students who were protesting the invasion of Cambodia and the escalation of the Vietnam War at Kent State and Jackson State. A big movement. It was the last gasp, in my interpretation, of antiwar activism on the Berkeley campus. It was called Reconstitution, and the intellectual spokesman for this was a political scientist by the name of Sheldon Wolin, who subsequently went to Princeton, but he was a Berkeley political science theorist. Reconstitution meant, we're going to reconstitute the classrooms of this university into political action groups, and we're going to go out in the community and we're going to talk to labor, and we're going to talk to citizens, and we're going to convince them of the wrongness of the war, basically. It was triggered, of course, by those two campuses. Many joined. They went down to Fremont, they went into the factories and other places. Some faculty members cooperated in condoning or joining in the turning of classes into political action groups in the community. It was basically a failure as far as its impact was concerned. After all, you go into the Toyota plant down in Fremont. These guys don't particularly like these spoiled students coming down, telling you what you should think about the war. There was a class antagonism that kind of self-defeated that whole enterprise. But a lot of students took part in it, and it convulsed the campus. That's what led Reagan to saying, this campus should be closed. So he did. Symbolic closing of one day at the time. The policy committee attempted to make a judgment on this matter. I was at the center of that.

14-00:43:25

Rubens: Were the regents—they must have been pretty hysterical about—

14-00:43:28

Smelser: Well, the regents, I think I mentioned that they immediately called for an investigation as to whether or not the educational process had broken down,

and called for Heyns to give a report on the Berkeley campus. That's when it was kind of soft-pedaled and Heyns got into trouble. Students came to me, teaching assistants—what should we do? They were worried. It was an unprecedented sort of thing. I didn't have any answers.

14-00:44:03

Rubens:

Were there a significant number of professors who participated?

14-00:44:06

Smelser:

Small. Small number. I couldn't give you a percentage. I'd say, at maximum, 10 percent of the faculty would be involved in this. But that's enough to be visible. It was quite clear that these students were going out into the community in various places, and sometimes a few faculty went with them. That Reconstitution Movement was what really led to the threat by President Hitch to challenge the senate to draw up a code of ethical conduct for faculty. Hitch basically said, if you don't do it, I will. So the academic senate drew up a code that reflected, directly, the Reconstitution Movement, having to do with the obligation of professors to meet with their classes, to conduct them in an academic manner, and so on. That was, you might say, a fairly major backlash from that event. I personally, myself, thought it was a very fundamental violation of the academic life, to say, okay, it doesn't matter. You're going to get your grades. You don't have to take the exams. Go out there and just agitate in the community, and I'll grade you anyway. I'll give you a grade. Those who engaged in that, I thought, were unethical. So I was behind that faculty code.

14-00:45:32

McIntosh:

As you ascended through the ranks into the position on the policy committee in the early seventies, were these tensions still playing out?

14-00:45:46

Smelser:

Oh, yes. The Reconstitution was the last gasp. It really was. There was a brief moment in which they turned their directions toward reforming Berkeley city politics. Got nowhere. That was just an afterthought, almost. Then that whole change of culture, and students drifting back toward normal student-like activities began to work its way back more into the campus. Then the war wound down. The war ended. The main motive for all of that was the war. Well, it echoed. The hippies were still about and so on. I was ready at that time, by the way. I cannot tell you how eager I was to go on sabbatical in 1973. I was personally feeling a certain sense of exhaustion. You might say political exhaustion, because of those many, many years of being distracted. Every morning, you'd wonder what was going to happen. That endowed the sabbatical with an almost magical significance of getaway, full return to my research, leaving town, going to Europe, which I loved anyway, taking the family to Europe. Planned it all out.

14-00:47:07

Rubens:

And doing your writing, immersing yourself in it.

14-00:47:09

Smelser:

Immersing myself fulltime again in my writing. As I say, my sabbatical leave paid two-thirds of my salary. I applied for a Guggenheim, and I got it, for this project on comparative analysis. We were in Europe during the OPEC crisis. The prices of everything started shooting up. But I was comfortable enough, and it was a glorious year.

14-00:47:36

McIntosh:

You still had money from the Ford Foundation?

14-00:47:38

Smelser:

That was before. I got this in connection with my agreement to work with Parsons on this—I wanted to do a book on Berkeley. It was via the American Academy of Arts and Sciences that I formulated this conference that was to be held down at the Center, on crisis and change and conflict in the University of California. It was a collective project, in that I was the main investigator, and I got money from the Ford Foundation to write my half of that book, on change between 1950 and '70 on the Berkeley campus. We involved many other scholars in that conference, and they all contributed a chapter, including John Vasconcellos, the assemblyman. He was in the hippie phase at that time, and so it was sort of a crazy chapter. I had later dealings with—

14-00:48:35

Rubens:

And that became your book—

14-00:48:37

Smelser:

Self-esteem. Later, I had this link with John. I was scared of him, because he was such an erratic sort of guy. But we developed a relationship, and that flowered later in the eighties, with the self-esteem movement. The Ford Foundation simply supported me. I think I got a semester off from teaching. They paid my salary for a semester. I began that heavy work before going to Europe. That was behind me at that time.

I was really welcoming going. I wrote a second edition of my economic sociology book—which was not a huge enterprise, but it was meaningful—during that year. I was really happy to be back in the full scholarly commitment. It was one of the most sentimental years of my life, the year abroad in Europe.

14-00:49:37

McIntosh:

So even with two relatively newborn children, you were able to still immerse yourself in the—

14-00:49:45

Smelser:

Well, when the children were infants, it's quite clear that, from everything I've said, I was certainly engaged with the children. I spent a lot of time with them. One of the reasons that I saw the trip to Europe with such pleasure is that we were completely a unit during that entire year. We traveled all in a little camper, staying in the same place and doing different things. They were

fighting with the different languages. It was a very joyous period in both our lives. It repeated itself later in that decade, when I went abroad to London for two years. Those were very, very meaningful years. I saw myself as an involved father, but a very busy and professional and committed person. I never got myself tangled up into knots over whether I should spend more time with my kids, and that kind of thing. We had a modus vivendi. My wife was wonderful with the kids.

14-00:50:53

Rubens: You said also your wife encouraged having your older kids stay with you.

14-00:50:59

Smelser: Oh, yes. That was just assumed that they would continue those visits during the weekends.

14-00:51:10

Rubens: Then they came and visited you—

14-00:51:11

Smelser: Wednesday, I'd pick them up at school, always. That routine continued right through. Of course, when we were away for the year, and then in the late seventies, we were away for two years, but they came to England twice during that period.

14-00:51:25

Rubens: And you said they came for the holidays during the '73 sabbatical?

14-00:51:28

Smelser: Yes, that's right. Then they came twice during the time living in London, in '77-'79. By that time, they were old enough to baby-sit our younger children, so we went off to France for an eating fest while they came.

14-00:51:46

McIntosh: We are getting to the end of tape here.

14-00:52:02

Smelser: There's one thing I want to talk about. That was my role on the faculty association.

14-00:52:07

McIntosh: If you are up for going into it now, why don't we go ahead?

14-00:52:11

Smelser: I'll do it. One of the responses to the closure of the campus, to the legislature's killing of a faculty regular increase, and to Reagan's threat to cut the budget so that the teaching loads would have to go up—all that surrounded that '71, '72 crisis. A portion of the faculty decided it was time to organize. In other words, these were blows, and really considered ripe, or occasions for collective action. Especially Lloyd Ulman and David Feller, both economists and labor economists, took the initiative—they said, let's start a faculty

association of some sort. Not necessarily a union, but a faculty association. They involved me in it. As a matter of fact, I and those two formed a kind of leadership core, in actually setting up a Berkeley Faculty Association on the campus. Got several hundred members. We got the senate to approve it. It wasn't an arm of the senate, but it was approved by the senate. It was meant to be a kind of political arm. Not collective bargaining. We didn't want to define it as a union. As a matter of fact, I thought it would be political suicide to define it as a union on the Berkeley campus. We formed it, and we made statements and hired a lobbyist in Sacramento. I'd say several hundred people joined it. Paid dues. The administration took us kind of seriously. Once again, the senate voted not to go in for bargaining. So it was not in the AFT tradition of faculty unions, but was a faculty association. It was meant to have a voice made collectively, but without any particular sanctions like strikes or the whole paraphernalia of unions. That group maintained a certain validity for five or six years. It began to fall off in numbers. It still exists. It really got revitalized a bit during the big budget crises of the last couple of years.

14-00:54:44

McIntosh:

And so you think that specifically marketing yourselves not as a union was, in a way, one of the things that allowed you all to have a voice?

14-00:54:55

Smelser:

If we'd have gone into collective bargaining—I don't know. I don't think we could have. The voice was with the faculty as much as anybody. The professionalism values of the Berkeley campus are so high. There is in many people's minds a contradiction between being professionals and being employees, and that behaving like an employee is something like a renunciation of your fundamental commitments as an academic. As a matter of fact, there was an incident that I should mention. When the campus began issuing, basically, membership cards that you all carried around, a delegation of faculty went to the chancellor's office. Protested against being issued identification cards as a faculty, saying, we are not employees. That was the kind of spirit. We are professionals. We are a member of a collegiate society.

Of course, if you look at the union movement, it succeeded in lower-rank institutions, where there was much more bureaucracy and much more treating of the people like employees. The idea is, if you're treated like employees, you're going to respond like employees. So we had a very interesting, inventive process of exactly what limits to set on this thing. We had a lot of debates about collective bargaining. In the end, the Berkeley senate voted not to become a collective bargaining agency for the faculty association. We asked the senate to act on that, and they voted, by sort of a narrow vote, not to go in for collective bargaining. It evolved in the direction of a lobby rather than a union.

14-00:56:40

Rubens:

It sounds like it must have occupied quite a bit of time.

14-00:56:44

Smelser:

We did a lot of planning and meeting. We hired a staff guy with the membership fees. I wouldn't say it was as demanding as a lot of other things, but it required attention. It was an organizing effort, and sort of one-of-a-kind activity on my part. It wasn't exactly in keeping with my self image to be an activist, but this was a kind of activism, if you will, on the part of the faculty.

14-00:57:12

McIntosh:

Reagan's relationship to the campus was uniquely antagonistic, it sounded like. I can see how—

14-00:57:20

Smelser:

He ran on two planks in 1966. One was to clean up the mess at Berkeley, and the other one was to get rid of California's wasteful welfare system. These were the two great planks. Almost his very first act in coming to office was to propose a 10 percent cut in the university budget. I was on the Board of Educational Development and in the chancellor's circle. We invented a proposal to give Ronald Reagan nine-tenths of an honorary degree after that. He was hostile both verbally—verbal abuse against the campus never ended—and budgetarily very tough. His successor, Jerry Brown, was also not very friendly to the university at that time, but not as negative and not as truly antagonistic as Ronald Reagan was.

14-00:58:18

McIntosh:

We're going to have to wrap up pretty soon, but maybe we can conclude with, did this faculty lobby have any successes?

14-00:58:27

Smelser:

Hard to measure. We commented on various kinds of legislation. We'd always comment on the budget. There were a hundred other voices commenting in Sacramento. We kept contact with the university lobby and diplomatic relations with the university lobby up there, which was hired by the systemwide administration. The local administration would listen to us. Let's put it that way. I cannot rewrite history and say what specific results we had, but in a way, it took the administration by surprise. It also took them by surprise that respected faculty members were taking the leadership. Sanford Elberg even was involved in it. A dean, Sanford Elberg. He was involved in our group.

14-00:59:13

Rubens:

Was Peter Dale Scott involved in that, too?.

14-00:59:17

Smelser:

No, he wasn't.

14-00:59:19

Rubens:

He organized a faculty peace committee.

14-00:59:21

Smelser:

He was much further left. He was a peacenik. Peter Dale Scott and I lived, during the time I was working on my dissertation—neighbors in London. Became very close friends. We subsequently parted politically, because he became a real extremist in the Vietnam period, and I wasn't in that camp.

14-00:59:39

McIntosh:

I can see how 1973 would be a very welcome break.

14-00:59:44

Rubens:

A real odyssey.

14-00:59:45

Smelser:

Yes, it really was. It was something I promised Sharin when we got married. I said, "Next sabbatical, we're going to spend a year in Europe."

14-00:59:57

McIntosh:

I guess that will be a good place to pick up next time.

14-00:59:59

Smelser:

Okay. Pick up that year.

14-01:00:00

Rubens:

What a wonderful session.

14-01:00:02

Smelser:

Thank you very much.

Interview #8 April 19, 2011

[Begin Audio File 15]

15-00:00:02

Rubens: Today is the nineteenth of April and we're on our eighth session. We're moving along very nicely. Before we move to your sabbatical abroad we wanted to talk about some of your students.

15-00:00:23

Smelser: Well, by the time I was here, by late 1960s, I had been here for ten years, and I had begun to accumulate a great many graduate students. Some of them via the orals examination, some of them via the fact that I hired quite a few good graduate students as teaching assistants for my theory course. Already a relationship was formed, and many of these students chose me to be head of their dissertation committee. I can mention just some names. Gary Marx, Max Heirich, James Wood, Susan Garfin, Barclay Johnson, Whitney Pope, Sam Kaplan, Ruth Wallace, Herman Blake (he was the man who went down and started Oakes College at Santa Cruz). And I kept a very close relationship with him. Arlie Hochschild, who later became a very famous faculty member in our own department.

15-00:01:15

Rubens: And who wrote the introduction to your edition of—

15-00:01:19

Smelser: Yes, that's right, for the sociological theory book. Jeffrey Alexander, a very famous theorist who's now at Yale. Jeffrey Prager, whom I saw through psychoanalytic training, and he went to UCLA. Elbaki Hermassi from Tunisia. I'll tell a story about him. Hermassi was an extremely brilliant graduate student who had gone through an advanced degree at the University of Paris already. From Tunisia, he had had that link, and he came to the campus here. He wrote a dissertation, under my direction, on questions of social change in several different North African countries. It's really a very brilliant piece of work, and subsequently got published here in the United States. Hermassi and Hochschild—we made two exceptions to our general rule of appointing them to our own faculty as assistant professors. This was a taboo, normally, like many universities have. They don't appoint their own graduate students. They may bring them back after a season in the field, but here are these two people that we felt were especially strong, and we appointed them.

Hermassi, you may or may not remember, during the course of his assistant professorship, got into deep trouble. There was a question of sexual harassment. A couple of female students brought charges. Hermassi denied any wrongdoing. There was a rather extensive faculty hearing, led by a professor from the Davis campus, who came in and made an investigation. The university gave Hermassi one term enforced leave without pay. It was a

bit like a prison sentence, in the sense there was a punishment there, but did not go further. He came back onto the faculty. I was always very close to Baki. I talked with him. Probably the only informal conversations he had with anybody on the faculty were with me. Because I had been his doctoral dissertation supervisor, and because I had a close intellectual relationship with him. I tended to be a person that he could talk to. Then came the vote for tenure. My view on it was that the tenure decision should be based strictly on the man's professional record, because he had paid his price. This was my view. It ought not to be a blot that would have a decisive role in his tenure promotion. That didn't turn out to be the case. He was recommended, and it went up the line, and the administration stalled on it to such a degree that he didn't get promoted. I tried to work on Heyman later. This was later, after he had been six years in the assistant professorship. He was not promoted.

15-00:04:59

Rubens:

Were you chair at the time when that happened?

15-00:05:00

Smelser:

No, no. He was assistant professor when I was chair, but the harassment issue came up later. It was when I was abroad that a lot of this happened, but I made my position clear. So he wasn't promoted. He decided, under the circumstances, to go back to Tunisia. He did get a job at the University of Tunisia. However, it was most interesting the way news travels. I received telephone calls from Tunisia, asking me about this case. Even they were curious or wanting to know about what it was. It followed him all the way there. He was appointed to the University of Tunis. He became a high-level consultant with the Tunisian government on issues of urban policy and so on, and ultimately was appointed ambassador to UNESCO from Tunisia. We happened to see him in Paris subsequently. That was an extremely interesting story. I got severely criticized for my position in the department.

15-00:06:22

Rubens:

By your colleagues?

15-00:06:23

Smelser:

By colleagues who were opposed to Hermassi's being approved. This was the one time in my life there was red paint splattered on my office door, and a couple of other faculty members, by feminist groups that were angry. I didn't keep my position a secret. I thought that he should receive completely fair treatment. But that episode was one that drew me in, ultimately, into a controversial situation. I'll talk about my story with Arlie Hochschild when we start talking about the chairmanship.

15-00:07:08

McIntosh:

Before we move on, may I ask you a follow-up question about this Hermassi case? Within your department, the colleagues of yours who were against promoting Hermassi, was it strictly because of this one incident, or were they using this incident as an opportunity to kind of critique his work as well?

15-00:07:32

Smelser:

The active criticism came from the left—came from the Kornhauser, Matza, Blauner, and to some degree, Duster—a group that had moved to the left in the split of the department that I'll talk about also during the chairmanship. It was a partisan one. The thing got kind of ugly, too, because Hermassi, in the heat of the battle, he read it as—I wouldn't call it a Jewish plot, but that this Jewish/Arab issue kind of came up in the middle of it. He felt that he was being wronged from the standpoint of his own background. I couldn't establish any evidence for that one way or the other, but that was the internal dynamics of it.

15-00:08:27

Rubens:

I think we can leave feminists concerns for when you talk about your chairmanship. It was an important issue, as we see here.

15-00:08:41

Smelser:

Very much so. I would like to talk about the divisions that were there when I came in as chair. That will be good.

15-00:08:48

McIntosh:

Back to your students. Did you actively recruit anyone, or did they gravitate towards you?

15-00:08:55

Smelser:

No. I was known. People have told me, decades later, the reputation I had among graduate students. That among faculty members, I was regarded as a student-friendly professor. The answer to your question is no, I did not recruit people. I don't think I turned anybody away. But on the other hand, I was very happy to serve people who came. I was proud that I was having increasing numbers of students and I was conspicuous as a director and educator of future professionals. I had a given style. I made a kind of philosophy not to press my own research agenda on students. I supervised the most enormous variety of dissertations, from different points of view, different styles, different theoretical approaches, different research methods, and so on. My philosophy was that I could do best by taking seriously what these students were doing, and try to bring them along, try to show them further ways of looking at things. To criticize what they were doing, but not to say, this is the way it was. I was very far away from wanting to clone anybody. That just happened to be an evolving personal philosophy that I did not enunciate publicly, but it just was a practice that guided my supervisory efforts.

15-00:10:34

Rubens:

With all your contacts and publishing experience, were a large percentage of the dissertations—

15-00:10:40

Smelser:

Well, to go back to those two, both Arlie and Hermassi, I pushed their dissertations to Prentice Hall, where I was the publishing advisor. Quite a few of my students got published. This happened to be with that particular link

that I had with Prentice Hall. Prentice Hall was publishing some scholarly books. That fell off later as they began concentrating on texts and more sellable books. So I was helpful, and of course I was always supportive of students when they were trying to get jobs outside the university.

15-00:11:22

Rubens:

Did most of them get hired, then? Well, we'll talk about your book *The Academic Market*, because that was, again, particularly—

15-00:11:27

Smelser:

This was just before the collapse of the academic market, actually, which was in the very early years of the 1970s.

15-00:11:34

Rubens:

So we'll come back to some of these issues. Did you meet with your graduate students one-on-one or was there a dissertation group?

15-00:11:51

Smelser:

Generally speaking, the pattern was the following. The chair was the decisive member of the committee. The student would, in varying, different styles, rely on the other dissertation committee members to advise, but the key guide and the key mentor was always the dissertation chair, who would follow it chapter by chapter, draft by draft, and give the student input and tell him when you thought it was finished and so on, and give the first decisive reading to it. The other two members, one in the department, one outside the department, would generally follow the lead of the chair if the chair found the thing satisfactory. They would read it, but they tended—deference is too strong a word—but they tended to be influenced very much by whether or not the chair had decided. That was a pattern that I followed. I was on a lot of other dissertation committees as second member, and quite a few outside member committees in history and psychology and other departments. When I was chair, this was the typical pattern, of being a pretty exclusive guide during the working period of preparing the dissertation, and being a kind of decisive judge once it was finished.

15-00:13:14

Rubens:

Did you find that the students were collegial? Did they form a group? Of your primary dissertation students, did you work in groups?

15-00:13:22

Smelser:

No, no, no. This was mostly one-on-one. I'd contrast my style with that of Parsons, who always had a group of his people, who would meet together periodically, and discussing mostly his theory. But I didn't have that. It was pretty much one-on-one. That was sort of Berkeley's style, I suppose.

15-00:13:47

Rubens:

I think you wanted to say something about TAs in Sociology 109?

15-00:13:53

Smelser:

Oh, yes. This was recruitment grounds. I've mentioned that. I formed a relationship with all my TAs, even when there were five or six of them. I met with them separately. As the course went along, I would have a weekly meeting with my TAs. Not always, but I did keep a tab on my TAs, and I would go to classes from time to time that they were teaching. It was something a little bit more intense than the usual teaching assistant that you just get—they meet their sections and then they help you grade the papers and assign the final grades. I had a somewhat more intense relationship. I didn't have any conflicts with the TAs. It was really very good collegial sort of relationships. For that reason, they would drift toward thinking I was a good person to work with, and I got chosen to direct dissertations. I had lots of students.

15-00:14:52

McIntosh:

Since we have focused on your research in past interviews, now that we're on to your relationships with students, I'd like to just dwell on it for a little bit longer. What obligation did you have towards undergraduates during this time as well? Are you teaching undergraduate courses?

15-00:15:10

Smelser:

Yes. I would teach different undergraduate courses on different topics, like collective behavior and so on, but the main one was this theory course. This was a required course for all majors. There were students also flocking in from other departments, at the recommendation of advisors, and I believe social welfare even required it of their students to come take that course. A lot of political science students came to take it and so on. That was the main one. I always followed the following procedures. I would declare that the class should be encouraged, despite its size, to interrupt at any time, if anything was either unclear or they found that I was off the track or whatever. So I actually carried on a good deal of dialogue in the extremely large classes. Sometimes I'd say, "I'd like to get back to the lecture" if it was getting too much, but then I tried to keep as open as possible in a class of that size. I also encouraged the students to come to my office hours. That's very hard in the Berkeley culture. Students tend to stay away. You really have to actively encourage them, and even when I encouraged them, students were always apologizing for taking up my time in office hours. That's what they were for—to have your time taken up. There was a kind of timidity among the majority of students. Some would come in. And there was an occasional pest. But by and large, you had to make sure that you were, in fact, open.

I also had a policy of writing a letter of recommendation for undergraduate students who were applying to graduate school if they requested it. I couldn't know them. I couldn't just say, in a letter of recommendation, that person got an "A" or something like that. So when I got a request—there were not too many, and it wasn't overwhelming—I would ask the student to indicate who the TA was, and I would talk to the TA about that student. I would also ask the students to bring me the papers that they had written in the course so I

could review them. I tried to make it a little bit more than stereotyped Berkeley relationships with undergraduates.

Then, at a certain point in teaching that course, and subsequently in teaching Sociology Five, the methods course that I'll talk about when I talk about the chairmanship. We would have the policy of each section of the class—there were maybe a dozen sections in these classes—each section, we would invite over to the house one evening during the semester, with their TA, and just have a social occasion in talking with the students. Subsequently, a lot of students would tell me that they didn't remember anything I taught, but they remember that evening, which was not a continuation of the intellectual content. It was just a social occasion. That helped also in this kind of setting.

15-00:18:40
Rubens:

So in light of claims about alienation of students at Berkeley in general. I see that you were obviously extending yourself and acknowledging the largeness of the—

15-00:18:54
Smelser:

Trying. You have to admit that the situation for any kind of breaking through of this large lecture pattern is not fortuitous on the Berkeley campus. The numbers alone militate against it, and it's also a quite individualistic culture. Most professors find it, frankly, somewhat of a nuisance to give extra time to teaching. They don't get very rewarded for it, particularly, except maybe in terms of gratitude on the part of some students, but it's certainly not the sort of thing you would do if you're making calculations about how to advance your career. It just seemed to me to be an important thing to do.

15-00:19:44
Rubens:

Maybe this is a good segue into your work at Cowell, to talk about it in terms of students you saw, because this gave you another avenue of understanding what students were experiencing.

15-00:19:51
Smelser:

Yes, and it's very interesting because it tied in with my style with students, in a way I'll indicate. As I indicated before, my affiliation with Cowell was tied up intimately with my psychoanalytic training. I didn't have any clinical training when I went into the psychoanalytic institute. I think, properly, they did not want to inflict me on patients without any advanced clinical training. They wanted me to spend a couple of years in less intensive therapy. The head of Cowell Hospital at the time, Harvey Powelson, sort of knew me through the Psychoanalytic Institute. He was a psychoanalyst, and he was very receptive to my coming there. He supervised some of my work. I had other supervisors among the senior therapists at Cowell. It added up. It was irregular. When I was abroad, of course, I couldn't continue to see people, but during and after my own psychoanalytic training, I was on the staff for a total of, perhaps, five or six years. Just one day a week, usually, maybe seeing four or five students,

with an upper limit that they had of twelve therapeutic sessions per student because of the demand that they wanted to accommodate.

But I was indistinguishable from the other therapists as far as the students were concerned. They did make a policy of keeping students in the social sciences from being assigned to me, on grounds that they might have been in my class, or read something I had written, or something of the sort. That always complicates the therapeutic relationship if they are aware of the other aspects of your life. I always joked with them about they're sending me nothing but engineers, who are reputed to be dull patients. [laughter] They would come in and say, something's not working, fix it, sort of approach. We had a lot of office humor about that. I saw a great diversity of students, men and women and so on. I must say that the fact that I was a faculty member didn't, except on one or two occasions when people accidentally had some relationship in class, make any difference. I was just a therapist as far as most of my clients were concerned.

With those kinds of limitations, you couldn't get too deep into the students' lives, but it's amazing how much you could do, even in a limited number of sessions. Most of the students had somewhat everyday problems. Fights with parents, difficulties with boyfriends and girlfriends, studying blocks, worries, unhappiness, unable to function very well, loneliness. The biggest single complaint was, especially from female students, worried about eating patterns.

15-00:23:13

Rubens:

Eating disorders were kind of a new thing?

15-00:23:18

Smelser:

Sometimes they were real, in terms of the person was obviously overweight. Sometimes they were psychological, in that the person was preoccupied with it, and doing crash diets and that sort of thing, but very worried. That was the biggest single presenting complaint at Cowell Hospital.

15-00:23:35

Rubens:

Anorexia as a category seemed not to be so prominent until—

15-00:23:40

Smelser:

No. Well, there's a state of life of people, young, preoccupied with looks and other things of that sort. It turned out to be a frequent presenting complaint. I didn't have too many people come in with that myself. That's just the luck of the draw. I have to say that one got some gratification out of helping students. In addition, I came to be instructed myself, as a faculty member, as to what students' lives are about. So many faculty members have this idealized conception of the student as the perfectly motivated person who likes their studies and who's willing to work and will respond to academic sanctions and so on. It's so much more complicated than that in terms of the real life of an average undergraduate. There are so many different things going on, and of course you come to appreciate that when you see them, when they conceive of

themselves as being in some kind of trouble or suffering in some degree—psychological discomfort. They come with a complaint. So you follow those as much as you can. You got an occasional person who wanted to use Cowell Hospital as an excuse to get an incomplete or for academic exploitation. We just stayed away from that particular role. We didn't communicate with any of the faculty of any of these students, as a matter of policy.

Now, sometimes, in talking with students, particularly graduate students, a kind of therapeutic relationship would develop with me as a professor. People would talk about their career uncertainties, or a couple of occasions, difficulties they were having with other faculty members would open up. I had to sort of make sure that I kept the lines drawn that a faculty member is not a therapist. On these occasions, I would almost always refer them for personal help, but not try to administer it myself, even though I was in the very same business at the same time at Cowell Hospital. I really considered that to be one of the most refreshing aspects of my work as a faculty member, to have this independent experience with student clientele at Cowell Hospital.

15-00:26:33

Rubens:

In terms of what your actual practice of your therapy was, one thinks of a psychoanalytic orientation demanding a long time and a relationship developing between the—

15-00:26:48

Smelser:

Yes, that was an obvious tension that my training was in an entirely different style. However, there developed a lot of variations of psychoanalytic treatment among psychoanalysts themselves, called briefer psychoanalysis. Psychoanalytically oriented psychotherapy, meaning you took account of the fact that the person wasn't there for so long by being perhaps a bit more directive. There are all kinds of writings in the psychiatric literature about the adaptation to time limitations, and Cowell Hospital was one of them. I found that of course it wasn't appropriate to use the full psychoanalytic technique, in which you might remain silent for three hours when the person was free associating. But on the other hand, the kind of insights you could bring and the kind of interpretations you could bring were fully informed by my own training. I didn't find myself fretting about inability to do some good work, even in a short period of time.

15-00:28:01

McIntosh:

So you've mentioned how working at Cowell changed your understanding of students' motivations, but did it change your understanding of university culture and what the university should be doing? And also, did it have any influence on your pedagogical approach during this time?

15-00:28:20

Smelser:

I developed, over time, irregularly, little lectures I would give to—presentations, or maybe in forums, or maybe it was a lecture—that I would give on different aspects of culture on the campus. I had to talk about

undergraduate culture, graduate student culture, faculty culture. I talked a lot about it. People were always very interested to hear someone take a step back and look at it from an objective point of view. Whenever I would talk to, say, gatherings of faculty advisors or officials in the College of Letters and Sciences, I would bring what I thought I had learned from the Cowell experience to bear, stressing the complexity and the psychological contingencies that students all experience. Trying to get that lesson through to people who were in, you might say, bureaucratic positions, and dealing with large numbers of students in an official way. I guess the answer to your question is yes, it spilled over in my general conversations and presentations about life on the campus.

15-00:29:41

McIntosh:

It seems like that had primarily an influence on your approach to undergraduates, but graduate students, it sounds like you're a bit more hands-off. Kind of encouraging, but not—

15-00:29:53

Smelser:

Well, I would develop closer relations with the graduate students because I'd see them more frequently, and obviously they had already made their commitment to what field they wanted to go into. I would say that my style of openness and relative accessibility was constant with all students. I did have a somewhat more instrumental relationship with graduate students. I often said to people that I found undergraduates more interesting because they were less under your control than graduate students. There's a whole complication of pleasing the faculty member, of not saying anything that would offend, and not getting on the bad side of a faculty member, because in graduate school, a faculty member has much more control over your career. In that sense, I would always say, not meaning it to be meant as a universal generalization, that the undergraduates tended to be more interesting and refreshing—the bright ones—than the graduate students.

15-00:31:00

Rubens:

By the 1980s, students' problems with prescription and hard drugs, sexual harassment, depression leading to suicide, the phenomenon of cutting are more evident. Those are things that seemed not so prevalent in the seventies.

15-00:31:32

Smelser:

Of course, I got a skewed view of student problems, because I was in the hospital. People chose to come there because they were having problems. I didn't generalize and say that all students were psychological wrecks by any means. Of course the problems differed, and one of the most interesting things that I and everybody else at Cowell experienced was the flow and preoccupations with the clients during the Free Speech Movement and afterwards. Demand fell off. In other words, there was something going on with this intense social life that accompanied the activism. In a way, a strong commitment to some kind of cause usually means a diminution of personal insight and a subordination of personal problems to larger commitments. We

had not only a fall-off, but once that episode was over, there was a kind of let-down on the part of many students that, in fact, life is going on, and that the mundane problems returned after this drama, and so on. A few students that came in were in this realm of having been quite active, but now finding themselves at loose ends. A little depressed, you might say, after the tremendous excitement that those months carried.

15-00:33:15

Rubens:

All right, should we move on? In your outline that you've given us, you mentioned job offers.

15-00:33:27

Smelser:

Yes. In 1970, when I had been here a dozen years, and I was at that time forty years old, I received, simultaneously, offers from three institutions on the East Coast. Penn—this was the work of Martin Meyerson. He had just gone to Penn and he had a longstanding and very positive relationship with me. He engineered that offer, I'm certain, to come. The Penn sociology department didn't object to it. It was a name professorship—a Benjamin Franklin professorship there. Then Yale simultaneously offered me also a high-level chair. I had been offered a position at Yale twice before and had declined to go. Whether they heard from Penn that I was going to be there or what, I don't know. I have no idea what the background was. But I had developed a relationship with Kingman Brewster, their president. They were very concerned about their sociology department. It was really much lower in quality and reputation on that campus than almost all other departments. They were trying, time and again, to build it up, but they weren't doing a very good job of it. It was one of those institutional embarrassments. Brewster was aware of that, and he would call on me from time to time, both about what their strategies ought to be and to ask me to evaluate different people they were thinking of appointing. So I had a relationship with Brewster, and when I went to Yale, I saw him again each time, so there was an element of additional familiarity. This was a straight tenured professorship in sociology, also with a name attached to it. Then, simultaneously, Harvard came after me again. I had declined to stay at Harvard after my Ph.D., as I mentioned, and I had been given one other offer in the meantime that I had not taken, but this was the big one. The symbolism, of course, was Parson's retirement. That was the very year Parson was retiring, in 1970, and nobody said it was a replacement, but the aura was there.

15-00:35:56

Rubens:

So 1970 is a very eventful year that these three—

15-00:36:00

Smelser:

Same time. As a matter of fact, Sharin and I, with our little son, Joseph, took a trip to the East Coast and went to all three of these institutions, sequentially, in December of that year. It was horrible weather. I was interviewed and courted by each one of the institutions. It was very interesting to contrast the style of the three. Penn treated us like absolute royalty. Gave Sharin a driver and a car

to go around Philadelphia, and just was completely smothered. Yale was sort of intermediate, even though it was very generous, and they took Sharin around to look at houses in different parts of the community. Harvard had the old Harvard kind of ideas, that we ask you and you decide whether you want to come here or not. The dean of the College of Letters and Sciences then was John Dunlop. He also was somewhat aware of my work, but he was a guy that worked closely with Kerr in industrial relations. Dunlop had a knowledge of me as well. There was kind of a linkage with all three of these departments. Interestingly, to make a kind of ironic point, each one of them offered salary and perks in accord with, you might say, their reputation. Higher salaries from Penn, intermediate from Yale, and lower from Harvard. Not lower than what I was getting, but just ranking among themselves. Each one of them had their own view of what it took to get a new faculty member, and it showed up in the conditions of the offer.

This was an extremely difficult time for me to decide. I had a really tough time. There was one offer later, to Princeton, that came during the time I was on the EAP [Education Abroad Program], that tempted me a great deal. My whole past history at Harvard and my whole past relationship with Parsons was there. It was psychologically very big for me. Of course, I had developed, as I've indicated in these interviews, an ambivalence toward Harvard. A huge attraction and a deep gratitude for what Harvard had done for me in my career. If it weren't for my going to Harvard in the first place, I would have been someplace very different in my own career. It was a sweat. Sharin was willing to go to any of these places. I think she probably preferred Penn and Yale to Harvard, just because of the general kind of atmosphere.

I have to tell you one very funny story at Harvard. The chair at the time was George Homans. He was one of my dissertation advisors. We were very different in our outlook and had come into some kind of conflict. He was playing the proper role of the chair, so he arranged that during my visit there would be a dinner at his house, as part of the courtship. But he invited only men, and Sharin was there. Marty Lipset, who otherwise was not the most sensitive man in the world, went to Homans. He said, "You've got to invite women. What do you think this is going to do? He'll walk away from here if you don't do it"—so Homans, who is the real old Harvard, descendant of the Adams' family, finally grumbled. He said, "Okay, we'll do it." So he reissued the invitations to bring spouses. This was the last of the three trips. Sharin was totally exhausted. We had this little baby with us. She said to me, "You go. You go. Just too much for me." I said, "Okay, fine." So here I appeared, without a spouse. Homans saw me arrive at the front door. He nearly had a heart attack because he had wanted this whole Harvard cigar-smoking kind of all-male thing to go on. [laughter] He, in the end, at the meal, he did take all the men upstairs for their brandy and cigars, and left the women to their cigarettes and crème de menthe downstairs.

15-00:41:04

Rubens: They were probably used to it, being Harvard spouses.

15-00:41:07

Smelser: Yes, it wasn't a foreign thing then by any means, but I thought that was a very ironic sort of story.

15-00:41:14

Rubens: Were you in touch with Parsons?

15-00:41:16

Smelser: Oh, yes. Parsons was never shy about putting direct pressure on me. I think he himself saw me as a kind of heir. We had had our troubles in writing this book that we had—collective project—but he was very loyal and very persuasive for me to come to Harvard. He kind of saw me as a worthy person to carry him on. I was also worried about the fact that they were very likely to ask me to be chair when I went back. It just was in the air. The department was a little troubled. It was right in the last stages of separation with the Social Relations Department. They were looking for leadership. While no one broached it to me, I sensed that the guns were loaded, and when I came back there, there was going to be a lot of pressure on me to be chair, which I didn't want to be, given the circumstances of the department. So that figured in my thinking as well. But in the end, I decided to stay here at Berkeley.

15-00:42:33

Rubens: But closely tempted to any one of these?

15-00:42:36

Smelser: I had reasons to want all of them. There were positive things about all of them, but I think Harvard was the most significant, for reasons I've indicated. Berkeley was really eager to keep me here. That's when the Institute of International Studies, had an arrangement via a series of Ford grants, to relieve some faculty members of half of their teaching time, indefinitely. It was an endowed basis of making them half-time in the Institute of International Studies and half-time teaching. David Apter, who was still at the institute, simply said, "This is what we'd like to propose as a way of making Berkeley even more attractive than it has been in the past." Then Roger Heyns went out of his way—because I had been in his administration—he went out of his way to call me in and ask me what they could do to keep me here. We discussed a lot of possibilities. I, myself, who was already beginning to feel a discomfort in the middle of the fragmentation of the Department of Sociology, I was the one who mentioned the possibility of a university professorship. In a way, it was kind of a way for me to carry out more interdisciplinary work. It was a way—I didn't exactly formulate it in these terms, I have to say—of being less dependent on my own department, which already, I was feeling, was a troubled part of the institution. Even though I didn't leave it, I had a long history of discontent with the depth of conflict that the department had developed. The university professorship, in my mind, was not only an honor—it was something that was available only to physicists and hard

scientists up to that point. Heyns went out of his way to recommend that I be made a university professor. It took a long, long time. I had decided to stay at Berkley even before it went through. It had to be reviewed at every campus.

15-00:45:05

Rubens: Oh, really?

15-00:45:06

Smelser: Ye, because you're a professor of the whole university, so they had to run it through the sociology department of every one of the nine campuses. It was just one of these huge reviews. I didn't really hear for six or eight months, even though a couple of my intimates here on the Berkeley campus kept me posted as to where the appointment was. I decided to stay at Berkeley before it came through, but it did. That, of course, did, in fact, change my life, because while I was still paid out of the budget of the sociology department, I could plan my teaching and visits any way I wanted, throughout the entire university. It was, in a way, a declaration of independence, even though I was still a member of the department.

15-00:45:57

McIntosh: It sounds like, during the search, one of your priorities is independence, just to label it. Another sounds like minimizing teaching requirements. Is that also—

15-00:46:12

Smelser: I didn't fight for that. That was put to me on a plate. I certainly wouldn't have asked, the way Erving Goffman did, to have my teaching simply cut in half. I wasn't that alienated from the teaching. It, of course, was a huge plum to be given this kind of permanent half-time status with regard to teaching, and with the expectation that part of that half-time teaching was going to be done at other campuses. That was even more independence from the department. I did take on the chairmanship of the department after the university professorship was granted. I wasn't totally alienated, if you will. But nonetheless, you're absolutely right that independence of several types was a theme that made the package that Berkeley put together extremely attractive.

15-00:47:06

McIntosh: This affiliation with the Institute of International Studies—were there any obligations to that institute that you had to fulfill?

15-00:47:16

Smelser: No, but I did assume the position of associate director of that institute, about that time. I didn't have too much to do in the direct administration. The director really took care of all of the budgetary and planning and the fundraising and keeping order and the staff of the institute, which was located in the old Anna Head school at first, and then moved right into the center of the campus, Moses Hall. They gave me an office in addition to my Barrows Hall office. I became kind of an intellectual spokesman for the Institute of International Studies. David Apter was the first director, then Ernie Haas and then Carl Rosberg. I was in there the whole time that these guys were the

directors. A pattern sort of developed that I would be kind of a spokesman or main person to say things about the intellectual policies of the institute, without having very much administrative detail on my portfolio. That was a rather rewarding affiliation.

15-00:48:40

Rubens:

What was that intellectual policy was? What research were you doing?

15-00:48:49

Smelser:

There were two issues facing the institute at the time. One was its relationship to the Area Studies centers. These were the Chinese Center, the Near Eastern Studies Center, Latin American Studies, and so on. The institute was an umbrella organization, administratively responsible for all of them. They all reported to it. It was a war. These regional study centers tended to act like colonies who were completely dissatisfied with the subordination to the Institute. The whole question was always exactly what kind of balance you're going to strike between administering these entities or giving them full freedom, allocations of funds from the center. Later on, it became much more a declaration of independence on their part, but at this time it was a kind of constant back-and-forth as to priorities of the institute, some kind of intellectual division of labor, and so on.

The other issue, an intellectual issue, was the degree to which it was going to be a handmaiden of the political science department and the study of international relations. All these directors were from political science. There was another side, of course, to what degree should international relations subsume comparative studies? Of course, I was in the middle of my big comparative studies work at the time, so I was kind of a spokesman for making it catholic. By making a comparative studies program, you bring a lot more historians in. You bring anthropologists in. It becomes truly more interdisciplinary than just political science. This policy won the day in the institute. That's in response to your question, what kinds of issues came up that I could play an intellectual role. Those were two that come to mind.

15-00:50:52

Rubens:

Did you funnel any research money through that institute? The Ford grant?

15-00:51:11

Smelser:

The Ford grant, I believe, came through the Office of Sponsored Research. I wasn't tied to the institute for that. But when I came back from the year abroad, I had been given a fulltime secretary as university professor. I'll get to that when I talk about Princeton. She had an office in International Studies, in Moses Hall. She and my office were one and the same. So everything I did—all the research, all the activities, even when I was active in the senate—things kind of converged on—it was my home, much more than the department was, the institute, because I had my secretary there. I went to see students in the department, I taught through the department, but the Institute of International Studies was really much more of a home for me.

15-00:52:07

McIntosh:

Was your relationship with Apter a substantive one for you? Do you remember any sort of specific goals that he had for the institute?

15-00:52:17

Smelser:

Well, he left so early, before I became really active. Apter was a big friend and supporter of mine. He brought me into these programs that the institute was sponsoring for foreign scholars. I remember lecturing before a group of Indonesians who had come to study at Berkeley. He was very active in the Peace Corps, so I talked to a couple of Peace Corps volunteer groups through him. He was very supportive. He was the one who got me to be head of this comparative scholars group when the Ford Foundation gave a huge hunk of money to the Institute of International Studies. He fashioned this Scholars Group, and specifically wanted me to head it up in the early-to-mid sixties. Then, of course, he was the guy who, pretty much on the eve of his departure in the seventies—he went to Yale—organized this research appointment for me. We had a good intellectual relationship. We were both interested in economic and political development. We were intellectually congenial, and I know that he had a lot of respect for my work, and that was mutual, so it was very positive. I worked more circumstantially with Haas and Rosberg during my administrative period. I never was the associate director under Apter.

15-00:53:43

McIntosh:

We have about five minutes left on this tape. But how we got off on the Institute for International Studies was as part of your interview process with these universities. I wanted to get back to that for just one more question, which was, did Harvard and Yale and Penn, and Berkeley as well, did they force you to articulate your plans for your own work going into the seventies and eighties?

15-00:54:10

Smelser:

No, they just wanted me to be there. I gave a talk at each place. It was not a job talk. The job offer was in hand when I went. That was one of the great luxuries of this era. It was a seller's market. I remember talking on my work on corruption at Harvard, and I gave an intellectual presentation at both of the other institutions. But no, this was a courtship trip. There was no, what are you going to do for us talk at all. They were wanting me to come.

15-00:54:44

McIntosh:

It's interesting how much the job market seems to have changed.

15-00:54:48

Smelser:

That was right at the moment of the shift. Not that some faculty members didn't get such attractive offers afterwards, but the whole tone of the market took a shift in the first few years of the seventies.

15-00:55:01

Rubens:

All right, should we change the tape.

15-00:55:04

McIntosh: Yes, unless you have any concluding thoughts about—

15-00:55:07

Smelser: No. I think we've spent a lot of time on these issues, which I had not intended. I think we could probably start on going abroad.

15-00:55:15

Rubens: Just to clarify, when you received the university professorship, this was the first time a non-scientist has received it?

15-00:55:24

Smelser: Yes. What happened is the university did break that line, in response to my raising the issue of appointing someone who wasn't in the physical sciences to the university professorship. They did it in a way that didn't constitute a simple, personal capitulation to my interest in the position. They appointed Josephine Miles and Sherwood Washburn. Josephine Miles was a most eminent English professor, and Sherry Washburn was an outstanding physical anthropologist. The three of us came in at that time, and that broke the barrier and made it a much more heterogeneous body. We'd always have one dinner a year.

15-00:56:13

Rubens: Had you known Miles and Washburn.

15-00:56:18

Smelser: When I was teaching my undergraduate theory course, I got my students to enter a special program the campus was sponsoring at the time, in prose improvement. Helping the TAs improve their own language, and bringing language and style into the judgment of the students' work. Josephine Miles was the head of that program. That linkage was forged in that time. Washburn I just happened to know because of my hobnobbing with anthropologists from time to time. I knew him and liked him, but not as well as I did Jo Miles.

15-00:56:48

Rubens: These people were pretty senior to you.

15-00:56:50

Smelser: Oh, yes. Both of them were near retirement.

[Begin Audio File 16]

16-00:00:01

Rubens: You mentioned that you talked about the issue of corruption at Harvard. You said this was one of the themes of your research.

16-00:00:14

Smelser: Yes. I guess it was in 1969 that I got this approach from Alex Inkeles, who was one of my mentors at Harvard, who put together a festschrift for a book on Parsons on the occasion of his retirement. The theme of that festschrift was

stability and change in society. So they approached me and asked me if I wouldn't be a contributor. They said I could choose my topic, but it had to be within this frame. Now, I had done a bit of fieldwork I described when I was in graduate school, in a small town in Massachusetts. It was my fieldwork requirement. I had to go out like an anthropologist and spend several months in the field. So I had gone to this little town in Massachusetts, which had just voted to go dry, to just ban liquor sales in the community. The whole place blew up, because a lot of the revenue came from a beach resort that served a lot of alcohol and so on. Samuel Stouffer, my mentor in this research, sent me out there to do a field study of the conflict in that community. He had some money for studying community conflict. We made a deal that I would go and one of my community studies was going to be in this little town that I called Beachtown.

I found in that town a tremendous amount of corruption between the city part of the town and the beach part of the town. It got into the police force and got into the regulation of the bars. A lot of scandals had opened this up. I was in the middle of a little town that had become really heavily involved in corrupt politics, and it had to become a big part of my analysis. That was the initial birth of my interest in corruption. I even wrote a chapter on the Beachtown study in my text on social change that I edited in 1967, as an introductory case study of how change takes place in the community. So it was on my mind, and I had talked about it in an illustrative way in my theory course from time to time, when I was talking about a little model of political behavior that I developed to introduce to students as to what a simple theory might look like. I introduced corruption as an issue. So it was kind of on my mind.

I was pondering this, should I write a kind of same old story for Parson's festschrift or should I try to do something a little different? So I picked up this theme of corruption. I got into the literature of the time, deeply, and tied up the whole issue of what is corrupt according to the cultural milieu in countries which have not yet developed. What happens when you begin to get a public sense developing during processes of development? What kinds of corruption are there? What are the tradeoffs that go into the corruption process? Why does it become a social problem associated with development? So I wrote a theoretical essay on it, which was rather different from the stuff I found in political science and anthropology, which tended to be case studies and much more practically oriented. It became one of the first few really theoretical articles as to where corruption might fit into larger processes of social change. It's remained an interest on my own part. In a chapter of a very recent book I wrote with John Reed, the banker, on usable social science, I included a big section on corruptions and informal markets in the chapter on sanctions. So it's woven in and out of my interest from time to time. This article actually received quite a bit of attention at the time it came out, and subsequently I spoke at a meeting of the International Sociological Association on the topic. It's a thread in my own work that crystallized in this work.

16-00:04:14
McIntosh:

Please correct me if you feel I'm mistaken, but I see it as a thread that began with *Sociology of Economic Life*, if not even earlier, and goes on to your studies of love and friendship and ideas like that, of looking at developing a theory that accommodates informal relationships and informal behaviors to explain economic development.

16-00:04:43
Smelser:

Very much so. Again, to return to that most recent book on applications of social science knowledge in organizational settings, if anything it is the common theme, it's the importance of informal ties, informal relationships that cut across the formal structures that many people have studied. You're absolutely right. It's been a constant theme. Corruption is an example of more intimate social processes that I try to tie into the larger social media and demonstrate its theoretical independence and theoretical importance. It was also, I have to confess, a topic that Parsons wouldn't have chosen. So there was this element, this mischievous element, I would have to say, on my part, of, again, doing something I thought was exciting—and it was couched in the framework of a lot of Parson's theoretical writings. It was an essay honoring him, so I included a lot of materials from his own theoretical formulations, but I took it in a direction that he never would have taken it.

16-00:05:57
McIntosh:

Why wouldn't he have taken that direction?

16-00:06:00
Smelser:

Well, he just wasn't interested in that seamier side of life. He had a rather benign view of society. I took it on its own grounds. Certainly I didn't take a blaming attitude toward the phenomenon of corruption. I wanted to understand it. It's sort of this idea that he kind of avoided conflict. He avoided the malintegration of society and so on. This was a way, in retrospect, of saying, okay, I'm going to honor this theory, but I'm going to take it where I want to take it, not just apply it in a way that Parsons might have. There's always that tension that I mentioned earlier with this mighty man. One was always, in some degree, in his shadow. I just mention that as yet another ingredient in my choice of that topic.

16-00:06:54
McIntosh:

It strikes me, with our perspective now, that it's not just Parsons that you're going against the grain with. This is a period in which people are trying to devise formal sets of rules that will explain behavior. All the sudden, you're coming in with concepts of friendship and corruption and partiality that are sort of—

16-00:07:23
Smelser:

Well, yes. Yes. Certainly my own work, even though I would heavily rely on empirical data and sometimes quantitative data, I really was declaring a little bit of independence from that super-scientific approach to, say, comparative

studies as well. You're right. I would confess full guilt in attending to the less formal side of social life.

16-00:07:52

McIntosh: No guilt necessary.

16-00:07:55

Rubens: What about Parsons' response? Did he specifically say anything about it?

16-00:07:58

Smelser: I got no feedback from Parsons. I think he was very glad that I joined the effort to honor him. It was a festschrift. I didn't get any intellectual feedback from Parsons on it. I got a lot of intellectual feedback from the students at Harvard who were in a heavily radical phase at that point. They would try to get me to confess that American society was corrupt when I spoke about corruption, which was far from my intent. They had their own agenda. It was antiwar. The corruption of the establishment. The usual thing that the late sixties and early seventies were all about. They were in the middle of it. This was Harvard SDS stuff. The sociologists were in the thick of that. They turned the discussion period into a kind of battlefield, actually, which made no difference one way or the other as far as my visit was concerned, but that was interesting.

16-00:08:56

Rubens: Okay, should we move on, then, to the year abroad?

16-00:09:01

Smelser: Okay, we'll do that. Of course, I had spent, myself, three years, mostly in England, but in Europe, prior to this time. Commitment to go back was already there. It also proved to be a very happy circumstance in our courtship. As I said, I had promised Sharin "On our next sabbatical, we'll go to Europe." I think that pleased Sharin, and was a good thing. It was a completely free year. It was not a year in which I had any commitments to any research institute or university abroad. I wanted to do my own work.

16-00:09:47

Rubens: You wrote all year.

16-00:09:48

Smelser: I wrote all year. I wrote a book. Wrote and revised another book. I had an intellectual agenda, but I didn't have a visiting professorship. I had a token relationship with the London School of Economics, mainly to use their library, but I didn't have the academic duties associated with that. So I worked out a plan that the first stop had to be in the U.K., because I had to have access to the British Museum and to the LSE libraries to prepare my own work on comparative studies, because I was doing a kind of encyclopedic coverage of literature and comparative economics and anthropology and in comparative politics. I wanted to draw from all these fields, even comparative psychology, and I of course had totally and completely ample resources in these

institutions. I said it was necessary to spend a lot of time in the U.K. The initial commitment was four months of hard work in London.

We went and lived in Islington with the family. A really joyous kind of time in family life as well. We remember taking our little children to the Little Angel puppet theater all the time, and around, and they got engaged with their own little friends around the neighborhood where we lived. Joe went into kind of a preschool setting when we were there. It was an absolutely glorious period. It's not that far from Central London. It was one of those areas of London undergoing some gentrification at the time. We had a very nice apartment of a faculty member at LSE who was on leave. We stayed there for four months. Then the rest was open.

We bought a camper, a little Bedford camper that was just the right size for our little family. We decided to camp for a month and search for someplace to stay for another several months on the continent, without having made up our minds in advance. We camped in Paris. We camped in the south of France, in the Provence area. We went into Spain, which was a disaster because we were robbed twice—the camper was robbed twice. We retreated from Spain and began wandering along the French coast. We discovered a villa in a little town, Cagnes-sur-Mer, which is between Nice and Cannes, on the French Riviera, that was called Ma Vie (my life). We rented it from the local proprietor, who happened to be a baker, boulanger, in Nice. He was our landlord. We said, we'll stay here, so we lived there for four months, where we did what we wanted, and I began writing, mostly the revision of my *Sociology of Economic Life*, on which I'd done also additional work in the London libraries. We lived there for four months. We had a routine where we found a French babysitter by the name of Jacqueline. We would go out a couple of times on the weekend and mainly eat in different places in the south of France. A lot of adventures. We just got to know that whole region. We would take a lot of side trips and so on. It was also quite a glorious time. It was in the winter, but the weather was benign.

16-00:13:35

Rubens:

You knew French?

16-00:13:38

Smelser:

Yes, I knew enough French so that there was no problem about that. As a matter of fact, Joe went to a Montessori school for a while. We tried to get Sarah to do something, but she rebelled. She just wasn't really interested in all this foreign language. She was just solidifying her English, so she was really angry with the imposition of a foreign language. Except at the very end, she began articulating a little bit, but then we left.

In the meantime, we had developed a relationship with an Italian scholar named Martinelli, who translated *Economy and Society* into Italian. He actually came to Berkley and took a Ph.D. under my direction, and he was a

faculty member at the University of Milan. So we organized an arrangement whereby he would live in our house for a couple of months when we were away, and we would live in a country villa that he had on the Lago di Garda, one of the northern Italian lakes, for two months in the spring. We lived four months in Cagnes-sur-Mer, then went to northern Italy, and inflicted yet another language on Sarah. She was just beginning to get the idea that French might be a possibility, and then here comes Italian. But we lived in this extremely nice villa, very close to the lake. Just about a hundred-yard walk to the lake. It was elegant. Martinelli's family apparently had money and he had inherited this place. It was a very wonderful place to live. Then we carried out our little life there, mostly around the lake, going to villages, going out to eat. In the meantime, I was beginning the writing on my comparative methods book.

In Spain, one of the events we had was that thieves broke into our car and stole a great many of my books and notes on Weber and Durkheim, so they were gone. What I did was I wrote back to graduate students to whom I'd lectured on these topics, asked them to send me their notes, which they all did, agreeably, and then I took a one-day trip to London and bought the books to replace the books that had been stolen. I landed on my feet rather quickly from that, but that was, as I say, one of the discouraging features of our Spanish travels. But the weather was grand. It was May. It was the end of April and May. I would usually write in the morning. We would take adventures in the afternoon.

I had intended a similar period in Vienna. Another sustained period in which I would write. That didn't quite come off. First of all, the rest of the year was filling up. I determined that, from the standpoint of my work, I had to go back to London at the end. The Vienna thing turned out to be just a couple of weeks of camping and enjoying the city and driving around Austria and Germany on the way back to London. We spent—I guess it was June and July—back in London, where I revisited the libraries and continued to write, and actually finished the book. We lived in Bloomsbury, not far from the British Museum, on that occasion. It was a dream year, in a way, being free. One might have been tempted not to finish one's work, because there were so many other attractions, but that was just a good part of it. I was able to finish both of those books and hand them to the publishers when we came back from there.

There were two involvements that I had. We went to Paris before going to London for just a few days because there was an OECD conference on higher education, to which I had been asked to contribute a paper on basic research. This was the Organization of Economic Cooperation and Development. Kind of a scholarly offspring of the NATO countries that is located in Paris that does a lot of good comparative statistical work. It's an intellectual organization. A conference was being held on higher education. I don't remember the occasion of my being invited, but it coincided with our arrival

in Europe. A paper came out on that, published in a UNESCO volume subsequently.

Then a most interesting event occurred. When we were abroad, it was a very turbulent year. '73, '74 was the OPEC crisis, and the implications of rising oil prices and shortages were all around us when that happened. Back in the United States, we were having Watergate. I'd get news on all these things, and it affected our life somewhat, especially in France and Italy, when gas prices shot up. Everything was kind of chaotic. They were having these walking Sundays in Italy, where people wouldn't drive at all. The scholars at Bologna decided to hold a kind of panic conference on what will happen to our societies if we have a permanent restriction of energy. It was, in a way, kind of inspired, because it became a topic that's been so important ever since. I have absolutely no idea how they located me, because I was in this villa in Cagnes-sur-Mer at the time. We got a visit from a neighbor who said somebody was trying to get hold of me. And I didn't even know how they knew I was there, nor how they would possibly know how to get a neighbor.

16-00:20:12

Rubens:

Well, through the professor with whom you had exchanged houses?

16-00:20:17

Smelser:

Martinelli? He may have known. It could have been that they knew that. Anyway, they wanted me to come quickly to get into this conference and give a lecture on what happens with respect to social change in society once you get a heavy constriction of energy. It was one of these impulsive Italian things. We'll have a conference. And I went there.

16-00:20:45

Rubens:

You didn't have the internet to—

16-00:20:46

Smelser:

No, it was on the phone. The guy was talking to me on the phone, and I was stumbling over to this other house, wondering why anybody would be calling me and how they could get me. But I went to Bologna for three or four days, before our stay in northern Italy, but I flew back. We drove, then, to northern Italy. There was a funny aspect of it. They got some money somewhere to pay me for going there. It wasn't a gigantic amount of money, but it was an honorarium. It was in lira. Of course, you get a lot of lira, even in a small honorarium. It was very funny. It was one of these prototypical Italian stories. They paid me the money in cash. Followed me into the men's room and gave me the money. It was all about Italian tax behavior. There was no record.

16-00:21:43

Rubens:

A small corruption.

16-00:21:46

Smelser:

In a way, it was a little bit of my corruption there. You just got away with not paying the taxes on that. It's sort of standard practice at that time.

16-00:21:58

McIntosh:

You didn't have any previous experience writing about things like natural resources and energy?

16-00:22:02

Smelser:

No. Just an exercise of imagination. I had written on social change extensively, and I would consider myself an expert in the study of social change. So I developed an idea. I said, here is a case where you have just the reverse of this phenomenon of social growth that all of us have been studying. Social expansion, social growth. What are the structural concomitants and types of phenomena—I was a kind of expert in this field because of all the work I'd done on the English Revolution. And a lot of work on development. So I posed to myself the question, how do we have to modify this model of growth if we start shrinking? Are any of the principles of growth applicable in a condition of stagnation or actual pulling back? That became the intellectual inspiration for this paper. I developed quite an extensive series of ideas of how different it is—after a period of growth, you start shrinking. Politics change. Stratification changes. The symbolization of life will change. It was a theoretical paper carrying out all the implications of the idea that the processes are different when you're expanding than when you're shrinking. It was about all I could do under the circumstances and the fact that I didn't have a lot of comparative material that I studied. It was a theoretical paper that subsequently was published, first in Italian, then in English. That was the way I confronted that surprise invitation. I quite wanted to do it, even though I thought it was slightly insane to call a conference three days after the Arab countries had declared this crisis. Anyway, it was a nice adventure.

16-00:24:09

McIntosh:

The prototypical picture for the U.S. during that time is gas lines around the block. Was it the same situation in Europe?

16-00:24:19

Smelser:

Yes. The shortages and inflation. That was what OPEC was primarily about. Once you get these gas prices going up, everything goes up, the way it is now. These gas prices will influence everything. The price of food, the price of everything, will be influenced. I was especially interested in patterns of group conflict that develop also under conditions of retrenchment. I had done a lot of observation on what happens actually in our own university when you get booming periods and budgetary largess, and then suddenly you get a dip downward. I had done a lot of informal observation of how groups behave differently and how the social psychology of the institution changes. I just brought all of these things to bear on this essay on energy constriction. I subsequently expanded the idea in a conference on economic sociology that was held in Holland.

16-00:25:36

McIntosh:

At the end of this year, did you have a desire to return to Berkeley or were you dreading it?

16-00:25:41

Smelser:

Oh, I had agreed to be chair. The agreement for me to become chair immediately on my return had already been fashioned. The dean of the college was desperately searching for somebody to replace Charles Glock, whose term was finished. I had been asked to be chairman a couple of times before and said no, and I sort of had determined that now that I was university professor, I wasn't going to be chairman. That it wasn't appropriate to the position. That would be part of my declaration of independence. Heavy pressure came on me to agree to be the chair.

16-00:26:24

Rubens:

Pressure from the dean?

16-00:26:25

Smelser:

From the dean, right. Was it Rod Park? I think it was Park. He was certainly my dean when I was in the chairmanship. I believe he was the one who hammered out this deal, that Leo Lowenthal, who was a very senior faculty member—had been in the faculty since the fifties—would be a chair for one year. That would be the year of our absence. Then I would come back. I agreed not to a full term, but only to two years. Then John Clausen, a colleague, social psychologist, in the department, would take over for a full term. This satisfied the administration. I did capitulate to it, with still the feeling that it wasn't my way of life to be chair of the department. Even before I was in it, I regarded it as a somewhat unrewarding assignment. But I agreed. The big decision to stay at Berkeley had been made in 1970, '71. I saw it kind of as a permanent commitment in my life. You're always saddened to see such a beautiful year end, but I was fully prepared and came back into the chairmanship, headlong.

16-00:27:54

Rubens:

So how are we doing on time?

16-00:28:03

Smelser:

We can push on for a little while longer.

16-00:28:04

Rubens:

For the first year that you're chair, at least.

16-00:28:05

Smelser:

Okay.

16-00:28:06

Rubens:

The years can vary. How long is a full term?

16-00:28:11

Smelser:

Three. Renewable. That's usually the case. But the typical appointment is three years.

16-00:28:18

Rubens:

And it is the dean that appoints that? I thought the faculty voted.

16-00:28:21

Smelser:

Well, the faculty is consulted, and it was then decided by the dean. The faculty was very happy with my becoming chair. The dean makes the ultimate appointment. There are many, many variations, nationally. Sometimes the faculty has full power to choose their own dean. Sometimes they have very little power. It varies a lot. But officially, it's the subordinate to the dean of the College of Letters and Sciences. I knew that was going to happen. I was also aware that the department was already in a bit of shambles from the standpoint of conflict.

16-00:29:02

Rubens:

How do you lay out the lines of that conflict?

16-00:29:05

Smelser:

The lines of conflict were laid down beginning in the student protest era, with a certain percentage of the faculty getting quite radically identified with the students' aims, with respect to the free speech on campus. This spread out into general appreciation of student demands and participation and so on, so that the movement to have graduate students participate in departmental affairs was already part of the division in the department, with half of the department feeling they wanted none of it. Some people were quite militant that we should have kind of full equality with the students, graduate students in particular, in all the committees and so on. That was another line of division.

16-00:29:54

Rubens:

How big was the department about then?

16-00:29:56

Smelser:

About thirty.

16-00:29:57

Rubens:

Divided in half?

16-00:30:00

Smelser:

Well, no. There was middle. I was in the middle. There was a left and a middle and a right, and the middle tended to be shrinking all the time because mostly these well-established senior professors, like Davis and Peterson and Swanson, all were extremely reactive against any changes in the academic side. That group on the left, which I said included Matza and Kornhauser and Blauner, and to some degree, Duster, and Arlie Hochschild sort of joined it, though she was not as fully committed. I would locate her out in that side. Wilensky was sort of on the right. I was in the middle. Bellah was in the middle. Glock was in the middle. There was some shifting of boundaries, but this was pretty much how it was laid out. As a middle character, I was not found objectionable by either side, though I did a big dance between the two during the course of my chairmanship. The other way it spilled over was the degree to which we were going to give graduate students autonomy in choosing what they wanted to do in required courses, and it spilled over into the whole definition of the field as being scientific or humanistic, or

politically engaged or not politically engaged. Everything spilled in, and people lined up on all these issues. That was what you call polarization. On many issues, they all could give a kind of predictable stance. So it was a package of issues that divided the department, and of course the place where this sort of takes its greatest toll is in new appointments. We'll come to that in discussing the second year of my term.

The big issue in the first year, the one that I had to do a big dance on during the first year, was that I was under pressure from the central administration, the dean's office, to tighten up the requirements, which had been more or less dismantled during the years of activism. Let the students take what they want. There was a general dismantling of requirements. This was a period of a bit of a backlash, so there was a pressure on a number of departments, not just ours, to beef up the major. To get it more intellectually rigorous, to make it tougher, and to require courses rather than just lay out a bunch of them for people to decide what they wanted to take. That pressure came directly from the dean's office. I shared that viewpoint, that things had started to fall apart and we should really take a look at the intellectual coherence of the department and the requirements that we had.

I invented an idea, among others—two ideas. One, that we would declare a certain package of courses to be core in the department to majors, thus limiting the total smorgasbord idea, and ask students to include a number of those courses in their major. But more important, and this had to do with this division between is it a scientific study or something else, to increase the demands for research methods for training—for statistics and research methods in the undergraduate major. All we had was a statistics requirement for upper division. I got it in my own mind we would make a lower division methods requirement in the department. I already had my idea of what it should be. It would be called Evaluation of Evidence. Would not require statistics, but it would take a variety of styles of research and raise the question of exactly what inferences could you draw on the basis of the design, the measures, the organization of arguments, and so on, for freshman and sophomores, a real evaluation of evidence. I wanted this course. I thought it was a productive response to the kind of administrative pressure that was coming. But it opened up this fissure in the department—left, right. Why are we requiring methods of the undergraduates? Then, on the other hand, it was welcomed by the right. In fact, some of them said, no, it should be tougher than I envisioned, in terms of quantitative ingredients and demandingness on the students. You saw the dynamics begin to unfold in connection with this course. I was under pressure from the dean's office and under two pressures in the department and I had my own ideas. All of those were floating around in that first year. We had a lot of debates, meetings, discussions about the philosophy of the social sciences. You just can't imagine. That's when I came up with the idea that nothing happens on the campus without going back to square one. No matter what the issue, you go back to the fundamentals.

16-00:35:24

Rubens:

Were you “negotiating” only with the department?

16-00:35:28

Smelser:

The department. I dealt directly with the department. I actually deliberately used a strategy that is not particularly original, but I decided it was one that I’d use. I said, look, we’re under real pressure from the dean’s office. It doesn’t take a genius to figure out that if you don’t cooperate with the dean’s office in some of these matters, it’s going to have some reflection on the kind of support and regard in which the department is held. Furthermore, if we don’t do something, we may be forced to do something. Putting the onus on the administration. Using it, in a way, as a kind of threat to my colleagues. I used that argument frequently, probably cynically, because it probably wouldn’t have happened the way I envisioned, but I did use it as an argument.

In the end, we fashioned a course called Evaluation of Evidence. I agreed I would teach it the first two years. We also agreed that anybody in the department could teach it, because it would be not technical, and it would be a general commitment of the department as a whole to this course and making it work, and it would be required of first- or second-year students who were going to major in sociology. It turned out that people took it later. It didn’t work out so neatly. It’s not the biggest issue in the world, but I played an entrepreneurial role in kind of finding my way through these—didn’t make it this heavy-handed statistical course. Made it required. Made it somewhat rigorous in terms that it was a methodological course, not just substance. The dean’s office was happy. I don’t know that the dean’s office was monitoring us in every detail. They just wanted some evidence that we were going along with this movement to beef up the undergraduate curriculum, which was in the air in the mid-seventies as a kind of, you might say, backlash or a swing-back of the extreme liberalism of the decade before. That actually took a lot of my time.

There were other issues. I guess I should tell my story about Arlie Hochschild at this time. She was made an assistant professor, just after she got her degree, and she came up for tenure during my chairmanship. One of the peculiar circumstances of her situation was that, midway in her assistant professorship, she had decided to go onto a half-time appointment. This was possible because her husband, who had fallen heir to a lot of money—they were independently wealthy from an inheritance on his part—so she did not need to teach fulltime. She sort of went the half-time route, but out of her own voluntary—

16-00:38:45

Rubens:

Probably because she wanted to have a family.

16-00:38:47

Smelser:

Yes, she was having a family. She wanted time to be with her family. It all made sense. I was responsible for putting forward her recommendation for

promotion. The department supported it, so I didn't have a fight within the department. The case wasn't 100% sealed shut. There might have been some questions raised in the review process. I knew that. I talked with Arlie a lot. Then I went on a campaign with the administration and raised the fundamental issue of what the expectations were of a half-time appointee. Because of course you teach half-time if you're a half-time appointee, but the general culture was you treat them just like everybody else with regard to research, even though they're getting half their salary and are half-time committed.

16-00:39:40
Rubens:

And service to the university as well, right?

16-00:39:44
Smelser:

People don't know what half-time is, just like they don't know what a joint appointment is. They treat you as though you're theirs. This was happening to Arlie in terms of the kinds of judgments that were going to be made of her productivity and everything. I brought it into the open. It was sort of a women's issue in a way. I got very aggressive in my own sponsorship of her case. To take account of this ticking of the clock and what kind of clock it was that was ticking. I just wouldn't let them forget about it. In the end, she got promoted. There was an outcome. I think that it was a positive role that I played in this regard, because the academic mentality is such a mindset that she'd be like any other candidate, just coming up. They just ask precisely the same evaluation questions. What's this? Only one this, only two of that, and so on. The usual questions. I wanted to make sure this got into the open and became a matter of concern. She wasn't sent back for more material or anything. It went through, but it was a guided thing, in a certain way, on my part, to make explicit this particular feature of that case. Of course, it raised the larger question of exactly how are you going to deal with an increasingly diversified faculty.

16-00:41:16
Rubens:

Now the women's movement and the women's studies program on campus were in full swing. There were issues of women not being promoted to full tenure that were litigated.

16-00:41:23
Smelser:

Well, the women's movement was in full swing. The other thing that I will talk about next time is the impact of the affirmative action movement on our own recruitment efforts in the department. The women's movement was very, very strong. We had appointed a few women. Gertrude Jaeger, who is Philip Selznick's wife, had been given a permanent appointment. We were beginning to respond to the appointment of women in the department. The ferment was high. But Arlie had no interest in women's studies program.

16-00:42:02
Rubens:

She never did have an affiliation with it?

16-00:42:04

Smelser: No, no.

16-00:42:06

Rubens: I just didn't know if they were taking up this cause. Was there any reason to enlist them?

16-00:42:14

Smelser: I'm not sure how active they were on this specific issue, but they sure were active in terms of getting women represented in the faculty, and the women's studies program was one wing of that general political pressure being put on—

16-00:42:30

Rubens: There were a couple of law suits –I can't remember the specific cases right now.

16-00:42:39

Smelser: I daresay I don't remember them in detail, but I would be very surprised if there weren't at that time, because it was a period of extreme activity, and of course, the recruitment of minorities was at its peak of interest at that time, in the mid-seventies.

16-00:42:58

Rubens: I'm wondering if we should leave discussion of the second year for our next session.

16-00:43:01

Smelser: I think so. That was a bigger and even more trying year than my first.

16-00:43:09

McIntosh: I have a question about this first year, though. It builds off of something you mentioned earlier. You said that the department was sort of split on two questions that perennially came up, one being, is sociology scientific or is it humanistic, and the other being, should—

16-00:43:27

Smelser: Or activist. And/or activist.

16-00:43:32

McIntosh: So these are kind of either/or questions, but you characterize yourself as being in the middle. What does it mean to be in the middle of these either/or questions?

16-00:43:47

Smelser: You're absolutely right in the sense that it's, in a way, hard to make peace among two competing, mutually rejecting models. Being in the middle means becoming something of a catholic, with a small "c." That is to say, to in fact not insist on a specific model, one or the other. That would have been suicide. That would have deepened that split right down to the point of break. But to say, look, sociology is a field, we all know, whose evolution has been really

complex and full of controversies, and there are many styles. We want to, the best we can, substantively cover the field and represent different approaches. That was kind of the line as chairman that I took with respect to this particular conflict—whereas, on each side, there was more of a standoff mentality of what should be right and how we should implement it in terms of departmental policy, and to appointments, above all.

16-00:44:49

McIntosh:

Did you have personal opinions on these issues that you couldn't express in your role as chairman?

16-00:44:56

Smelser:

Well, it happened to be consistent with my own role as a social scientist. I have always had this catholic, interdisciplinary, encompassing attitude. I was put in the functionalist camp, was uncomfortable in it, and in the long run, the label doesn't apply. I didn't like it. I didn't like this idea of being a partisan, lance-carrying type who's going to defend his position. I think that sectarianism has been a mischievous effect in the social sciences, and I've always had a more encompassing view that happened to coincide with the role I could play, administratively, in the department.

16-00:45:40

McIntosh:

It's interesting, because that sectarianism often is a result of sort of career-building desire. I'll stake out my place in this camp. But here you are, being courted by all of these different universities. You're the chair of the department. You're a university professor. You've done that specifically by avoiding that sectarianism.

16-00:46:04

Smelser:

You could describe me as a living exception. Obviously, I was a very ambitious social scientist. I fully acknowledge that. It's a big part of my life. But I didn't follow this line of saying, this looks like the hottest thing going these days. I'm going to go down that route. Or, this seems to be dying on the vine. One example of that is Russian area studies. A lot of people made a splash in it when there was a lot of money going into it during the hot part of the Cold War, but then it began to fizzle out. A lot of the scholars said, I'm getting out of that field. Truly opportunistic moves. I've always said that interdisciplinary inquiry is for the foolish young man or the wise old man. I took the foolish young man route, in a sense, but wasn't punished for it.

16-00:47:09

McIntosh:

It seems like you're actually actively rewarded for it by a lot of foundations. It seems like you had no trouble—

16-00:47:15

Smelser:

No, I didn't suffer disadvantage from raising funds or getting research support or advancing my career in the field. It's just kind of a stereotype that you had to be that instrumental, but I think I was in a kind of small minority of sociologists who decided that this principle of catholicity and interdisciplinary

borrowing and synthesis was going to be a main theme in my life. It was very consonant with my whole personality. I don't know that I could have done it otherwise. But you're absolutely right. An atypical career pattern in academic life.

16-00:48:01

McIntosh: Very interesting. Well, we're getting up towards the end of this tape, too.

16-00:48:05

Smelser: Okay. I have to wind down a little bit. We're having a lunch.

16-00:48:09

Rubens: Were you teaching this first year as well?

16-00:48:13

Smelser: Yes. I was teaching one course.

16-00:48:17

Rubens: And seeing students at Cowell?

16-00:48:21

Smelser: I think I took leave from Cowell those years, and then joined it later. I had a crisis. Philip Selznick's wife was entering the terminal phase of her life. She had cancer at that time. He was teaching Introductory Sociology, and he came and begged me to opt out of that course. That was the first year of my chairmanship. In a matter of kind-heartedness, I said sure. But then we had to teach the course, so I taught it. I stepped in. It was a quite miserable experience, because the discontinuity in teaching and the assignments and the style and so on. That will discourage students a lot. It wasn't a happy circumstance, but it added to my burdens in that first year.

16-00:49:13

Rubens: All right. To be continued.

Interview #9 April 26, 2011

[Begin Audio File 17]

17-00:00:01

Rubens: So it's the 26th of April and we're meeting with Neil Smelser to discuss your second year as chair of the sociology department.

17-00:00:14

Smelser: Yes. I'd like to mention one decision that I had in the first year that I didn't mention before and that had to do with my dealing with several emeriti. Kingsley Davis, Herbert Blumer, Leo Lowenthal and Wolfram Eberhard. They all had retired. And we had some provision at that time to recall emeriti to teach and pay them to teach. All four of them were eager to take advantage of that but I didn't have the budget. There was a tightening of that and I only had one position but I was getting pressure from all of them to be recalled to come back. This was a pressure because they all were pretty insistent, a couple of them very insistent. So the way I had to deal with this, the only way I had to deal with it—my back was kind of against the wall, I just didn't have the resources. I said, "Okay, men, this is the way we've got to do it. You're each going to get one quarter recall and that's it." This is the only thing I can do. And they had to buy it because I was basically saying, "I'd love to bring you all back but this is—" I made up a very interesting mantra that lives to this very day in the sociology department and that is that I said, "The key to being a successful chairman is to anybody who comes to you, you say to them, 'You're absolutely right and I'm going to do everything I can to support your position, but you best understand there's almost nothing I can do.'"

17-00:02:02

McIntosh: So was this a coordinated effort on their part or were they each coming in individually?

17-00:02:06

Smelser: No, no. They were each individual. Each one of them wanted to have more.

17-00:02:14

Rubens: I don't know Eberhard. What is his field?

17-00:02:17

Smelser: Oh, Eberhard was an interesting scholar. He's not among those giants that are usually mentioned in the department at the time, largely because he was a Chinese historian who joined the sociology department. A very smart man. Wrote extremely good stuff on Chinese imperial history and he was a good citizen of the department. But he's never quite mentioned in that group that was the leading entrepreneurial cadre. Blumer, of course, was chair as it began its takeoff and Kingsley Davis was an extremely eminent scholar and leader. Well, not leader. He was a star in the department and Lowenthal was a very good senior citizen who had a role. Not as active as people like Lipset and Selznick and others, Bendix. But nonetheless, these were all people of force

and prestige, and all senior to me, of course. They had hired me and seen me up through my career up to that point.

17-00:03:37

Rubens: So did they buy your solution?

17-00:03:38

Smelser: I don't think they had any choice. They each taught a course. Well, it did a little something for them. Because I didn't have many bones in my bag, I had to give a small bone to each one of them. I don't know that I had any choice in that matter. It would have been disastrous if I favored one of them over the other in terms of resentment and potential trouble that might arise. So I thought it was kind of a King Solomon decision. Just had to sort it out. It didn't flare up at all. I dealt with each one of them individually, explained exactly what I had to do and couldn't do more and gave them a supportive message about their service to the department and the legitimacy of their desire to be continued after their official retirement. We had official retirement age at that time and they'd all reached it.

17-00:04:37

McIntosh: What was that age?

17-00:04:40

Smelser: Let's see. Seventy, I believe, at that time. Then it got uncapped later on by the federal legislation in the nineties.

17-00:04:51

McIntosh: Well, that sounds like a politically complex situation that you found yourself in. But also because they are these eminent men who raised you in the field, sort of emotionally complex, as well.

17-00:05:03

Smelser: I don't think I suffered that much emotionally. I didn't like it because I was under pressure and I had to come up with some kind of solution.

17-00:05:14

Rubens: Well, I was interested in—it's a bit unimportant for your oral history. But why were they so bent on doing this? They had reputations, books.

17-00:05:26

Smelser: They wanted the money. They wanted to supplement their retirement. They weren't poor, but on the other hand, none of them had full benefits because they hadn't been here that long.

17-00:06:12

McIntosh: So transitioning into the second year. What were some of the major issues that you confronted then?

17-00:06:19
Smelser:

In fact, the second year was dominated by a single process and that was the accidental fact that the retirement—some of these were the retirees that had put the pressure on me—left us with the opportunity to hire three new assistant professors. This was almost unheard of in that particular decade, which was the beginning of the big slowdown in growth of higher education generally around the country and certainly affected the Berkeley campus. But we were given the opportunity to hire three assistant professors and in the end it turned out to be four because we got the opportunity to have a half-time position with political science, and that was an interesting story in itself, that position. We'll get back to the others.

That position was given to us because the political science department had been particularly backward, it was perceived, in appointing women. And the administration gave them one of these targets of opportunity positions, meaning if you fill this with a woman you can have the position. If you don't, you can't. That was what target of opportunity meant. And so the administration dickered around with it with the political science department, who didn't like being shoved around in this way. And they came up with the agreement that that appointment would be joint between us and political science. So we took it. Bendix later went to political science. As did Wilensky. But those events were independent of this particular thing.

This just fell in our laps because of the particular relationship between political science and the administration. In the end we had four positions. And that was a tremendous logistic problem for us. We didn't want to say don't give us those four because we don't have the manpower to fill them, because that would have been self-defeating from the department's point of view. But we did have very, very limited resources and our own size of faculty would be strained to do all the searching. We knew there was going to be heavy demand and heavy applications for these spots. And furthermore, we had just entered that era in which there was a great deal of extra procedural work dealing with the nature of the search, the affirmative action requirements that were beginning to be put onto the departments. So we had a kind of a—almost a workload crisis just to fill these positions.

Luckily, a new administrative assistant had just joined. Her name was Robin Content. She was an extremely talented person. She'd worked at Davis for a while and she had worked with the man who later became a vice president, systemwide, and she was organizationally savvy and a person of initiative and also more knowledgeable about the computer processing of things, which just had come in, than I was. So she immediately began working with me putting—actually putting pressure on me. “We've really got to rationalize this, right, from a technical point of view to handle everything in terms of preliminary interviews over in San Francisco at the ASA meetings, setting up an actual machinery for data that could be accurately recorded for the reviewing bodies to review them.

I myself took the initiative at the faculty level to rationalize the search process for these four people in that we had a regular personnel committee in the department made up of maybe four people with a chair. It seemed to me totally unfeasible for us to ask that personnel committee to handle the whole load of these four appointments. We had up to eighty to a hundred applications for each position, you see. So the paperwork was really quite overwhelming. So I devised a proliferation of this personnel committee that involved almost everybody in the department in the review process. That is to say, for each position I took one person from the personnel committee as chair of a subcommittee and then supplemented it with several other faculty members. So each position had a search committee. But it would all go to the central personnel committee after each subcommittee made their recommendations and a central personnel committee would then present it to the department. So I actually recruited almost two-thirds of the members in the department in these searches according to their expertise, according to their interest in each of these subcommittees, but continuing to involve the central personnel committee and working myself directly all the time with the chair of the personnel committee as to what was going on at all these committees. So I went in with the chair of the personnel committee, Robert Bellah, and monitored the whole process.

And in the meantime, Robin had set up ways of entering data accumulating. Letters of recommendation. Systematizing how to make available in an efficient way to each of these subcommittees the processing of the material.

17-00:12:15

Rubens:

You also created some standardized set of questions to evaluate the candidates.

17-00:12:17

Smelser:

Yes. Standardized set of questions regarding how you evaluate letters of recommendation, how you evaluate the writers of letters of recommendation, how you evaluate the sample publication of the person. We just tried to rationalize it as much as possible. Of course, you can't guarantee that it's going to work that way because everybody is loaded with work and they'll take short cuts and they'll make their own decisions and their own processes. But nonetheless, we tried to rationalize it as much as possible. I wrote it up in the book about this plan that we had in *The Changing Academic Market*.

Now, the department at the time was in bad shape from the standpoint of political divisions. We had inherited the entire—it was several years since the Vietnamese War had wound down, but nonetheless we had inherited a whole series of cleavages, I also lay those out in that book, that traced back to beginning in '64 when people took different stands on the student activism and different stands on how aggressive the department should be with respect to war protests. Stands about requirements to be imposed on graduate students, curricular requirements. Just name it, there was a division and it they

all tended to fall down the same lines. It was a polarized departmental situation. And I knew that this was going to be the main consideration. This cleavage in the department was going to be the main dimension in appointing personnel because the people were quite militant in their ideas as to which way the department will be shaped by this significant number of new appointments. It was an addition of almost, what, 15 percent personnel. Four new people in the department is very, very significant in the longer run, even though they were all at the assistant professor level.

17-00:14:42
Rubens:

You were you trying to fill certain areas in the field of sociology?

17-00:14:48
Smelser:

Oh, yes. We described the four different areas, one being methodology. I forget the exact job descriptions of each. The one with political science, of course, had to be in political sociology and we were constrained to go along with that because it was a joint appointment. We were required, as part of the new sets of regulations associated with affirmative action to describe the kind of person we want. You just couldn't say, "Wanted, sociologist."

That was already beginning to be seen as leaving too much room for arbitrariness and you had to be straightforward. So we were under considerable administrative constraints, too, about describing the characteristics of each candidate, particularly the finalists, top ten and for each position, identifying their ethnicity and gender and for those that didn't make it to the top we had to develop a statement as to why they hadn't. These were the requirements at the time. And so it added enormously to the administrative workload of the department to take care of this. I had an ironic relationship with a couple of administrators because the requirement came down that we had to give a report on the ethnic and racial and gender background and I was interested in how we could get these data. So I came upon this demand that we describe the characteristics of the top ten or whatever it was of each position. But then from another office in the university I got the message that we couldn't ask the candidates about those categories. That was a requirement inherited, also a university regulation. So you were kind of stuck how you were going to find out. You can't superimpose your own guesses by names or that other indirect influence entirely. It's a very inaccurate way of finding out. So what we fashioned out was with this optional thing. "What's your racial or ethnic background?" Optional. Right. So that it wasn't required. We got partial information that way. Mostly white males refused to even respond to that question because of their own sense of what it was all about. It was a threatening thing from their standpoint. But nonetheless, we went through all of this.

And I comment on this in the write-up of this later on. It's that there was a great tension between the social justice aims that affirmative action was requiring at the time and the administrative burden that it was imposing on

people who were doing the work. And that I came up with the formulation that this tended to send administrators into a formulaic direction of why isn't this person in the top ten? Well, you give some reason that was about the quality of their preparation or something of that sort. And this would tend to repeat itself. You tend to repeat these acceptable reasons without going into much detail just to save your own time and commitment. And as a rule what these sorts of quasi-formulae—I didn't engage in this myself to an extreme degree, though I found myself moving in that direction. I said, "The reason this person didn't make it is they're not good enough." Right? That was what my view was on this. But you couldn't stop there, so you get to generating these formulas that you have to repeat from time to time. And, in a way, that tends to defeat the whole purpose of the affirmative action, too, if you just sort of were making this decision. And here are the reasons and accept them. Normally upper administrators will accept what you give them. So it takes on an empty or almost ritualistic—move in that direction and to some degree subverts the whole process. We were very sensitive to issues of that sort. As it turned out, we appointed two men and two women, no minorities, in that particular year. But nonetheless, we went through a very thorough procedure in meeting the requirements of the administration. This is the administrative side of it.

The political side of it came up mainly in connection with two candidates. The candidate who came in from political science was at the end because they had to go through their search and we had to go through our search. It was separated from the other three because it was a joint appointment and it required a joint effort on the part of the departments. We came across one candidate we both completely agreed upon, a woman in the study of Soviet politics that everybody liked. So in a way, that was non-controversial. There was no division in the department over this person. And then we hired Victoria Bonnell from Brandeis, a Russian historian with a sociological outlook. And there was no significant dissension on this appointment. She was well prepared. She'd done good work. She was already a faculty member at Santa Cruz, if I'm not mistaken, and we'd brought her from there.

The two candidates that caused most conflict were both graduate students who had just gotten their PhDs from the University of Chicago. One was a man named Ronald Burt, who's a very conventional quantitative sociologist. A whiz kid. Sort of a genius methodologically. It was just quite evident that he was at the top of the pile—and that was one of the job descriptions. Had to do with emphasis on methodology. At the same time it was quite clear that he was politically very mainline. Then for the other position there was a whole array of candidates, but one of them from Chicago who was also shown was Michael Burawoy, who was an open Marxist, who had done his work and was quite public about his own position. And I knew that both of those candidates were going to be contentious for different reasons because of their different specializations. I didn't manipulate it this way. I don't think I manipulated it this way. But we considered them more or less simultaneously. There was

never the idea that there was a horse trading. “We’ll appoint this person if you appoint that person.” There was kind of an undertone in the department that everybody was going to get his or her way in some respect. And that actually worked out. After considerable and heated discussion, the department voted pretty strongly for both of them. But they weren’t presented as a package. They were presented separately but they were presented close to each other in terms of sequence so that it was quite evident what was going on.

17-00:23:19

Rubens:

Meaning the counterbalance?

17-00:23:20

Smelser:

Yes. That each of the contending main factions or divisions in the department would have something, right? And as a way, that kind of turned out. As it turned out, the feelings about Burawoy on the part of a few people like Kingsley Davis on the right were so strong that they tried to block it. But they were so small in number and the general consensus of the department was that this was a good solution to a latent political problem in the department. That their stringent opposition to Burawoy and support of another candidate, a demographer, a promising woman demographer who they wanted to appoint instead of Burawoy for that position, that didn’t get anywhere. It only got two or three votes and those were from the right. And so those appointments went through but there was a lot of negotiating and a lot of sensitivity and a lot of anxiety. Talk about the personal overtones of a political conflict. That was as close as I came to losing sleep was when these battles in the department were looming. Definitely it was in my responsibility to handle these somewhat heated discussions that were going on in the department.

17-00:24:51

Rubens:

Now, did you have any personal horse in those races and did you have any role in stacking the committees?

17-00:25:03

Smelser:

No, no. The contest between these two political candidates only emerged late because we had to have that whole processing of how they rose to the top and the committees all did the—I did not intervene in these committees’ work at all. I let them come up with their own recommendations and through the personnel committee—and Bellah reported on the work of the larger process. And I didn’t intervene. I worked closely with Bellah in this process because he and I had a—we were both kind of in the middle and we had a good collegial relationship. We’d both been at Harvard. We knew each other at Harvard. I had been active in getting him to come here a few years after I came here. Supported him strongly. And so it was a pretty good working relationship. It later fell apart but that was a good relationship at this time. And I was in constant contact with Bellah. But I didn’t go around picking off individual faculty members and trying to influence them to vote this way or that way. I handled it more at the collective level at the department meetings and they went through.

In a sense, the search was a success. We filled all the positions. All the people that were in there we agreed were top flight from the standpoint of their qualifications and we got them through. Some might say that in a way political issues played a role. They did play a role, as they do in appointments in many departments. But it wasn't strictly speaking a political compromise. We didn't take anybody that was thought to be inferior as a scholar and so you have to say it was a successful search in that we went through the required procedures administratively and as exhaustively as we could possibly arrange it in terms of the evaluations of these people and then delicately maneuvering through some of the more sensitive political issues as the process began to come to a close.

17-00:27:15
Rubens:

I hope this is not too banal a question. But I can understand how a person on the left would be measured. You're an outright Marxist, you can see their politics. How about the more mainline? What were you looking at that—

17-00:27:28
Smelser:

We were not looking at their party affiliation. Well, the main line would be that their commitment to sort of positivistic sociology, commitment toward neutrality and research, not being bitten by the most sensitive social issues of the time. This man Ronald Burt was interested in corporate decision making as a process. He certainly wasn't challenging any establishment. He wasn't bitten with a sense of social injustice. He was just the kind of objective, positivistic sociologist who believed that the best way to advance the field was to conduct good scientific research. Now, to the left that wasn't acceptable. To the left, that's right. So that was the feature of this candidate. He was too square. He was too establishment in his outlook. And he was. And he didn't talk rightwing politics. But given the character of the field and the mixture of both political—he didn't have any echoing feelings about the war or anything of that sort. He was just neutral in this regard. But given the political spectrum in the field at the time, that would be considered to the right.

17-00:28:56
McIntosh:

Many intellectual historians of the 1970s look at the University of Chicago as being a particularly important place. The economics department, obviously. But that's where this model of rational choice that becomes sort of repopularized in the 19—

17-00:29:15
Smelser:

Yes, Burt was closer to that.

17-00:29:16
McIntosh:

So were you all aware of that shift that was taking place?

17-00:29:22
Smelser:

No, no. I was certainly aware of the ideological panoply of the University of Chicago. In fact, I'd been back there a few years before when they were courting me. I got a good picture of the thing. Chicago was sort of an accident.

It very likely would have been Michigan or Harvard or other leading—North Carolina—leading departments in the country. And the fact that those two came from Chicago seemed to be sort of an accident. And we didn't make anything in particular of the fact that they both came from there. We saw them more as individuals who had chosen their own particular route of preparation.

17-00:30:09

McIntosh: And so just to clarify. It sounds like your role in this process was to set up the procedure.

17-00:30:15

Smelser: Yes.

17-00:30:15

McIntosh: And then to work closely with Bellah to make sure the procedure is being followed basically.

17-00:30:20

Smelser: If you're going to be a chairman worth anything, you have to step back from your own personal preferences. In other words, I don't think it would have done for me to come in there and push a candidate aggressively. As chairman you've got to sort of focus more on the procedures. You've got to focus more on getting some kind of consensus or quasi-consensus in the department. In other words, you become committed. If you don't, the thing's going to fail. If you become one of the battlers in it, you just have stepped out of that role. I believe, fairly self-consciously, I saw myself as constrained more by the organization and the process at the time rather than my own particular preferences as far as the fields were concerned. I was satisfied with the academic quality of all the candidates that we hired so that minimum was established already. Then it was a matter of engineering a process in a very difficult political situation. That was a top priority for me.

17-00:31:25

McIntosh: That's just such a different way of hiring, basically, than took place two decades earlier or even a decade before. That sort of old boy network.

17-00:31:37

Smelser: Well, yes. The point is we did do one thing in the process. We, of course, advertised and everybody who responded to the ads we put in the pool. So that was a universalistic maxim and there was some question that we might have advertised further. We did do the *Chronicle of Higher Education*; we did the official publication of the American Sociological Association. We advertised in those. And the question always remains in any of these searches, and this came up in other searches I was in, do you want to advertise any further? How aggressive are you going to get? Are you going to advertise through deans of four year lesser institutions? How widely are you going to search? So we did that. And we processed as many sources as all. In addition, however, I wrote to some twenty or thirty colleagues around the country who obviously would be selective from the standpoint of our needs. I asked them to supply names

into the process. So we supplemented it. There was a little bit of the old boy there. Not that we gave special privilege to the people who appeared by these kinds of personal contacts. But because I had a selective network they tended to show up higher in the rankings than the John Doe who read an ad and sent in his stuff and hoped for the best.

17-00:33:20

Rubens:

Well, in *The Changing Academic Market*, you say—this is chapter seven when you're analyzing the pool—that you are asking people to look at the prestige of the institutions they come from.

17-00:33:32

Smelser:

Yes.

17-00:33:32

Rubens:

You're interested in the role of their collegial networks? And so all of that had to—was part—

17-00:33:37

Smelser:

Well, it was entered in. It was a mixed case. It wasn't a strict transition from the model old boy pattern. When I was hired, it was strictly old boy. Lipset talked to Parsons and Parsons said I was the best person. That was the way the thing did operate. And Caplow and McGee, who wrote up that particular system most vividly, had it more or less right. I think they stereotyped to some degree. But we didn't just forsake personal contacts altogether in this. And, in fact, our choices were from, let's see, two from Chicago, Gail Lapidus, who had been in political science—she was from a major institution maybe Stanford. I'm not sure. Vicky Bonnell who got her PhD at Brandeis and studied a lot with Harvard faculty when she was at Brandeis. So it was kind of an establishment series of institutions from which we got our candidates, our ultimate appointees. But on the other hand, there ought not to be a surprise about that because they're selective all the way up into the ranks of these. I wrote up the whole issue of selectivity and differential socialization and differential recruitment and how the pool successively narrows as people go on. So we fought that battle and, of course, we were in a transitional period at that time because the requirements of affirmative action and extensive search had just come into place.

17-00:35:17

McIntosh:

Well, there's one more intriguing element which is that you mentioned the requirements of affirmative action but then also mentioned that no minorities ended up being selected. When the final decisions were made, two white men and two white women, did you have to—

17-00:35:36

Smelser:

No one in the department raised a fuss.

17-00:35:58

McIntosh:

You didn't have to answer to that to anybody?

17-00:35:40

Smelser:

No, no. Well, because we actually had invented and carried out an extremely thorough procedure. We were invulnerable on procedural grounds.

17-00:35:50

Rubens:

Troy Duster was already there.

17-00:35:53

Smelser:

He was there. He didn't form a group or get his colleagues to push hard for minority appointment. There were in fact—

17-00:36:08

Rubens:

You had three. I think then you already had three African American males in the department, and one Mexican American.

17-00:36:19

Smelser:

Yes.

17-00:36:20

Rubens:

So you were not so vulnerable to—

17-00:36:24

Smelser:

They were more junior. They were more recently on the scene. Interestingly, the race and ethnic dimension did not play much of a role. We didn't have very strong minority candidates to appoint.

17-00:36:36

Rubens:

But you already had some representation.

17-00:36:37

Smelser:

We had already begun both the appointment of minorities and women to the department. But in this particular fight, it was really more the left/right dimensions of a sociological profession at the time as they were manifesting in extreme form in our own department that were the main political points of tension. If there had been a really strong minority candidate and had come up in heavy competition with one or more of the four that we appointed, then I imagine it would have surfaced. As it turned out, it was a weak field from the standpoint of minority candidates.

Then Robin Content was very much interested in having this written up as a case study. And I was sympathetic to this. She was not a scholar herself. She had been mainly in mid-level administration in different academic settings. And I did not want any write-up to be simply a matter of a description of our own case. And I took the lead in writing it. She didn't write too much. She gave me tons of data. She organized things. She obviously made critiques of what I had written and so on. And besides, a large part of the writing took place in England after I had already left the campus. So I was in communication with her and showed her all the stuff but I basically did it. I wanted to make it a general analysis of the changing academic market and half of the book is on that. It's an academic book about the academic market

and then the second half was a way of putting flesh on this to describe how we maneuvered in this particular case study to come to terms with these changing forces in the market. So I pushed it in a scholarly direction, if you want to put it that way.

We did have a little trouble getting it published. I sent it to two or three publishers, I forget who they were. They weren't interested in Berkeley in particular for quite obvious reasons. So after two or three rejections I sent it to UC Press. So I thought, "That's a much more natural place for it to be." It was my first publication with UC Press and they took it. And I think they should have published it. It was about our own institutions.

17-00:39:25

Rubens:

Were you thinking that another press would have more prestige or be distinct from Berkeley?

17-00:39:31

Smelser:

I may have sent it to Prentice Hall or my own press. The University of California Press is a high-prestige press. It's almost invariably grouped in the top four or five in the country. So there's no problem about its own prestige. Of course, at that time there were still books being published by commercial publishers of an academic nature. And, of course, they had a much larger apparatus for promoting and selling these books. But this was one which didn't ring a bell with them from the very beginning. There was no particular problem in getting UC Press to do it.

17-00:40:16

McIntosh:

Was this a project that you found as stimulating as some of your other more personal studies?

17-00:40:25

Smelser:

I had a lot of libido in this study. I felt it was a valuable thing to write up and I also had a sense that it would contribute something to the understanding of the issues of the time that other kinds of books wouldn't. So I don't put it in the category of some of the deep scholarly work that I've done. It really had a much more applied and contemporary significance. It's been cited and looked at for quite a long time afterwards. It has justified that, I think. So I guess that's about the best answer I can give to your question.

17-00:41:14

McIntosh:

That's a fine answer.

17-00:41:16

Rubens:

And it did have a bit of a splash. I was reading Nathan Glazer's review of it.

17-00:41:23

Smelser:

Oh, I don't remember that. He had left.

17-00:41:27

Rubens:

Oh, he liked it. Yes, he thought it was—

17-00:41:34
Smelser:

Glazer would have been on the right. He was very impatient with the excesses of affirmative action and I think he probably saw my work in a—he and I always had a very respectful relationship with each other. We were friends when he was here and kept in contact after we went to Harvard. He was to the right of me as far as this was concerned but I think he had an admiration of the way that I dealt with the ticklish situation, both administratively and substantively, in the department.

17-00:42:16
McIntosh:

Since we have diverged a little bit from your administrative role into your intellectual life, on your CV here it says that you got a grant from the Russell Sage Foundation right about at the same time of your second year.

17-00:42:30
Smelser:

Well, let me get into that. I think I've covered the administrative side of that second year. I had all kinds of other things to do, of course. Naturally, the usual routines of the chairmanship but I don't think those sorts of things make very much history so I don't think I need to go into some of the personnel decisions that we had.

The grant from the Russell Sage Foundation was intimately tied up with my acquaintance and friendship that developed with Erik Erikson and here's how it went. Erikson was a professor at Harvard when he retired, along with David Riesman and a number of other notables that they appointed to give general courses at Harvard. He also had had a history in California. He had been at the San Francisco Psychoanalytic Institute and with the Institute of Human Development here on the Berkeley campus and wrote a good many of his early empirical studies out of the California setting, using the data from that institute. And so he always had a kind of home and he had many friends in the San Francisco area because he practiced here a long time as well.

So right after he retired, this would be in '73, roughly, early seventies, he moved to Tiburon for his retirement with his wife. I didn't know Erikson at that time. I obviously knew his work. I knew *Childhood and Society* and I knew other work and I was impressed with the man. And in the Psychoanalytic Institute we talked about Erikson quite a lot because he was one of the leading intellectuals of the day in the psychoanalytic world.

So I was no stranger to him. Bob Wallerstein, who was a colleague of mine from San Francisco Medical School, a psychoanalyst in the Psychoanalytic Institute, set up a seminar about that time in which he invited, you might say, more intellectually inclined psychoanalysts. Not those who were just simply more deeply involved in practice but a more reflective subgroup of the psychoanalytic society. I mentioned this in an earlier interview. I had collaborated with Wallerstein on an article called "Psychoanalysis and Sociology" a few years before and we had developed a friendship. So he

invited me to be one of the members of this group of maybe fifteen people. He also invited Erikson, with whom he was a friend.

So Erikson and I came together in this group which met in the evening in San Francisco for a year or two or however long it lasted and we discussed various intellectual topics that people would choose or their own writings. It was one of these intellectual discussion groups. Erikson and I kind of began to make music together in this group. We each noticed what the other were saying. We took note of that and began to have an interest in each other outside the confines of this group. And that friendship developed and he and I then began to schedule periodic lunches together in the Berkeley Faculty Club. And these would last a long time. These would like go on for two and a half or more hours and we'd walk around afterwards talking. It was one of these really, really nice relationships that began to develop. I'd known his son Kai before. Kai was just about my age. I think maybe I've talked about this before.

That was a link. And then I think on top of that my father had just died a few years before. Erikson was a beautiful father. He was about the age of my father. And then the conversations became personal. I learned a lot about Erikson's own vulnerabilities and sensitivities. He was able to open up about his own ambivalence about his own Jewishness, for example. He would talk to me frankly about this and talk sometimes in an embittered way about attacks on him. It was an interesting interchange, I have to say. One day we were talking at lunch and I mentioned my birthplace was Kahoka, Missouri and I noticed a little registering in Erikson's face but he didn't say anything. And then in one of our walks around the faculty glade, and just after that lunch, he said to me, in a diplomatic way, he said, "You were born in Kahoka, Missouri?" He said, "Are you Jewish?" I said, "No, my background is completely German." He said, "I thought so." But it had been kind of preying on his mind. That was at the top of his consciousness as to what—

17-00:47:52

Rubens:

I think I had asked with you off camera about the origin of the name Smelser.

17-00:47:59

Smelser:

It's German. It means ironmonger. Schmeltzer.

17-00:48:01

Rubens:

So by the name he could have—

17-00:48:04

Smelser:

Oh, the name. A lot of Jews picked up German names. There's some ambiguity there as to what it might connote. Spelling got simplified in the course of, what, Ellis Island or whatever administrator—someone spelled it that way. And the American spelling of Smelser isn't all that uncommon. The C-H's and the T-Z's all dropped out and became more simplified. Anyway, Erikson was very conscious about—

Then Erikson began to say, "We should collaborate on something." This was on the intellectual side of our relationship. And, of course, Erik was Mr. Life Cycle at the time. A very influential man. Probably the most influential cycle analyst in the country at that time and even subsequently. So we began to think about what we should do. Back and forth and back and forth. As we talked about this, particularly in terms of arranging anything concrete, he deferred to me. He wanted me to take responsibility for any organization of any collective enterprise that we undertook. He said, "I'm retired. I don't have any infrastructure and so on." And he basically turned over the leadership to me but we talked about it together the whole time and decided to have a conference on the idea of the life cycle, particularly as it applied to adults. Not the early stages of development but adult development. Is there anything that can be said? The issue in the field at the time was: is there a systematic pattern that goes through stages in the adult life or is it mainly the adaptation of a less identifiable sort depending on situation and crisis?

So this was the intellectual terrain in which we were dealing. Erikson, of course, learned over strongly in the systematic pattern model of development at all stages of life. That was what his contribution was. But we were determined, and we worked through the American Academy of Arts and Sciences on this to try to get funds for it, to have a conference that turned out to be in the Center down at Stanford. Bringing in psychologists, sociologists, psychoanalysts. A mix of people to contribute to this particular issue with the idea that the papers that we would commission and that they would write would turn into a book. And once again, Erikson left the whole leadership to me in this sort of thing. Matter of fact, that book I organized after the conference was held just before we went to Europe for the EAP. I inherited the whole organization of getting the thing edited and put together, contacting a publisher. Ultimately we published with Harvard Press, with Eric Warner, who became a big figure in my life later on, was the editor at Harvard at the time. And so that book called *Themes of Love and Work in Adulthood*, which came out in 1981, was the result of that conference which was held in late 1977. It was just before going to England. We went in July of '77. I think it was held in the late spring or in June of that year at the Center. So I organized that all during the time that I was directing the at the London EAP Center and was in constant contact with Erikson.

Now we get over toward the Russell Sage foundation. I had been itching for a while to get back into a really major scholarly project and that sent me in the direction of going back to Victorian England, a period which I, of course, knew intimately from many standpoints from my dissertation research. It covered the years up through 1840, the beginning of the Victorian era. But I decided that the nineteenth century was my century and I was going to do some other type of work. And I was heavily under the influence of Erikson at the time and the Russell Sage Foundation had developed a program in life cycle analysis, an interdisciplinary program that they had chosen as one of their lines of research. Matilda Riley, a very famous sociologist, was the

intellectual force behind this program and she was a member of the staff of the Russell Sage Foundation. And there were a couple of conferences I went to in connection with that.

I had the idea that I was going to take British education, primary and secondary education and bring to bear—and to write a historical monograph on the changing bases of the life cycle in these years that came up with the introduction of formal educational systems. This was the framework I was going to bring to bear. That wasn't a literal application of Erikson but it had the idea of the age cycle as being the central focus of things and what are the kinds of social and institutional and psychological problems that arise in reorganizing the years of that phase of life and, of course, what the introduction of primary and secondary education did is to change the whole complex relations of family, work and education into a new pattern. I was going to use this idea of the structuring of the age sequences as the intellectual framework for the monograph. And, in fact, I wrote two articles at the time. One came out of a conference at the Russell Sage Foundation. Called it the "The Triangulation of Work, Family and Education in England." I wrote it with a woman named Sidney Halpern. She was a graduate student at the time. She did a lot of the research connected with it, so we wrote it together. And then, a little bit later, I wrote an article on the Victorian family that was commissioned by an English editor of a volume.

So I was beginning to get into that field and begin my research. And the grant I got from the Russell Sage Foundation was one to take to Europe with me, that would help finance a little bit of research assistance and my own time to begin work on this major project having to do with transformation of education in British society, which was a book that was much delayed in its ultimate publication but which came out in 1991. I'll tell you the saga of that research later on.

But one of the most interesting things that happened in developing this line of research was that as I began to get into the materials, reading the blue books, reading other people's history of the schooling in the country, doing the usual preliminary searches into the problem, I discovered and became gradually convinced that this life cycle formulation was not going to be the most helpful one. It just didn't seem to fit what was going on. As I put it, the subject matter turned its back on me as I brought this particular framework to bear. The whole story of British education reform just in the end turned out to be a story of religious conflict and class conflict. And it just hit me in the face that I wasn't asking the right kind of questions. So it was over a period of time that my framework changed. But the framework changed largely by virtue of what history was telling me and what was really going on. So I really developed a very different kind of model to explain the vicissitudes and the course of development and the fights and the loyalties. The book is about that, not about the age cycle. As a matter of fact, that perspective is almost unidentifiable. It's one of these very interesting intellectual journeys that you take. And you have

false starts and you trip over things. I think it was fortunate on my part that I wasn't so committed to the framework that I had been working with and linking myself with Erikson, that I could step away from that and really make the decision that that wasn't the story of British primary education at this time. A very different kind of framework had to be brought to bear and that framework was invented in part out of an adaptation of some of the things that I had written about in my doctoral dissertation but on the whole a rather new version of social change that I brought to bear in that book.

17-00:57:50

McIntosh: Well, this case seems like a good example of the value of historical work for sociologists, as well, and the ability of historical work—

17-00:58:01

Smelser: Empirical historical work. Let the facts guide you. Even though you sort of have made up your mind of what you think is important about what's going on.

17-00:58:10

Rubens: Yes. Your material tells you.

17-00:58:11

Smelser: The material told me I was really going down the wrong path, that it wasn't totally irrelevant but it wasn't the main story.

17-00:58:20

McIntosh: I think we're going to get more into the details of the composition of that book and the research for that book later on. But since we're at the origins of that project right now, I would be interested to know what the sources are that you're consulting for the article that you're publishing with Sidney Halpern and the later one on Victorian families. Because this is still when you have the life cycle model.

17-00:58:46

Smelser: I, of course, had a lot of intellectual capital built up from my doctoral dissertation because I had given some reference to education in that book. When the family was being reconstituted, I immediately turned my attention to some of the educational reforms that were agitating at the time, though I didn't go into them in any detail. But I was not totally unfamiliar with this. We did not have time in this case to get deeply into the historical process. It was one article. We familiarized ourselves with some of the main sources on what the changes in the educational system were and we wrote mainly, partly a theoretical, partly empirical story, of the tug of war between education and the family and work at the time because child labor, of course, was so widespread. We focused on that aspect of it. It was short. Compared with the subsequent work I did it was short on original scholarship and we wrote it in

the time span that was available to us. I considered it as a kind of introduction to my own more detailed research.

[Begin Audio File 18]

18-00:00:04

Smelser:

During the course of my second year as chair I got a feeler from the National Science Foundation. A part of their granting is in the social sciences. It's much smaller than it is in the natural and life sciences, but nonetheless there were programs there and they had a program staff that made the ultimate evaluations of support that were given. And it's organized by discipline. They had one in economics, one in sociology, anthropology and so on. And the mode was to go around the country and find academics to serve as program officers. Those program officer terms were for two years. This was because universities typically will not permit people to be gone more than two years without having to resign. Those were policies at the time. So the NSF accommodated its appointment policies to what university policies were. So I got this call to come back and be interviewed if I were interested in being the sociology program officer. It wasn't exactly the sort of thing that I would have sought out but I went to the interview. Interestingly, Robin Content was gung-ho that I should take this. "God, this is a new area of influence." She gave me a strong pitch. And I went back and was interviewed. The guy who had been program officer was leaving and he interviewed me, as well as several superior people in the staff of NSF and they offered me the job. They wanted me to come and take that position for two years. My heart was never warmed up to the position. If I had been completely and totally honest with myself I would not have gone to the interview. It was just kind of a touch-and-go decision as to whether I'd want to be interviewed because I sort of sensed what I discovered when I went back, is that I would have buried myself into a huge bureaucracy back there with a certain amount of formal responsibility but any kind of imagination that I could bring to bear on how much impact I would have was completely dulled by the interview processes. So I didn't have any trouble in saying no to that. I'm not sure how they came across me as a person. I had gotten one grant from NSF but that's not anything peculiar. And I, of course, later had a lot of dealings with the National Science Foundation when I was at the head of the Center because they funded the Center rather handsomely for part of the time that I was there. That was going on at the time.

Another very interesting thing was that the state of California, under—oh, he was a governor at the time—was very much interested in going on with the move to maximize attention to general practice in medicine and they were interested in supporting efforts in the state to do so and an initiative began on the Berkeley campus and was strongly backed by Bowker to get some kind of medical school.

18-00:03:36

Rubens: On the campus?

18-00:03:37

Smelser: Here at Berkeley, on the campus. And so he appointed a joint commission between the Berkeley campus and the San Francisco campus. Always the idea was that it might be collaborative between the two. A full scale medical school wasn't in the cards but somehow or other some program might get set up. So Bowker appointed me among the Berkeley faculty members who would be instrumental in designing such a program.

18-00:04:04

Rubens: Why do you think he chose you?

18-00:04:06

Smelser: I have absolutely no—

18-00:04:08

Rubens: I see that in your record that you had been involved with the evaluation of the medical school and—

18-00:04:13

Smelser: Well, my nomination may have come out of San Francisco because my link with Wallerstein involved me with the medical contacts in San Francisco more than—well, there were no medical contacts on the Berkeley campus and, of course, my research had never touched it. So there wouldn't be much reason on the Berkeley side to—

18-00:04:31

Rubens: And the affiliation with Cowell, although that was psychological services?

18-00:04:37

Smelser: Cowell. Well, that was relevant, of course, but it was nothing to do with my scholarship. It was kind of a student service on the campus. Some sense could be made of why I might have been chosen for it but it's a mystery as to how that process filtered up. You just don't know. It may have been an accident or some—

18-00:04:56

Rubens: But you were willing to take it?

18-00:04:59

Smelser: I took it. I took on this job. It turned out to be interesting. And I had an interesting role in it. That's the reason I put it down as something to talk about here. There was a tension from the very beginning between the San Francisco people and the Berkeley people. The San Francisco people were in principle interested in a joint program but they did not want to sacrifice one inch of the rigor of their training program, of which they were very proud, and they had some negative attitudes towards Berkeley as being kind of a soft-headed non-rigorous sort of place in comparison with their own professional training

program. They were, however, interested in a modest joint program whereby students would spend a period of time on the Berkeley campus and actually get a degree of some sort. At Berkeley they would do all the pre-med work and some interdisciplinary work, broaden out their pre-med training, then go and spend the clinical years, and then subsequently residency, maybe in San Francisco. But the clinical years would be on the San Francisco campus.

18-00:06:17

Rubens:

And this would be through the school of public health?

18-00:06:22

Smelser:

No. It was a special administratively created body ultimately. It wasn't out of the school of public health. It was a program of the Berkeley campus, not linked with a specific unit. Well, that tension between the San Francisco people and the Berkeley people took two forms. Mostly it was initiated by the San Francisco people. One is these people were going to take all the pre-med stuff. Any program we're involved in, they've got to have the rigorous training in pre-med science, right? And secondly, they didn't think Berkeley taught the right kind of pre-med science. It was just too academic. They didn't really apply it and students are not going to learn proper anatomy. They were not going to learn proper bio—so there was a real kind of status tension. The San Francisco people thought Berkeley was not really the place where they were going to get the right kind of training. And furthermore, they ought to have a lot of training. The San Francisco people began loading on these requirements that would have to be necessary to qualify them as being needed to finish their degree at San Francisco.

So the whole idea of the interdisciplinary character of this program was threatened by the—really basically by this pressure to just involve these people in a normal pre-med training and then they'd go into the medical school. And it would shoot the whole interdisciplinary goal—because they were going to get MAs or BAs or joint majors, whatever the plan turned out to be, in philosophy or in social science or in literature or wherever or maybe occasionally in biology. But mostly in science, the social sciences and humanities as a way of diversifying the training of medical people. That was the kernel, to give them a broader, more extended base of knowledge from which to begin a medical career. And Berkeley liked the idea because we were going to humanize in some way the whole process that we saw as lockstep, closing down the minds of these practitioners. So each campus had some reason to like the idea but many reasons to want to have it shaped in different ways. There was a lot of tension between the Berkeley and the San Francisco people. And in my view of these meetings, was that, while they never got really hot, very firm positions were being taken. And it occurred to me as we were going on in the first months, six months perhaps, of the design of this program, that it wasn't going to happen; it was going to be paralyzed.

So I was the one who came up with the idea. I said, “We can’t do this in the time constraints that are now being envisioned for the preparation of this. I say the only way we can do this is to add a third year. Add an extra year to the Berkeley program and let them have a proper exposure to something other than the pre-med. The only way to do it is to expand the time.” It did make for another year of training, for sure, and some people said, “They’ll never come if they have to spend yet another year in the pre-med,” but nonetheless my idea took hold. It took hold because it was probably the only way you can think about putting together a program that would satisfy the demands that were being made on both sides of the Bay about what it should be about. So we designed this 2-3-2 program, meaning five year undergraduate program whereby something the equivalent of an MA might be granted in this period of time, with the students having the option of where they wanted to make their supplementary studies and then going into them. And it was on that formula that we actually created the joint program, which exists up to this day. We only had twelve students. It was thought to be modest.

18-00:10:36
Rubens:

Was there a point faculty person for the program here at Berkeley?

18-00:10:43
Smelser:

Well, that was an administrator. I forget his name just now. He was the person who took over the first directorship. He was a biologist. I remember he was in on the planning committee and he became the first administrator for it. I subsequently served on the selection committee for students and in other capacities on the board. I was a member of the joint board later on. But in the design we put together, this program that was bought by both campuses. It turned out to be very, very successful. In fact, it was a little bit to the chagrin of the San Francisco people that the—there were twelve Berkeley candidates who every year would come over to do their—had superior performance to the San Francisco natives consistently. Well, there was good reason for that. We tended to recruit older students who’d had a lot of interesting experiences. We wanted to have the cohort be an interesting group, as well. We had a prejudice against what we called the straight arrow nuts who are going to go right into medical school and never look in either direction. And so we took into account these people’s work experience. It was a benefit that they would have been in the Peace Corps, for example, or something like that. So we, by way of recruitment, kind of set the stage for having more interesting and reflective people. And the ones who would come in such a program already were predisposed to be a little bit broader than the straight down-the-line medical progression. Turned out to be a very successful experiment and still is an ongoing part of the university’s involvement. It was responsive to the outside offer for state funds to help set it up because of the movement at the time. It became a very valuable enterprise on the Berkeley campus.

18-00:12:42
Rubens:

So your draw to it, firstly, the interdisciplinary nature.

18-00:12:50

Smelser:

Yes, the idea appealed to me kind of intuitively as a faculty member on the Berkeley campus. I thought it was a good idea and I was always committed to it. I think I have to say that I got some personal gratification of taking a leadership role. I didn't know that it would happen that way.

18-00:13:10

Rubens:

But you liked how systems work and how to—

18-00:13:13

Smelser:

Yes. And I was interested in having some influence on it. As it turned out, I was able to break some kind of logjam. I wouldn't say that would have killed the program but it certainly would have imparted a different character to it if some compromise had not been worked out.

18-00:13:30

McIntosh:

You mentioned you maintained a relationship with it?

18-00:13:34

Smelser:

Oh, yes. I was on the board. And I was on the selection committee for several years. After I came back from England, I served a couple of years, I believe, on the selection committee.

18-00:13:44

Rubens:

Was Len Duhl involved with that?

18-00:13:45

Smelser:

Yes, he was. He was one of the figures in that. He was out of public health.

Now, I was also active nationally in the Social Science Research Council. They had a huge crisis in which one—Henry Riecken, one of the directors, was fired in the early 1970s. I, having been involved in the committee on problems and policy, I got onto the council itself and for a period of time I was actually chair of the council, between 1971 and 1973. And in that period we had a crisis in the SSRC. It had to do with the director wanting to move it to Washington, DC from New York. And it was of course tied up with the presence of federal funds and a lot of the professional societies were moving to Washington at that time. And this man, Henry Riecken, he was a teacher of mine at Harvard in social psychology, who pressed very hard to make it a Washington enterprise and all the old established SSRC types were vehemently opposed to it. They thought they were going to be entrapped in the Washington scene and the whole distinctive independent character of SSRC would be compromised, if not sacrificed. So you had a huge clash. And Riecken wouldn't give in. So in the end he lost his job just because this conflict couldn't be resolved with him in that particular position. So he left.

He subsequently had a long and quite distinguished position in the National Science Foundation. He might have been behind my being asked—because I was his student in a couple of courses and he helped me out in my

undergraduate dissertation. And we liked each other and he was also a big man in that BASS survey that I participated in through the NSF and SSRC.

Anyway, I was already a big figure in the SSRC by that time because I'd chaired their board for a couple of years. And I was put on the search committee to search for the new chair, new head. We selected a woman after a long search named Eleanor Sheldon who had been with the Russell Sage Foundation. She was a sociologist and she was a very strong leader and we appointed her as director of SSRC. She then brought me on as chairman of the most important committee on SSRC, which was called Problems and Policies. We decided policy issues and what new fields should be emphasized within SSRC. We had a very strong kind of exec—it was almost like an executive committee that she worked with and I was chairman of it. And I worked with her. She turned out to be an extremely strong—strong is the weakest word I can use. She was a very pushy woman and she wanted things her own way and she didn't brook opposition and ultimately she got herself—her term wasn't terribly long, maybe five years. She created a lot of noise. If you wanted some person who wanted her away, she wanted it and she felt the council—meaning the governing board—was a nuisance that stood in her way and raised objections to what she wanted to do.

And I had an actual fight with her at one time when I was chairman of P&P, problems and policy. She thought I'd be a good chair of it and so I came to meetings and everything. And it was a time when the affirmative action things were bubbling up and that affected the SSRC in terms of who they appointed on the committees and whom they hired locally and so on. She was absolutely a stick in the mud opponent of affirmative action. She just didn't want to have anything to do with it and she was extremely assertive about that and the committee which I chaired was not so adamant as she. As a matter of fact, we came into conflict with her and I came into conflict with her on this whole issue of what kind of policies we should be thinking about, what kind of diversity measures we should—I was on the affirmative action side compared with her. And she would have got me off that committee. I knew it. She just didn't want this kind of opposition. As it turned out, I resigned when I went abroad in the summer of 1977, so this big fight with her was resolved. I later came back to be on the board of SSRC in the nineties for six years.

18-00:19:21

Rubens:

Well, when you came back you were also on the nominations committee.

18-00:19:23

Smelser:

Oh, yes. We chose Fred Wakeman, who was a historian here as director of SSRC when I was on that committee. So I kept my relationship with the SSRC going. This is a part of the larger story of my involvement in these various national—Russell Sage Foundation, SSRC, National Academy of Sciences. Those and other foundations.

18-00:19:50

Rubens: So you were going through this contentious experience with this woman the same years that you were chair?

18-00:19:56

Smelser: Oh, yes. I was chair at the time. So that reason became so direct because I was the one who was really speaking for that Committee on Problems and Policy and she just wanted to put all—there were some resolutions that were going to—

18-00:20:09

Rubens: Well, I meant chair of the Department of the Sociology.

18-00:20:12

Smelser: Oh, yes. I was chairing sociology at the time, going to New York for the meetings. I was on the other side of the fence there than I was here. I was a navigator on this campus and I was an advocate within the confines of her SSRC presidency.

18-00:20:34

McIntosh: So what were the policies and problems that your committee was addressing?

18-00:20:41

Smelser: Oh, normally the committee in its routine work would get proposals, say, forming a new—one of the main things the SSRC did was to form new intellectually oriented committees, usually interdisciplinary. I referred to that committee on economic growth that I was on when I was a very young faculty member here, that was an SSRC committee. And they had one in political socialization; they had one in comparative politics. And they would foster new research. They had some money and they could seed new research, they could start new ideas. They commissioned volumes and conferences and so on and so forth. Those were the problems and policies that committee dealt with. And especially it had to do with the hiring of staff in SSRC and other matters which affirmative action would touch. She became so adamant that we got drawn into it even though personnel policies were not the normal range of interests of the policy committee. But we got in there because of a peculiar initiative on her part.

18-00:22:03

McIntosh: Before we talk about the Education Abroad Program, there's your induction to the American Philosophical Society

18-00:22:07

Smelser: Oh, yes, yes. The American Philosophical Society, of the three major societies, is one of the most conservative. It's the smallest of the three. It takes in a very limited number of people. It had an upper limit of 500 people and it's basically—they open up new places when the old places get vacant through death. The age of the average member of the American Philosophical, I learned now because I'm still involved in it, is seventy-four and there are many, many in their nineties. They're always having youth movements that

never work in bringing in new members. But anyway, they were having a youth movement at this time. I was nominated by Wilbert Moore, one of my close colleagues—he was a student of Parsons, a little senior to me. He was active in the Russell Sage Foundation. We became professional friends through the American Sociological Association. We had common interests in social change and economic sociology. We were very close. He pushed my name, though at the time to be inducted into the American Philosophical at age forty-six was almost not heard of. But they had a youth movement. And he nominated me and I went through the—what is a labyrinthine series of elections and was named to be taken in. And the induction ceremony was in 1976, which was the bicentennial year.

Sharin and I decided we ought to take advantage of this and go to Philadelphia with our children, who were by this time eight, six or something like that. So we went back to Philadelphia for a prolonged stay and then went to Washington, DC, which were the two great cities where the bicentennial was celebrated. It was a beautiful trip, actually, to take the kids and show them around the two cities, which had really put on the dog for the bicentennial celebrations.

Well, it's a very stuffy organization and they had an extremely formalized induction ceremony. You were sitting out in the audience and they called your name and you walked up to this front and you signed a book while they had some words about you and then you went back down. So sequentially there were that year maybe ten or fifteen people being inducted at that meeting and I was there and so on. So I brought the family to see me inducted. And so here I was sitting with Sharin and then there was Joe, our son, and Sarah, our daughter, sitting next to me. Joseph's name, my son's name, is Joseph Neil Smelser. I'm Neil Joseph Smelser. We didn't make him junior but we did give him the same—we gave him the first name of my father. So when they were reading out this bombastic call they read out Joseph Neil Smelser. And Joe ran up. [laughter] Joe stood up and this row of ninety year olds that were sitting there in the middle, the whole place just absolutely—totally fell into laughter that this little boy was going—That was their youth movement, right? And he's the one who took it seriously. He thought they were calling him.

I tell that story to the people at—actually, tomorrow I'm going to an American Philosophical meeting and we're flying early tomorrow morning. Going to Philadelphia tomorrow to their annual meeting. I'm on the council now of that. But that's one of the nice little human stories of my life. Well, I think we've now gotten to the trip abroad.

18-00:26:22

Rubens:

I think so. But one other job of yours—you were with the undergraduate curriculum development group of the American Political Science Association.

18-00:26:38
Smelser:

Oh yes. I'll pick it up. I was asked by a man named Donald Stokes, who was the head of the School of Public Policy at Princeton. They later tried to hire me into the school of public—Woodrow Wilson School at Princeton in 1979. It was in the second year of my time abroad that the offer from Princeton came to join the Woodrow Wilson School—to be the first sociologist to join the Woodrow Wilson School. [Offer for Princeton discussed Interview #11]

Anyway, Stokes was the head of an undergraduate education committee within the American Political Science Association. Somehow or other, somebody brought me to the attention of Stokes, who said, “We’d like to have a couple of people outside the field of political sciences talk about curriculum.” So I decided, “Well, what the hell. I’ll do this.” And so we had several meetings, mostly in Washington, maybe one up in Woods Hole, I’m not sure, in which we were talking about the most effective pedagogical ways to present political science to undergraduates. Of course, in the course of that, I developed a lot of my own ideas about undergraduate general education that were to later feed into this essay I wrote for Carl Kaysen through the Carnegie Commission. Those were mainly ideas generated in this undergraduate group in political science. It was kind of a strange foray for me to be in. But nonetheless, I had a lot of electricity in that group. It was a very interesting one and rewarding really in terms of my own continuing development. I’d say it was a kind of fore-runner to my work on the lower division and on general education generally.

Okay. After this glorious magnificent year we had in 73-74, I was very much interested in returning to Europe in some capacity. I, of course, didn’t have a sabbatical until another seven years and that was it. But I also was very much aware of the Education Abroad Program, which runs by the appointment of a director for each regional center—there are dozens of them—for a period of two years of supervising students who are studying abroad. I very much wanted to have it in the UK. I thought it would be much less complicated for our children to be in the UK rather than me going to France or Germany, one of the others in which there would have to be a very complicated language adjustment on their part. And so I had my eye on the UK.

The UK is an interesting one among these. Most places have a center in which you go to it and all the students are around you. The UK is a big program. It had 135 to 140 students per year from eight campuses—not from San Francisco, and of course Merced wasn’t in the picture. Mostly juniors who would go abroad and study in one of perhaps ten or twelve British universities, only one of which was in London, even though the administrative office of the EAP was in London. It was on a little side street not far from Westminster Abbey. A very charming location. So the beauty—

18-00:30:01
Rubens:

And also Ireland?

18-00:30:02
Smelser:

Ireland. UK and Ireland. There were students at the University College Dublin and at one other campus, at Maynooth. And so we supervised these students during their year. They were not residing where you were and you were responsible for their academic performance and assigning grades, American grades, California grades to their work that they did in those places. So the role of the director and the associate director was, three times a year, to go around to all these colleges and visit all the students, the ten students or twelve students that were at each of the colleges. Three of them were in Scotland, two were in Ireland, the rest were scattered around England and there was one small program at Westfield College in London. But that was quite incidental. And your role was to be academic supervisor. Kind of a dean, a local dean. An uncle. Kindly uncle to these—kind of an entertainer, and as it turned out, occasional therapist to these students who went abroad. And I then, of course, had to maintain full contact with the office in Santa Barbara for budget, for visitors who would come there who wanted to—I had to entertain David Saxon, for example, when he came and the new chancellor of Santa Barbara when he came abroad. It fell to me as the director of the EAP there to be their official host and show them around and do what we could. So you had a role looking back toward the United States as well.

I was completely severed from my Berkeley connections. I didn't come back to the United States for two years except for an interview at Princeton when they offered me a job. That was it. So I was completely away from Berkeley campus at that time.

18-00:32:13
Rubens:

How did you get appointed, by the way? Did you apply or who—

18-00:32:17
Smelser:

I applied. I applied and then Bill, Bill Alloway, who was the director of the whole program at Santa Barbara, came around. Went around the state interviewing various applicants. It was a heavily applied-for position. I wasn't at all certain I was going to get it. He interviewed me and he interviewed Sharin for the job. He was especially proud that one of the functions that we had was on Thanksgiving, to get as many of these students as we could to come to London and prepare a Thanksgiving dinner for them in our home. There were eighty. Eighty of them would come to this Thanksgiving dinner. So he interviewed a lot. "What's your idea about this kind of entertainment?" and so on.

18-00:32:56
Rubens:

But is it always an academic?

18-00:32:59
Smelser:

Yes, it's a professor. And, of course, they had candidates from different campuses so I didn't know how I was competing with the exigencies of appointing people from different campuses either.

18-00:33:10

Rubens: And usually a two-year appointment?

18-00:33:14

Rubens: Well, I can see why they chose you. Your research alone—

18-00:33:17

Smelser:

Well, I made it. And, of course, I had the idea that—I'll confess this to you. I had the idea that running the EAP was going to be an incidental part of my life and that I was going to really bomb ahead on this research project that I had now formulated about British higher education. As it is, I did a lot of research there. But I completely misread or misestimated how involved I would get with the students. They were already a select group of students. They have some pretty good requirements that people who go abroad have to have certain grade point average. They even give them a little bit of an in—not exactly a psychiatric interview but they interviewed them from the standpoint of if there's obvious vulnerability they don't want to send the kids abroad for that kind of period, which can be psychologically complicated. But I came to like these kids. They came to London always on their way to their campuses and then I would go to the campuses. They always streamed through London during their holidays in which they'd be going abroad or traveling on their own abroad. So there was much more contact with these students than you might guess from the fact that they were physically isolated from me during most of the year. We'd go around and we'd give them these nice—every town we went to we gave them a nice dinner, took them out to a kind of restaurant that they couldn't go to otherwise or wouldn't go to otherwise because it was a little more expensive and then interviewed every single student on every single trip that we took. And then in the end we had to grade them and make sure sometimes—and then there were individual problems that came up. And I'll mention that in just a minute.

I did get a lot of work done on my doctoral dissertations. I did a lot of primary work. I was stationed very close to the British library and I went back to bury myself into the North Room, reading room of the library, all the parliamentary papers, all the debates on education, all the secondary stuff. I built up a tremendous reservoir of raw material for my study without actually doing any writing. It was just that stage of my research. And I would be able to take afternoons off in the different periods of time when I wasn't engaged in the business of the office. I had an associate director. I had a secretary. And so I was able to do a lot of work. But it turned out to be out of keeping with my original expectations of making this basically a two year sabbatical. It wasn't that. I was heavily involved in the administration of the program and came to like it a lot.

We had a very interesting social life in Highgate, where we lived. Our children went to the Saint Michael's School and we became very involved in that school and came to know the headmaster—he and some of the faculty members. We were really deeply involved and the kids got really involved in

it. It was a super school. It was a state school even though it was originally Church of England. Saint Michael's was its name and the local vicar came and closed it up every Friday. But it was basically a state school, though they did have religious education in it still, which was a national requirement in British primary education, to teach some Christianity to these students. It was pretty watered down Christianity, but nonetheless they had it.

So we became friends also of many parents of many of our kids' classmates and this was really interesting because there wasn't any personal competition. They thought I was a professor from abroad and almost none of them were academics. So we became acquainted with all kinds of people who were in the world of advertising, worked in business, worked—some primary school teachers, others. And so we had a very warm network centered around our own children. And some of these friendships last to the very present. We're going to London in the fall. We're going to stay with some friends we made in Highgate at that time. So it was a socially really full time.

And I had some official duties. I would go to the American embassy from time to time for certain ceremonies. I went to the University of Cork when it was celebrating the 250th anniversary of Bishop Berkeley because Berkeley was named after Berkeley so they wanted to have a representative. Bowker called me up and said, "Go to Cork." Said, "Go to that ceremony." So I had kind of a—there was a certain minimal diplomatic role that I played. I was also invited frequently to speak on sociological topics in those campuses where our students were. There's always a sociology department in these and they knew me. And so on every campus where our students were I gave at least one presentation of a professional sort and then I got involved in the—I forget the exact federal agency it is but they maintained a kind of logbook or a record of all American academics and other scholars who are abroad at any given time and they supply this to different European universities. So I got invitations to Paris and to various places in Germany to lecture from time to time. And, of course, we traveled ourselves in our little—got another camper and we traveled during these long vacations that the British institutions had. We would go abroad ourselves. France, Italy and so on, and continue this camper life that we liked so much.

18-00:38:54

Rubens:

Now, you were writing up the book with Erikson and doing your own research.

18-00:39:00

Smelser:

Right.

18-00:39:01

Rubens:

What did you lecture on?

18-00:39:02
Smelser:

Oh, I lectured on standard sociological topics. They asked me if I would lecture on the contemporary changes in family, for example. It was a frequent topic that I would lecture on. I gave some lectures at the London School of Economics on higher education, where they invited me to come and give a certain sequence of three or four lectures and I talked about recent changes in higher education there. It was a negotiated series of topics. Mostly professional sociological topics on which I had written or which they—I talked about my dissertation research at Oxford and Cambridge where I received an invitation to come up and speak there. So I kept my intellectual life alive, as well, both in libraries and in presentations around Britain, mainly, but the continent in addition. So it was an active period of time for me in all respects.

I guess I should mention something I did not anticipate but which turned out to be one of the most important parts of that job, the dealing with students who had trouble. There are 130 of them. You would always expect some problems. Most of these were psychological problems. I had one case of a guy who got arrested for dealing in hashish in the Birmingham library. There were no suicides, luckily. There had been in the past a suicide in some director's periods there, which was extremely bad news. And no threats of students dying from some illness or something. I escaped all of those. However, there were some percentage of students, a small percentage, five, ten percent, who developed what you might call fairly serious psychological problems associated with being away. Most of these problems had to do with some trouble at home. A divorce. There are a couple of cases of a sibling who died. A crisis, real crisis. And this precipitated serious psychological complications for the students who were studying in England. And the impulse, almost uniformly, was to drop out of the program and go home. Just forget this year. And they would always bring to my attention, these problems, one way or another. And so on occasion I went to the colleges where they were. But mostly I would talk to people in London.

Of course I was brand new out of Cowell Hospital. I had finished my analysis a few years before. I was obviously equipped to kind of understand, although I never pretended to appoint myself as therapist to these students. But I did talk to them sympathetically, but tried also to suggest alternatives to the rather extreme solutions that would always come into their mind about quitting. I'd say, "Well, look—" and they all got the idea that they were going to have some kind of—or typically they got the idea they were going to be some kind of savior. They'd go back to help out someone who's suffering as a consequence of this event, their parent or brother or sister, whatever, and that there was this almost interesting reaction to these disturbing events, that they would be heroic or needed. And, of course, the big motive of this was guilt that they were away and they were sort of on this privileged appointment and basically didn't deserve it. And somehow there was an atonement motive that I picked up on the part of these students.

And actually, I did some of my theoretical thinking about the odyssey experience in the middle of all this. Because all these students were on odysseys, right? One year away, very special. They're selected and they felt special and so I began thinking about this whole issue of feeling special. Other people are not special and here you are and something good happens to you or something bad happens to someone else. This is going to create additional complications as to how you're going to react to it.

I was usually able, by very careful talking, to persuade these students to take time off and go home. Two weeks maybe. Do what you can. But I always raised the questions of exactly who they thought they could save or what they thought they could save because these were usually unrealistic expectations on their part when they would get them. And so a very few dropped out. A couple dropped out but I did hard work to try to help save the year for these students who were having obvious trouble. Some simply couldn't cope. There were also student kind of disorders of heavy sleeping and not being able to study and all the other symptoms. And I talked with some of these students, as well. You can't say that I was a therapist but I think I had more than usual sensitivity to the kinds of problems that students under these circumstances would develop. And, of course, they're all played down because all these studies were romanticizing their trip abroad. It's the sort of thing on which it's special and nothing's going to go wrong. It's going to be a beautiful year. Then bang, something happens and they're somewhat more vulnerable to psychological disturbances at the time.

18-00:44:54

Rubens:

I know that country directors in the Peace Corps have to deal with pregnancies and marriages. I don't know if that came up. Would that be in the purview—

18-00:45:05

Smelser:

I would always say we had no suicides and no known pregnancies in these years. So I escaped the big ones. There was a near death of a student, under another director, who nearly drowned down at Cornwall. So those are the really bad ones. I guess I was fortunate in not having the toughest of decisions under these circumstances. Because it can happen. A hundred and thirty people, you're going to—

And I knew enough about the British educational system, too, because I had been in it myself and was acquainted to be able to help them cope with different kind of expectations and different kind of classroom settings. And some of them are in tutorial relations with their faculty members and so I would try to do—did quite a bit of orienting of these students.

I tell this funny story about the orientation of this. They all come to London first, 130 of them, and we give them a dinner and I give them this brilliant speech, telling them about British society, about British higher education, about different understandings that the British have about friendship and you

should be alerted to this. So all of the usual—a very exhaustive orientation. And then at the end I'd say, "Any questions?" "Where can I get a vegetarian meal?" "How do you get 110 volt electricity?" [laughter] All their instant moments of detail that they were all focused on. All my orientation work went to naught.

18-00:46:55

McIntosh:

Now, while you were over there, Parsons passed away?

18-00:47:01

Smelser:

Yes, he did. Here's the story. Parsons was—we had continued to maintain a relationship despite this slightly rocky period we had in the early seventies. Parsons was working on a book which was called the *The American Societal Community*. That was to be his last work. It actually wasn't published until a few years ago because he never really came close to finishing it. But he wrote one chapter in particular called "The American Economy" and he revisited a lot of the ideas which we had visited in *The Economy and Society*. And he wanted me to take a look at that. So I said fine. So he sent it to me. It was in very rough form, though it was coherent enough for me to have reactions to it. So I wrote him back in Cambridge a really long and quite critical letter. Ten, twelve pages, really highly detailed commentary on what he'd written and he wrote back a thank you letter and said he was going to be coming to Europe. He was going to be lecturing at Heidelberg and in Munich. And receive an honorary degree, I believe, at Heidelberg if I'm not mistaken at that time. This was 1979, spring. And then he would come to London on the way back and we'd spend the day together talking about this work and anything else. And this was going to be, I believe, in May of 1979 or in the spring for sure. And I received a postcard from him in London on a certain day confirming our meeting on the following day and shortly after receiving the postcard I got a call from his wife saying he'd died the night before. So he never came to London and, of course, I was deeply moved. Actually, I think I was in Ireland at the time. She called my secretary. My secretary called me and I got the news when I was in Ireland seeing students. And, of course, I was deeply disturbed by it. And this was a spooky sort of thing to have this postcard arrive and then he doesn't show up.

Subsequently I was asked by a German scholar, Uta Gerhardt, at Heidelberg, to write an article on Parsons' economic sociology. And I took the occasion to review that correspondence between me and Parsons at that time and how it related back to the early work that he'd done and the problems I had with it, the shifts I pointed out, the embarrassments I thought were in his chapter. So I did do an article that was published in something around 2005 that is an account of the last intellectual exchange that we had. There were more things in the article than that but it was mostly based on intellectual issues. And I included some personal notes in that as well. So that was a sad moment of my time abroad.

- 18-00:50:24
Rubens: Did you feel any unresolved feelings or—
- 18-00:50:28
Smelser: Well, I had sort of come to terms with my conflict with Parsons. When a person dies, every past ambivalence makes its appearance and, of course, I experienced those. I was quite saddened. I would say there was a strong component of—
- 18-00:50:46
Rubens: He had a heart attack, I assume? It was a sudden—
- 18-00:50:49
Smelser: Yes. He had been suffering from diabetes. He died in his sleep in Munich. Subsequently I went to lecture at Munich and spent a lot of time with a man by the name of Hesse who had been his host. So we compared a lot of notes about that visit and our general feelings about Parsons, as well.
- 18-00:51:16
McIntosh: Well, I hate to follow-up such a kind of emotionally laden event with an intellectual question.
- 18-00:51:23
Smelser: Oh, then, never mind.
- 18-00:51:26
McIntosh: The mid-seventies to late seventies, I think by that time Keynesian economics is in disrepute, right?
- 18-00:51:36
Smelser: Yes.
- 18-00:51:36
McIntosh: And if I'm not mistaken, *Economy and Society* was largely informed by Keynesian economics.
- 18-00:51:47
Smelser: It had a lot of reference to Keynes. There were others. Marshall and Schumpeter played a big role. But Keynes was big in it, yes.
- 18-00:51:55
Rubens: And so in the chapter of Parsons that you critiqued for him, is he still sticking to that sort of Keynesian economic theory or is he—
- 18-00:52:06
Smelser: Oh, Parsons was dealing with an issue that had come to the fore during the great conflict over his theories. And that is the tensions between the studying or emphasis on conflict versus integration. Parsons had been the whipping boy of conflict theorists all during the late sixties and through the seventies. And Parsons had himself reacted to this controversy and to this attack, becoming somewhat more brittle in his own theoretical position. So, in a way, his essay

that he sent me was a reaction and a reaffirmation of some of the integrative threads that are not normally thought of as being integrative in economic life. For example, he gave a special interpretation of labor/management relations which stressed the peacemaking qualities of the involvement of common membership between labor and management, these sides which don't show up in the labor/management literature very much but which he was reasserting. And I saw it as a large part of the agenda, reasserting his main intellectual thrust of his work at the time. And, of course, I engaged in this in my comments as delicately as I could, pointing out the reservations I thought I had about his new formulations and shifts that he had taken, without acknowledging them, of positions that we had written about in *Economy and Society*. This was, in a way, a reactionary document that he wrote because he was beleaguered. The man was really wounded after ten, fifteen years of being bludgeoned. He was a proud man. He was proud of his own work. He believed he had genuinely contributed with this emphasis and he stuck with it and he wasn't giving an inch to his critics. So I guess that's the framework I would bring to bear on that particular essay that he sent me. And then I engaged him in a dialogue on this whole issue once again.

He sort of responded to my comments in one way. I got a copy of the manuscript that survived his death and he had written some comments, marginal comments in response to my letter. And so when I wrote that article in the nineties or late 2000, Uta Gerhardt got the copy of that last article and sent it to me with his comments on my comments that I had to decipher and work out. He never formally responded to me because he died and all of that was going to come in our conversation.

18-00:55:01

McIntosh:

Well, we're almost here at two hours.

18-00:55:03

Smelser:

I think we've reached the end of my discussion here. I will just only say that I was, of course, away from the Berkeley campus that whole time and the visitors kept coming through. That was another feature of this. We got visitors from not only Berkeley but everywhere else. We hosted a lot of people. But I forgot about Berkeley sort of during this period of time but they didn't forget about me. And in the spring of 1979 I got a communication from the office of the academic senate asking me to be chair of the Committee on Educational Policy when I came back. I was a sitting duck. I felt a residue of privilege and guilt at having had these fantastic two years. I felt I sort of owed the campus something. I believe the senate knows this and they pick a lot of people for onerous assignments after they've had a very pleasant sabbatical or something of the sort. And so I agreed. I agreed to come back and chair the Committee on Educational Policy and proved to have a very exciting year. And it opened the door. It was the first event of what I have to describe as the 1980s as my decade of service.

That's the story of that and it's the story of the very halting progress I made on my major research project even though I stayed active in a variety of ways. I was able to finish it by the end of the 1980s when I had a full year to return full-time to that project.

18-00:56:40

Rubens:

And so we'll take that up next time.

Interview #10 May 10, 2011

[Begin Audio File 19]

19-00:00:00

Rubens: Hello, Neil. This is the tenth interview. It's the 10th of May and Jess and I are here to talk to you about your decade of service to the Berkeley campus, to the university system at large and then some beyond the university. But before that, I want to reference that while in London, you discussed how you'd been involved in your children's school. And when you return, in 1980, you go on the board of Head-Royce, the private school where your children enroll. How did it come about and why did you do it?

19-00:00:44

Smelser: Well, it came about because my children were enrolled in Head-Royce at that time. We came back and the kids were in fourth grade and second grade, I think, and we took that route, of putting them in private institutions at that time. They were heading toward that era in which—the time in which there were certain problems in some of Berkeley public schools, so we went that route. And the Head-Royce—I don't know how my name came up to them. It was all a surprise. But I was asked to be on the Head Royce Board of Trustees. I felt in a way pleased and obliged, because my kids were there, and the school was in a particularly dynamic phase of growth. There was a change in leadership. As a matter of fact, I turned out to be a very strong advocate of the guy who came, Paul Chapman. We more or less found him in San Francisco and I pushed him very hard against the rather more conservative side of the Board of Trustees. And he stayed there for decades.

19-00:01:57

Rubens: Just retired last year.

19-00:01:58

Smelser: Very effective leader and I'm very proud of that episode because I fought a—there was a fight in the board over him and a couple of other people.

19-00:02:05

Rubens: He came out of Stanford history.

19-00:02:09

Smelser: Well, he was a student of one of my classmates, David Tyack at Harvard, and so we had that link, too. The Head-Royce School was an interesting one. I used to describe it sort of comically as having two cultures. One was a culture of the Berkeley academics because many children were enrolled there and they were a very important group. And the second was of the socially-conscious ladies of Walnut Creek. It was a socially conscious school and they were all into their own dress codes and dances. There was a social side of it that I was somewhat alienated from because it was one of these kinds of privileged places, something that CPS [College Preparatory School], where they went subsequently, didn't have. That was a straight academic school.

And so I started to feel I was representing the serious intellectual side of the school when I was on the board and having debates. Whereas there was a lot of talk about issues and fundraising and rah-rah and what an important school this was and having all the right people here and so on. There was also an affirmation action movement, such as it was, to bring minorities to this school on fellowship funds and I was, of course, very much behind that. I found it quite interesting, but in the end we took our children out. They were there about three years, I think. But we took them out and put them in CPS because we thought that was an intellectually more potent school and we turned out to be right. They had a superb educational experience there and I think the social atmosphere was one that fit with our expectations and preferences and they had good years there.

19-00:04:00

Rubens:

So you were on the board six years, though? That was—

19-00:04:03

Smelser:

That's right. Yes, it was a long time. And toward the end was when I did the search committee with Paul Chapman and then after a while I said okay, that's it.

19-00:04:14

Rubens:

About how often were the board meetings?

19-00:04:17

Smelser:

Once a month, I think, in the evenings. It was a pretty big commitment. And I got involved in things and found it to be, as I say, profitable with this ambivalent fit between my philosophy and the school's culture.

19-00:04:35

Rubens:

Okay. I just thought it was important to document.

19-00:04:39

Smelser:

Thank you.

19-00:04:40

Rubens:

And it bespeaks the variety of interests you have in educational systems.

19-00:04:45

Smelser:

Sure.

19-00:04:45

Rubens:

All right. So you've come back to Berkeley, with the prior commitment that you will serve on the educational policy committee.

19-00:04:59

Smelser:

Yes. They contacted me in England on that. I had been head of the policy committee, which was a very important committee. It was the executive committee of the senate on the campus at the time. It went through People's Park. It went through the Reconstitution Movement. So I was in the thick of

things and I chaired that committee for a year during a very hot period. So I was known in the senate. There is this theory that after you're on sabbatical or after you're away, you're a sitting duck.

19-00:05:30

Rubens: You owe them.

19-00:05:30

Smelser: And I was, yes. I didn't object to sitting on this committee. The committee itself is a very important one in the ongoing day by day work of the senate. It doesn't make many headlines because its main work is to review all other committee work that's done. Any report that comes out, we assessed it and made recommendations separately from some kind of investigating committee. One of the most interesting ones of these was the Berger Report on the School of Education, which was very dismal. I think it came out in '79 or '80, something like that. It was fresh out and it came to us for review and we made commentary to the head of the senate, to the administration, on this. And that, of course, was, what, just before I got put on the committee to review the School of Education, which was just after my service on the Committee on Educational Policy. And I'll come to that in a second.

The other memorable thing that happened on the Committee on Educational Policy was an initiative on my own. And that was to do with the requirement for American Studies. It had been a requirement on the Berkeley campus that all undergraduates had to take a course in American Studies and one in American institutions. This went way back to the early 1920s. It was a response to the right wing kind of nativistic backlash after World War I and the university accommodated by adopting—it was a kind of national accommodation on higher education—by adopting these requirements. And they'd been on the books the entire time, for sixty years, without much change.

19-00:07:26

Rubens: I'm sorry I'm stuck on this. This wasn't called a U.S. institutional requirement? Did they use the word American?

19-00:07:34

Smelser: History and institutions. American history and institutions. And what had happened was that the requirement had drifted into the history department for American history and into political science for American institutions. Kind of a very advanced civics course on the constitution and things of that sort. Of course, they had enormous numbers of students enrolled in this. But however, it had an interesting evolution and this is what I noticed and was very concerned with. Other units smelled the enrollments that were gigantic in this and they began putting forward courses that would qualify for American institutions or American history. For example, the sociology department had a course in American society. They said, "Why can't this count for American

institutions in the same way as American political institutions did in the political science department?"

19-00:08:40

Rubens:

And you'd been a supporter of this, for the sociology department though.

19-00:08:51

Smelser:

No. It had been done without my notice and it was just there when I was—and we had big classes in it. And you got it spread out and spread out and spread out. It was different units asking. At the time I came to the Committee on Educational Policy, I counted eighty-eight to ninety courses that qualified for this. And they'd gone all over the place. The joke was that this course in the big band era in American music qualified as a course for American institutions. And of particular notice was the fact that the ethnic studies programs, which were relatively new as a result of the ferment in the late sixties, had themselves put forward courses in ethnic history, the ethnic groups which our department's concerned with as also institutions. And they were in fact legitimate and they were approved. But at the same time they were competitions for enrollments in programs that were having some trouble getting ample enrollments at the time.

However, I saw this multiplication of courses, both in history and in institutions, as being a kind of corruption of that requirement. It had become a basis for academic competition for students' bodies, right. I said, "That's not an intellectual justification for this requirement." So I initiated a movement in my own committee, basically, to kill this requirement. It's no longer relevant. They can meet it in high school with their American history and their civics courses and if they don't meet it in high school then we'll have courses that they can take. That'll be for those who didn't make it in high school but were enrolled at the university. That was my scheme. I said, "This just makes things much more rational. This requirement has run its course and there's no need to have it." And I got my committee to come along with me unanimously on this. But we had to bring it to the senate as a whole and there we had a fight and the fight against my proposal was led by the history department in league with ethnic studies.

Those two formed up a strong coalition. They sat together right in the senate when I was introducing this for approval because I reported as chair of the committee to the entire senate. And there was a very big debate on this between the two. We prevailed. The majority for the senate who was present at the time followed the logic that I put forward about the irrelevance of the requirement. But it got pretty heated and there were some—and it got a little bit nasty at certain points. One of the heads of the ethnic studies program had an interview with the *Daily Californian* and the quote came out that I was racially insensitive. I got pulled into that line of debate. Of course I wasn't racially insensitive. It just so happened that it landed in a political situation on the campus in which it lit this fire and that was all of it. I didn't get in any way

discredited. There was no demonstration. It wasn't that public. But nonetheless, it got to be a fight. But it passed.

19-00:12:30

Rubens:

Now, was it reduced to just one requirement? One course? Or was the whole thing eliminated.

19-00:12:37

Smelser:

Reduced to nothing, except for those who had not properly passed a high school course in this area. Well, there were historians who said, "This is watering it down. What's a high school worth? The course is worth nothing." All the academic arguments came up and then there was this subtle kind of affirmative action type or minority argument that came up on it. That was the biggest single moment of mine. Otherwise the Committee on Educational Policy, we did a whole—we met frequently. I think we met every few weeks and we went through a lot of routine material that the senate was doing and that's the important work. And, of course, I would meet with the head of the senate as head of that committee. All the committees would meet together periodically, maybe once a month, and sometimes—and we'd meet with the chancellor. So I got involved in the larger university scene pretty thoroughly by virtue of that one year assignment on the Committee on Educational Policy.

19-00:13:48

McIntosh:

At Berkeley now we have an American Cultures requirement.

19-00:13:51

Smelser:

That comes up later and I played a—the later eighties. As a matter of fact, our killing of that American history, institutions requirement is intimately connected with the subsequent effort to get an American Cultures requirement, which I opposed at the time. But maybe we'll get into it in more detail in the later eighties. But that was intimately connected. The American Cultures requirement was intimately connected with this idea of requirements in general and what do we do about American history. It opened the idea of minorities and gender and spokespeople and white male establishment, the great fight that—

19-00:14:31

Rubens:

And this was after decade ferment over affirmative action.

19-00:14:36

Smelser:

Well, it came with the cultural turn, with the multiculturalism and following on the deconstructionist movement. It was the cultural manifestation of the affirmative action movement that appeared in great strength in the late eighties and manifested itself with the attack on the Stanford Western Civilization requirement and the initiative to get an American Cultures requirement on this campus.

- 19-00:15:03
Rubens: And then history and ethnic studies will no longer be the allies that they were.
- 19-00:15:11
Smelser: No, no, no. The breakdown was completely different in that one.
- 19-00:15:13
Rubens: They were really at each other. Yeah. Is it appropriate here to talk a little bit about Michael Heyman? I know he'll figure more prominently into your story later, but he becomes chancellor in 1980 and he comes in with a real mandate, or at least a strong view of what he wants.
- 19-00:15:33
Smelser: Strong view of affirmative action. Yes. I was going to get onto that because I had a real clash with Heyman on this very issue about the time of the American Cultures issue.
- 19-00:15:44
Rubens: All right. Do you want to wait for that then?
- 19-00:15:46
Smelser: Well, I think so. Because that's a concise episode and it was my only real fight with Heyman during his whole chancellorship.
- 19-00:15:59
Rubens: But you must have known who he was. Were you pleased to see him come in as chancellor?
- 19-00:16:04
Smelser: We knew a lot about each other because of the sixties. He sat on a student disciplinary committee at the same moment and he came the year after I did. He asked me over to his home the first year he came in 1959. So we had been kind of kinsmen all the way through. And it was on this affirmative action matter that he and I clashed.
- 19-00:16:35
Rubens: When he came in, you think that was a good choice? Were you looking for—
- 19-00:16:38
Smelser: When he came in, it was very interesting. I was still in Europe at the time, I believe. Or I had just gotten back. The *Daily Californian* had reported the people who were in the running for the chancellorship and it listed Heyman and it listed me and it listed two other people. So there was—
- 19-00:16:55
Rubens: Did you know that? That you were in the running?
- 19-00:16:58
Smelser: I learned it by reading the paper. I apparently didn't come close.

19-00:17:02

Rubens: Okay, interesting. But you felt it was a good choice? You were looking forward to his administration?

19-00:17:09

Smelser: Oh, I was close to Heyman. I thought he was a very good choice. He's a smart man. Knowledgeable. I thought his background in law might be a little narrow for the general tenor of the campus but that was never an issue. That was just kind of a little thought on my part.

19-00:17:35

Rubens: Okay. So really I derailed you but we were talking about your work on the educational policy committee and this is preparing the way for your selection as chair.

19-00:17:48

Smelser: Well, yes. Apparently my work on the educational policy committee was such that I became then a figure in the senate and I would say without having served in that role I probably wouldn't have been selected as chair of the division.

19-00:18:09

Rubens: How are you literally selected? I don't know that process.

19-00:18:11

Smelser: I don't know. There's some kind of committee on committees and I believe they were the ones who processed various nominations that came in. It was all a mystery. All I knew was I was asked. And I said, "Okay. This sounds like a good idea."

19-00:18:28

Rubens: And it's two year tenure?

19-00:18:33

Smelser: It was for two years. Let's see. Heyman came in 1980.

19-00:18:49

Rubens: And you were chair of the senate for the whole two years.

19-00:18:52

Smelser: Oh, that's most interesting. Well, I came in as chair of the division. There wasn't a whole lot going on in the senate when I first came in. As a matter of fact, it was kind of a somewhat sleepy body and I had trouble with getting quorums. And a quorum I think was seventy-five. That's a very tiny quorum. But fewer would show up for that and so we didn't have a quorum for the business of the senate, because we had to approve things. The senate had to approve the naming of a new unit, approve a new educational program coming up, policies that the graduate council made—so you had a lot—

19-00:19:39

Rubens: And issues of tenure, right, and promotion.

19-00:19:42

Smelser:

Oh, that was all in budget committee. The senate as a whole didn't have anything—

19-00:19:50

Rubens:

They didn't have to then approve what the budget committee and—

19-00:19:53

Smelser:

No. Selected issues. There was a constitutional body on that, of all the things the senate has to approve and most of them are routine. However, without a quorum you can't do it. So here I was really having nothing to lead. So I invented a procedure all on my own. So I said, "We do not have a quorum here but we have a lot of business to do." I would say this before the senate. I said, "Let's do the business. Let's pretend we have a quorum and the next time we have a quorum I will report on what we did here today and ask for approval of what we did." And that worked. So I was able to keep the thing going. We did get quorums from time to time when people would show up and they would always approve what we had done when we had fifty people there or whatever. And so that was my way of keeping the senate alive.

19-00:20:54

Rubens:

How often are you holding meetings?

19-00:20:56

Smelser:

I think the general meetings were held about four times a year. Now I think they're probably less often of the whole senate. And we didn't have any of these great crises. Except for one. It was when Jeane Kirkpatrick, who had been Secretary of State under Reagan came to the campus to give the Jefferson lectures. And this was on some aspect of American history or foreign policy or so on. And there was a group called SAINTES, S-A-I-N-T-E-S, Students Against Involvement in El Salvador. And it, for some reason, managed to get a group of supporters, of kind of militant student activists on it and they went to her first lecture and they disrupted that. She couldn't finish it. They were yelling and shouting and so on. And, in fact, they were able to leave before being noticed or apprehended. So the campus was kind of caught in an embarrassing position of having had a disruption and she was infuriated with the whole thing. Canceled her second lecture entirely. And so that was one of these huge fights. The question is what are we going to do about this. It was a serious incident on the campus. The police had been caught unawares and so had we.

And what I decided the senate should do was really to take a principled stand on this matter and declare that disruption a breach of academic freedom, that this was an interruption of a regularly scheduled event on the campus, and it was—we didn't call it illegal—it was just illegitimate. And I had to get the Committee on Academic Freedom to approve this. Well, the Committee on Academic Freedom was a questionable body at that time because it was one of the bodies that had put a couple of students on it as a result of the pressures of

the sixties and seventies to include students in the governance. The senate didn't respond very extensively to that movement, student involvement in the senate, and there was naturally no question that they would be on things like the budget committee or the really important committees or the senate policy committee. The Senate simply was guarding its privileges here. But on the Committee on Academic Freedom there were a couple of students. I think it was four or two or something like that. And the students wouldn't go along with this resolution that we prepared declaring it a breach of academic freedom. So I had to work very closely with that Committee on Academic Freedom to pass a vote or resolution that would later come to the Senate. I wasn't sure what that Committee on Academic Freedom was going to do. So I had to do a lot of behind the scenes political activity with the chair of the Committee on Academic Freedom to make sure that it came up with the proper resolution that the senate could approve. And I did. I was able to do this. The students didn't vote for it but we had a majority. It was a vote, right, and then the academic senate, even though there was the stirrings of dissent—it was anti-Reagan, really anti-Kirkpatrick, anti-Right feelings in the Senate that grumbled. "Why are we calling this a disruption?" The political sentiments were so strong that they thought it was worthwhile or legitimate to break up meetings of these people who were doing wrong in the world. So there was a debate on it but it passed the whole senate pretty handily.

19-00:25:02

Rubens:

And in your mind, this was an order of protest that was different than picketing or mass demonstrations?

19-00:25:11

Smelser:

Well, disruption. This was a clear disruption of an event, right. She couldn't continue. It could have been someone on the left, as far as I was concerned. But we had taken a procedural academic freedom approach to it rather than—and removed ourselves, the senate removed itself, from the content of the politics because nobody there liked Kirkpatrick very much and almost nobody liked Reagan very much. But nonetheless, we said this is a matter in which the campus has to take a stand. It was a procedural and principled matter of academic freedom. On that one I'd say we won on that issue, because it was controversial.

In the meantime, it caused a stir amongst the regents, as well, and it caused a stir with Ronald Reagan. Because he had a long history of antagonism towards the campus. He was close to Jeane Kirkpatrick. She was his cabinet member. And he began making noises about it and there were editorials, I think, in the *Wall Street Journal* and around the country about this shame of disrupting the Secretary of State and so on. So it spilled over and Glenn Campbell was close to Reagan. And Glenn Campbell was this very right wing member of the Board of Regents. It was his term as chair. They have somewhat revolving—it was when he was chair of the board of regents. Long-time member of the board of regents and one of the core conservative

members. I got to know Campbell when I served on the regents later. I never had a fight with him but it was a very tense relationship because his politics were so far to the right. In any rate, he got the regents to make an inquiry into this and he had a resolution before the regents to demand that Heyman find out who did this and punish them and indicate what procedures he was taking so that it wouldn't happen again and really made this a point of honor for Heyman. And Heyman was really under a somewhat blistering attack from the rightwing of the board of regents. He brought this resolution before the whole board after the—several months after the Kirkpatrick incident had occurred. Since I had been so active in the senate in getting this resolution passed, Heyman asked me to come with him to the regents meeting.

19-00:27:55

Rubens:

To make his report.

19-00:27:56

Smelser:

I sat by his side. And it was a tumultuous and almost impossible—how can they ask him to go find these people when they were completely without—

19-00:28:07

Rubens:

So it wasn't a registered student organization? It was an ad hoc—

19-00:28:13

Smelser:

It was a group that we didn't know. We said it was done by this group called SAINTES but it was a somewhat evanescent kind of group and we couldn't put our fingers on anyone. They couldn't put their fingers on anybody and the police didn't have any evidence. As a matter of fact, the police were very much in presence at the meeting in which we got that academic freedom committee's resolution passed. I worked with the police as to make sure that our senate meeting was not disrupted. Because this would be a natural target because we were coming out against what they did.

We did not have a disruption but we had a lot of police around. And so that was another point I wanted to make about being involved in the higher Senate. I reached out and interacted with all kinds of new bodies. Heyman was really upset by this and I basically was sitting there trying to keep him calm. He was so mad that Campbell had done this. And as it turned out, Heyman got up and said, "Look, we're running the campus. You don't like the way I'm running the campus, would you please fire me. Otherwise, we're going to handle this in our campus way." Basically he was telling them it was an illegitimate thing for the regents to be doing. In fact, Campbell didn't have a majority. So the resolution did not pass and Heyman was not asked to resign. Basically he volunteered to resign, or he said he would resign, if they passed it. And that kind of cemented my relationship with Mike because I wouldn't say that I—I had an advisory relationship with the Heyman administration that reached a kind of high point in the—

19-00:30:14

Rubens: With the taskforce.

19-00:30:15

Smelser: No. Well, the taskforce on education. That was a little bit of tension with Heyman, actually, though it wasn't a fight. But I had it in connection with the divestment.

19-00:30:24

Rubens: So the politics of the regents would change, because wasn't it chair of the regents who led the campaign to divest?

19-00:30:43

Smelser: Oh, yes. Well, actually, it was Governor Deukmejian who led the campaign for divestment in South Africa. And Heyman was for that. But Gardner was against it. And one of the regents, Hennessey, was strongly for it. But I think that episode about the divestment audit was later because I was chair of the system wide senate at the time.

19-00:31:16

Rubens: So we'll get to that.

19-00:31:17

Smelser: We'll get to that one. The Kirkpatrick episode was a moment of greatest closeness with Heyman when I went with him to the regents meeting. We were on the same page on that issue completely and so were much—

19-00:31:34

Rubens: I was thinking of Yori Wada earlier who was chair of the regents 1983 to 1984.

19-00:31:39

Smelser: He was one of the more liberal regents, and he advocated divestment. It was really Deukmejian who called the tune on that, the divestment, as part of his larger California politics and his campaign against Bradley of Los Angeles for the governorship next time around. Because he jumped on board for divestment as the regent.

19-00:32:09

McIntosh: Now, I just have a couple of questions about the Kirkpatrick episode, just to clarify a few things. Do you know why Kirkpatrick was even invited to campus to begin with?

19-00:32:19

Smelser: Well, there's a committee on the Jefferson lectures. It's kind of like any of these lectures series. And it's an honorific. And they invite a variety of people to come and she was secretary of state at the time. It would be interesting to me to know the inner dynamics of that committee as to why they chose her rather than someone else. My sense was that you get a committee of that sort, your question is a good one because Kirkpatrick wouldn't have been one of the candidates who would come up because of her own politics.

19-00:32:51

Rubens:

Right. Having had the experience with George Ball back in the seventies.

19-00:32:54

Smelser:

She was a Reagan person and the campus didn't like Reagan at all from way back, coming from the sixties. They didn't like his national politics in the eighties. And so your question is right. I don't know the dynamics. Perhaps there was a special composition of the board, of that committee, I mean, the Jefferson lectures committee. Perhaps they had had a series of liberal speakers for years and they felt under constraint to have some "balance". I have no idea. Those were my speculations.

19-00:33:27

McIntosh:

Then I have a couple of specific ones about your role during this episode. You mentioned your negotiations with the Committee for Academic Freedom. Did you have any interactions with the students who were on that committee?

19-00:33:42

Smelser:

No. I worked with the chair only. But I was very direct and very outspoken with him.

19-00:33:50

McIntosh:

And then you didn't have any interaction with Kirkpatrick yourself, did you?

19-00:33:54

Smelser:

No. No, I didn't go to the lectures. Didn't witness the event. Certainly became familiar with what the event was but I didn't go.

The other thing that I did here as chair of the Berkeley division, I was a member of the Academic Council system wide. The Academic Council is made up of the chairs of each division plus the heads of the major committees of system wide, like educational policy, like academic personnel, like faculty welfare. That's what the council is made up of. So I went and became an active member of the Academic Council. Well, it met at the time of the regents meetings. It met once a month. And it was where the regents meetings were because the president always made an appearance before the Academic Council and some vice president or some other member of the system wide administration, depending on the issue that we were considering. So it made sense that the Academic Council should meet before the regents meeting because of its involvement with the system wide administration. So that is where I became kind of noticed, at the level of the system wide, because it was only one year after I had served as chair of the Berkeley division that I was asked to be chair of the Academic Assembly and Academic Council.

19-00:35:43

Rubens:

That's 1985 and 1987.

19-00:35:45

Smelser:

1985 to '87. I may as well talk about that now, since it's on the same theme. You had two years with the Academic Council. You were vice chair with the

outgoing chair. This was Marjorie Caserio from Irvine. And she was there and I was her vice chair. So I was very active as vice chair in it and she and I went to the regents together. Both the vice chair and the chair go to the regents meetings every month, right, as non-voting members. I had just finished chairing this lower division report and it came out in my first term as the vice chair. And this was a very fortuitous moment because some of the recommendations that we called for in that lower division report, particularly the encouragement of increased transfer students—which had fallen precipitously at that time—and the development of a common core curriculum that would ease transfer. That is, say, to specify and come to agreement with the community colleges and the state universities on a common core of courses that would serve to meet the general educational requirements. My commission on lower division came up with this recommendation and there I was, present and asked to work with the state colleges and universities and the community colleges to hammer out a thing called the Intersegmental Committee. That's what it is. It's a multi-segment committee.

19-00:37:44

Rubens:

But I think it was composed of members from volunteering institutions and had just begun right around that period.

19-00:37:48

Smelser:

I'm not sure what the history of it was. It was a very weird committee. I didn't understand half the things that were going on and I really didn't feel very much at home but I was given a leadership role in getting passed this idea of a common core curriculum that would serve to facilitate transfer from community colleges and state universities into the university.

19-00:38:14

Rubens:

You're on that committee in '86. So am I right that that's when the report comes out?

19-00:38:22

Smelser:

Yes. Kind of came to the attention of the system wide academic senate and then that was one of the main things I did as vice chair. Marjorie Caserio and I worked well together. She was a life scientist from the Irvine campus. She was an extremely competent woman and she did a very good job in chairing this committee. But I say woman because it showed up in connection with our role in the regents. Because we would go and I sort of felt personally that during my first year with the Academic Council I should defer to her in the regents because she was chair. Whenever we were called upon in the regents meetings to say something or comment on something before the regents—we were pretty active in the regent's discussions. It was a very real participation that we had. However, she sensed, and I think she was right, that some of the older male regents really had, either conscious or unconscious, sexist attitudes. And they began pointing their questions toward me, even though I was vice chair. And she saw that and she and I talked about it quite a bit. She

was sort of complaining. She did a certain amount of fretting in private and I was a bit of a therapist for her. We never had any conflict over this issue but she saw it and she felt, “Well, this is what happens to women,” right. That was basically her comment. But she was kind of hushed. Never made it public. It was all just something that unfolded in the course of the regents meetings and she accepted it philosophically in a way that she didn’t make an issue of it. But she was hurt. She didn’t blame me for being called on. She blamed it on the regents.

19-00:40:45
Rubens:

This is before the UC system had a female chancellor?

19-00:40:50
Smelser:

No, no. Those chancellors were chosen in the second year of my—the woman at Santa Barbara, Barbara Euling and then the Riverside campus chose a woman chancellor at the same time. There were four chancellors chosen in that second year that I was there. But we worked very well together at the council. She chaired the Academic Council meeting, the Academic Assembly meetings, and she did a very, very good job.

19-00:41:23
Rubens:

There were certainly women on the board of regents.

19-00:41:25
Smelser:

Yes. But this was these outspoken male regents. They were sort of still, even though I couldn’t say they were running it, they were the more aggressive talkers. In kind of a traditional male like mode. I thought she was right. And I didn’t bring it up either as an issue to the senate but it was a very interesting kind of undercurrent of tension at the time. The following year, I had Schwartz, what’s his first name [Murray], who was my vice chair, a lawyer from the University of California at Los Angeles. He was no shrinking violet. But we had a more traditional relationship. He deferred to me because he was vice chair and I was chair, although he did not hesitate to speak and I would often on certain issues say that he was the person they should—really more qualified to talk about this than I.

Working with the regents was quite interesting. There is a certain kind of logic for a fact that a non-voting temporary member would end up being a somewhat second class person. You’re not a full regent. You’re not appointed by the governor. You don’t have a twelve year term. You come and you go. And it was not controversial that we were there. The regents generally thought the presence of faculty was important. But I never got the feeling that there was anything but fully equal participation. And I got to know quite a few of the individual regents, just in the comings and goings in the hall and there were these bacj-and-forth discussions and full respect I thought was granted. I was helped by the fact that after the commission report on the lower division came out, David Gardner scheduled two extended portions of a regents

meeting on this report. I was front and center there reporting on it to the regents and having the discussions that went on at length.

19-00:44:13

Rubens: While you also had this position?

19-00:44:15

Smelser: I was vice chair at the same time. All those things kind of came together, and really enhanced my influence and my presence, you might say, on the board of regents.

19-00:44:27

McIntosh: Were there any regents with whom you were particularly close or with whom you—

19-00:44:32

Smelser: Well, I had a good relationship with Vilma Martinez. I liked her a lot. I kind of kept my distance from the really conservative members. Campbell knew me and I had a civil relationship with him but he was always lecturing me and blowing off his political views and I didn't want to start a fight with Campbell. Why should I? This was usually in a cab going to the airport or on some informal occasion. But I had kind of a friendly conversation. Brophy, Roy Brophy, was one with whom I had quite a close relationship. He sought me out. I invited him to speak on the Berkeley campus at one time. So we had a more than usual friendly relationship. We happened to sit on the plane while going to the Citrus Bowl later when I was invited. After I had my athletics report, I got invited to the Citrus Bowl. So Brophy and I mixed it up there. This was after I had been on the regents. But nonetheless, he was one of them that I had a relationship with. I didn't have any uncivil relationships with any of the regents.

19-00:45:47

Rubens: To reiterate, you're on the board of regents by dint of being head of the—or first vice chair and then head of the Academic Council.

19-00:45:56

Smelser: Yes. It's automatic. It was just a part of the job.

19-00:45:59

Rubens: Now, you told us off camera that you wanted to talk about your "cups" and what you did at meetings.

19-00:46:04

Smelser: Oh, yes, yes. This is something I haven't mentioned. I had in my life, beginning farther back than I even can remember, a habit of doodling on styrofoam cups. I have a bunch of them in the cabinet in there. These were symmetrical multicolored designs, often reflecting designs from American Indian culture, which is a part of my own personal Arizona background. I would make these cups and they turned out to be actually kind of impressive once you saw them in the final form. None of them was ever the same. And

they always attracted the interest of everybody else. I did it only in committee meetings. They always attracted the interest of other people in the committee who were themselves often bored and watching me do this. It took me about four hours to do any given cup. So in a two day meeting, I could make, say, three of them or something like that.

19-00:47:15

Rubens:

Did you do this on the Berkeley campus with—

19-00:47:18

Smelser:

Oh, yes. I did it in orals committees that I was on. I'd give them to the students who passed the orals as a gift, as a kind of congratulatory gift for them. And then in senate meetings. And then, of course, my productivity on these cups went up greatly when the meetings were boring and a lot of these regents meetings were completely boring. They were rubber stamping things that had been done in committees or talking. It just didn't command my attention because it was this parliamentary stuff going on. So I would do these cups. And I also developed a habit of giving these cups away, not just to my students in orals but to someone, practically anybody who flattered me about how nice they were. I would give them a cup and I would put my signature in the inside as though it was some kind of art object. Well, the board of regents, I was a big producer of cups because of the pace of the meetings. I made the mistake, as it turned out made the mistake, of giving Vilma Martinez a cup publicly.

19-00:48:35

Rubens:

She was the first Hispanic appointment?

19-00:48:37

Smelser:

Yes, yes. She was. And she was one of the liberal regents and a very sparkling, alive, vital person. And I took a liking to her. I gave her one of these cups in full view of the rest of the regents. And then the other regents began lining up wanting a cup. So I have this institution of giving the cups to the regents. It became a kind of source of humor. They'd comment on it and they'd laugh about it and so on. So I practically supplied every regent with one of these fancy, decorated, multi-colored cups.

19-00:49:19

McIntosh:

It's interesting. I thought this story was going to be that they saw you give Vilma a cup and then they were going to accuse you of having some sort of favoritism.

19-00:49:28

Smelser:

Could have gone that way but it turned into lust rather than favoritism. She wanted it.

19-00:49:36

Rubens:

No one at a Berkeley meeting ever said, "Hey, knock it off, Neil," or, "Aren't you paying attention?"

19-00:49:41

Smelser:

There was one incident that gave me a lot of pause about whether I should do it. Most of the time the students in the oral examination were just as happy as larks to get it and have a souvenir of their orals exam. There was one woman graduate student who was particularly uneasy and discombobulated in her orals exam. In any event, she lacked confidence and was having trouble with her performance. She, even though I gave her a cup, she complained that I wasn't paying attention and that got to me. And I said, "Should I give this up?" As it turned out, I didn't give it up. I said, "This is one case out of thirty or forty of the orals examinations that I was on." But people were mostly curious about it and interested and would tease me about the progress I was making.

19-00:50:38

Rubens:

Were you a doodler back in college?

19-00:50:45

Smelser:

No, I wasn't. Not a doodler. And I do not know how that started.

19-00:50:52

Rubens:

Sharin's influence at all, who of course is an accomplished artist?

19-00:50:53

Smelser:

No, it was before her time. I began before we were married for sure. I once gave one away to a committee member at a national meeting at the Social Science Research Council.

19-00:51:04

Rubens:

Oh, you did it there, too?

19-00:51:06

Smelser:

Yeah. Every place. National Academy is full of these cups because I give them away to people. Anyway, this guy I gave it to was the chairman of the SSRC at the time, president of the SSRC. And his wife was a potter and he took it and gave it to her and she transferred the design onto clay and baked it and sent it to me. So I have a full cup. It turned out to be an interesting—because I go to these past meetings of academic chairs or I go to these regents' dinners. Still invited every couple of years. They say, "All new cups." They don't even care about the rest of my career. They talk about—[laughter].

19-00:51:48

Rubens:

How long did you keep that up?

19-00:51:51

Smelser:

Up until my meeting—I don't do it anymore because I don't have many meetings. But I still do it and people have asked me for them. I give it to them and so on. Sort of trademark.

- 19-00:52:00
Rubens: We'll have to include an image of one in the finished volume of these interviews.
- 19-00:52:01
Smelser: I wanted to get it into my oral history.
- 19-00:52:02
Rubens: That's great. Very un- expected, unusual.
- 19-00:52:15
Smelser: I should say that I developed a beautiful working relationship with David Gardner during this period because he was president.
- 19-00:52:28
Rubens: We'll pick him up when we do the—
- 19-00:52:31
Smelser: Okay, that's all right.
- 19-00:52:34
Rubens: We did neglect your review of UC Press in 1979.
- 19-00:52:44
Smelser: Yes, I was coming to that.
- [Begin Audio File 20]
- 20-00:00:00
Smelser: Regarding the cups, someone got the brilliant idea that they should auction off one of these cups at the fundraiser for Head-Royce. So they took bids on it. I consider them basically worthless. They asked me what they were worth and I said, "Ten cents." This one went for \$500 and then the following year it went for \$1,300. It wasn't my money. It went to the school.
- 20-00:00:31
Rubens: They're really wonderful. I can see that the styrofoam kind of absorbed the ink.
- 20-00:00:36
Smelser: I always advised people not to keep them in the sun because they would fade. But other than that it was permanent. Permanent object.
- 20-00:00:48
Rubens: So we'll resume our chronology. We'll wait for David Gardner but should we pick up the UC Press review?
- 20-00:01:02
Smelser: Yes. Yes. One of the things that happened to me in the eighties was that— interestingly, this was between age fifty and sixty. 1980 and 1990. That was when all this activity took place. And I did an awful lot of work independently of these official senate committees for the university, the campus and system

wide. And the first of these committee reports was to chair, I was always being asked to chair these things, a first review that the University of California Press had ever received in I think maybe eighty, ninety years of its existence. It had never been fully reviewed. And so they asked me, along with a system wide committee, to take a look at the press and its publication policies and the issues and how it made its decisions and the role of the director's board and just a whole—and how they chose manuscripts and what their academic emphases were, how about the balance between journals and books, how about the balance between scholarly monographs and what they call trade—not trade but coffee table type books. So we had a very wide charge and we actually took it very seriously.

We interviewed. We contacted a sample of scholars in many, many different fields around the country that the committee was able to pull together to try to get a sense of the reputation of UC Press in the scholarly world. We knew it had a high prestige but we didn't know who its main competitors were necessarily. We had an idea but we wanted to get some empirical basis for this. We contacted a lot of authors about their past experience with the UC Press and delays and editorial policy and whether or not they felt that UC Press enhanced or interfered negatively with the books and the process and so on. We undertook something more than the usual committee review of the organization.

One of the interesting features of it was that the staff person from systemwide—this was a systemwide committee because the UC Press reports to the vice provost on academic affairs. So he was the one who constituted the committee and he gave a full staff support. And this happened to be Lynne Withey. Jim Clark spotted on her work on this committee and stole her away from a system wide office as his assistant.

20-00:04:08

Rubens:

Jim Clark was the head of UC Press.

20-00:04:09

Smelser:

Jim Clark was the head of UC Press. He brought Lynne Withey in to do editorial work. She had a PhD in history. She was a good editor. And then she moved her way up to the number-two position in the press and then became the director. I always teased her about finding her and being responsible for her career. An interesting—

20-00:04:32

Rubens:

But she had been in the provost's office?

20-00:04:35

Smelser:

She had been in the systemwide office. I think she maybe had some teaching in her career of American history. She was a student of Bob Middlekauff when she got her PhD here. So she is an extremely talented person.

20-00:04:48

Rubens: Was there something about '79 that prompted this review? Was there some—

20-00:04:54

Smelser: I do not know the origin. I don't know—Clark had been there awhile. I think Clark probably was responsible for my being the chair of it, because Clark and I were very close.

20-00:05:12

Rubens: How did that—

20-00:05:12

Smelser: When I first arrived on the Berkeley campus, he was the very first publisher's representative that ever called on me. He was working at Harcourt at the time, I think, and he'd gotten wind that I was a young scholar. These were the days when scholars were in demand and the presses were looking around for people all the time. Buying them dinners and taking them out and seeing what they could do for them. So Clark came to see me in South Hall maybe two weeks after I joined the faculty. He dropped in and we immediately hit it off on a personal basis. And he signed me up for all sorts of things. He was the one who organized my role with Prentice Hall as their series editor. He influenced me to write a couple of books that Prentice Hall published. And then he went off to other assignments. He was a successful publisher and then was chosen as head of UC press. And he called on me all the time. I reviewed all kinds of books for them.

20-00:06:22

Rubens: But you were not officially on the press—

20-00:06:24

Smelser: On the board? Not on editorial. No, I never served on the editorial board, though I read manuscripts. Dozens and dozens of manuscripts for the press. I was one of the regular readers in sociology and in higher education. These are the areas in which I read. So Clark, I have a feeling, had confidence that I would be both a sympathetic and a good head of this review committee.

20-00:06:47

Rubens: And maybe because you weren't on the editorial board. They wanted someone outside?

20-00:06:51

Smelser: Maybe. I don't know. I think it probably wouldn't have been right to have an editorial board member chairing this review committee because you're a little too intimate.

20-00:06:59

Rubens: Did you ever aspire to be on the editorial board?

20-00:07:02
Smelser:

Oh, I was asked and I said no. The same reason I didn't go on the budget committee. I was too busy. Too much. A lot going on. I read a lot for them. That was just because I just obliged my friend. Anyway, it was a thorough report. Got very well received and Clark actually took some initiatives that the report picked up. I've always considered myself to be a conscience of the UC Press because under times of hardship and under times of declining subsidy, the Press is always tempted to go in the direction of popular books. To put pressure on young scholars who have written a highly obscure dissertation to make it more readable, to bring it to a wider audience, to, you know, whatever. And they have stopped short, though now they may even break this rule, they stopped short of publishing textbooks and didn't compete with the commercial market. I don't think they could because they don't have a sales force or the promotional capacity. But nonetheless, the pressure's always been to go in the direction of the more popular books and the culture—I witnessed this when I was later on the board of control at the press, which is now their board of directors. That was in the nineties. I served several years there.

They also got the idea that they would count the books that would be reviewed in the *New York Review of Books*. They'd count the books the *New York Times*—they'd count the prizes. And the glitz began to take—I said, “Keep in mind what you're doing here.” This is going in a different direction and, in a way, compromising your historical mission of publishing scholarly works even though they're only going to sell three to five hundred copies. This is your role and it's very much tied up in the career patterns of young faculty because they're judged on the publication of these books often, these prestigious university presses. So this was a note that I kept pushing all the time. Another way was to argue that they shouldn't compromise in taking on the publication of journals.

20-00:09:38
Rubens:

That's been fairly recent, hasn't it?

20-00:09:42
Smelser:

Ten or fifteen years, I suppose.

20-00:09:46
Rubens:

Rapidly.

20-00:09:47
Smelser:

Yes, they're moneymakers. So they got big time in competition with Sage Publications and other universities—University of Chicago Press—in publishing scholarly journals because you've got a captive audience of libraries and of membership in the professional associations from which it's being published. So they were getting into that competition. But I was saying, “You've really got to exercise the same kind of review responsibilities for these journals rather than just take them,” because they're not all worthy.

There are too many journals in this world of academia. But the idea is if they sell enough to make money, we'll take them.

20-00:10:29

Rubens: They let go of the *Oral History Review*.

20-00:10:29

Smelser: Did they?

20-00:10:30

Rubens: It's at Oxford now.

20-00:10:34

Smelser: Well, sure, there are probably tradeoffs when they discover they're not doing too well. They try to shunt them off to other publishers.

20-00:10:41

McIntosh: Well, getting back to the '79 review. Do you remember any of the major conclusions that you all reached?

20-00:10:48

Smelser: Well, we did harp on the mission of the press and Clark was very sympathetic to that. As a matter of fact, he even took an active role in the search for the new president to replace Lynne Withey. And he told me about it.

20-00:11:08

Rubens: Just recently?

20-00:11:11

Smelser: Well, he did it informally behind the scenes. But he called me up. He's still alive. He called me up and he wanted my opinion on this and whether I'd say something about it. I said, "No, go ahead. Just tell them what you want." But I didn't involve—but he wanted to restore the traditional values of the press, which, of course, had to do with all the electronic stuff going on, with all the cost crunches that university presses are having and so on. So this was one of the lines we took.

We also gave some very practical discussions of the review procedures, because they're often—they're just so sleepy and so slow that these editors would—and we actually did a lot of analysis in the report of exactly what alienates authors and what endears authors. This is one of the lines we took. We also wrote an analysis of the place of the university press nationally. It's the only public university press that's in the top half dozen.

20-00:12:18

Rubens: It was then?

20-00:12:20

Smelser: Oh, yes. We simply state straightforward, on the basis of our own knowledge and on the basis of this empirical work we did, here are the five big ones. And

then we also added Johns Hopkins and MIT in quality, but they're more specialized in their—so we didn't put them exactly in the same category as Harvard, Chicago, Berkeley, Princeton, Yale I think were the ones that we—

20-00:12:47

Rubens: Was Oxford not as—

20-00:12:48

Smelser: No, we didn't consider Oxford or Cambridge because they're such mixed presses. They do what university presses do but they also publish cookbooks and the bible. A lot more. So much more diversified and larger. Two institutions which obviously are high prestige but we didn't put them in that category because they didn't exactly fit.

20-00:13:10

Rubens: Was this fun for you?

20-00:13:11

Smelser: I loved it. And one of the thing that Lynne Withey said, a surprise, as we were coming toward the end of it. She said, "You actually wrote that report." I wrote the prose. And she was fully accustomed to the idea that she would draft it as staff. But I just simply wrote it myself. I regard myself as a pretty good writer. So I wrote the whole thing myself and I sort of apologized for taking her job away. But I decided that should be my practice for all of these reports that I did. Perhaps I should talk a little bit about the School of Education.

20-00:14:01

Rubens: Absolutely, yes.

20-00:14:25

Smelser: One more reflection on Jim Clark. I just had a close personal relationship with him. We are so easy and he would call on me. He'd call on me for ticklish jobs. Let me just give you an example now that we're on it. Glenn Seaborg wrote his memoirs. They were long, they were detailed. They had a good deal of this what-I-had-for-breakfast quality about them and Seaborg really wanted them published by the University of California Press. And Clark looked at these massive books and manuscript and sort of smelled them out, saying he didn't think they were probably the kind of thing the press wanted to do. And he sent them to me. He said, "Should we publish these memoirs?" And I read them all over and I said, "Jim, it's going to be very hard for you to make the decision but I don't think you should. I just don't think they have the interest or the quality for UC Press to publish them." And he stuck by this. He decided not to.

20-00:15:40

Rubens: That's not when Ray Colvig came in, to rescue them?

20-00:15:41
Smelser:

Ray Colvig co-authored these with—or was listed at co-author. I talked to Colvig about them. He did a lot of work on this. He was Cal's media person and knew Seaborg pretty well.

But Seaborg, he wrote down everything. He was a sort of paranoid. He wanted to guard his—especially on the Atomic Energy Commission. There wasn't a note he didn't write and he wanted all his notes in the book, so it became long and boring because a lot of it was inconsequential. That's really it. It was well enough written and so on but I just thought this is—just on balance, this probably should not be—

20-00:16:21
Rubens:

And couldn't be edited?

20-00:16:24
Smelser:

I didn't say it was beyond hope. But I gave a definite opinion about it and Clark took that opinion and said, "Can't do it." And Seaborg hit the ceiling. Went over and talked to the president of the university, complaining. He was really, really angry about the whole thing. Finally it was published by the Institute of Governmental Studies. That was the story behind it. He had them published by the Institute of Government—which is a press and they are in published form but it's not the same as UC Press. It's more of a house press on the campus. So it's a little story.

20-00:17:06
Rubens:

It's a good story.

20-00:17:11
Smelser:

Well, now, the School of Education had had this long history of very unhappy academic existence on the Berkeley campus. It had lots of deans. They had lots of difficulty filling the deanship. They were ranked a lot lower nationally than they should have been given the fact that they were on the Berkeley campus. I think at one time in the seventies they were ranked tenth, which was probably maybe even higher than they deserved because of the halo effect of Berkeley. They had been the subject of several severe senate reviews, all of which were negative and complaining about the quality of the institution and its policies and its faculty and everything. And they accumulated during the seventies, several of them. There was this Berger report that we reviewed on the Committee on Educational Policy and commented on it at some length, I remember. So I was kind of familiar with this background. Recently they had had six deans in six years, acting deans, and they couldn't recruit people from outside. They'd try to recruit somebody and they wouldn't make it, wouldn't take it, and they finally recruited a very promising man from the Berkeley campus from the physics department, Robert Karplus, to be the head in 1980. And he submitted a plan for the reorganization and expansion and revitalization of the School of Education. Very ambitious. And I knew Karplus. He is a very smart and very capable man. I think he really meant it.

He was going to throw himself fully into this. He tangled with the administration over how much resources they were going to give him to do this, especially Provost Maslach, under whom the School of Education was. All the professional schools were under Maslach and Maslach had this bristling relationship. So Karplus said, “I won’t do it. If I can’t have the resources to do this, I won’t do it.” And so he backed out. Another crisis, right. And so that’s when Heyman came to the senate. This was not a senate committee. This was an administratively appointed committee. Only three people on it. Me and Eugene Lee from political science and John Wheeler. He was head of the summer sections for years and years. And he was on a major senate committee at the time. So three heads of senate committees.

And we did a very thorough job of looking into the School of Education. We interviewed most of the faculty. We interviewed national figures. We interviewed Maslach. We interviewed people who were writing on the schools of education. We went to Stanford. And so we did an unbelievably thorough—it lasted months. We’d meet at lunch. We had a full staff member, Andrew Jamison, on this.

20-00:20:43

Rubens:

So you were given some resources then.

20-00:20:46

Smelser:

The chancellor’s staff had an administrative committee. But they took three leading senate members, right, and Heyman asked me to be chair of it. And we obviously consulted the past reports, as well, but we began to come up with the same very unhappy picture of the school, its organization, the quality of its scholars, its insufficient links with the rest of the campus, its low standing nationally and we were getting gloomier and gloomier. And in a way, I was the one who reflected the gloominess most of all because I said, “Do we need such a school?” I was the one that raised this question. And I got the acquiescence of the other committee members, though Lee dragged his feet. His father had been some kind of dean of a school of education somewhere in an educational system and somehow or other he had a personal thing about a really radical surgery like this.

And so what we did was we did a diagnosis of the school. Its pattern of growth, its drop in faculty members, its drop in students—we just did a thorough analysis. This was a piece of institutional analysis, and I sort of felt it was almost a kind of sociological study as leading up to what we—we considered thirteen options as to what might be done to improve the school of education and bring it out of the doldrums that it had been in. And one by one we examined exactly what these were and how they would not seem to work with respect to the analysis we had made. It just would only scratch the surface and they wouldn’t work. So we said, “We believe that the School of Education, in its present form, ought not to be the vehicle in which the campus addresses the problem of education.” We called for heavy involvement of

other units and heavy involvement in the pedagogical aspects of it but not the School of Education. So, in effect, it was to say, “Let’s not have it.” This was a bombshell to give this kind of recommendation.

20-00:23:20

Rubens:

You had a great phrase in this report about standing on a razor’s edge.

20-00:23:26

Smelser:

Yes. Well, that was the razor’s edge that Lee represented, really, because razor’s edge was let’s try to patch it up versus let’s not bother with this particular forum. And we leaned over toward the latter. I was the one who pushed us over in the direction of that side of the razor. And that’s what the end was. And I knew how hot this was. So I went to pay a visit to Heyman before it was made public and said, “This is what we are going to say,” all right. And he was really disturbed. He said, “We can’t do this.” He said, “Can you see the state of California standing by and watch us discontinue the only public school of graduate education in a university in northern California?” He said, “We can’t do this.” He gave me a straight political argument of the rage that we would get out of Sacramento if we let this school go. He said, “We can’t do it.”

20-00:24:30

Rubens:

It couldn’t have been transferred to the CSU system?

20-00:24:34

Smelser:

Oh, that would be killing it. It would have been an even worse insult almost. But we didn’t change it. We didn’t change a word on account of Heyman’s obvious discomfort with this. He wasn’t mad, because he, above all, knew. He formed the committee. He was the one who was—so we let it out. The budget committee immediately endorsed it. The big powerful—and they began to say, “Where can we transfer the worthy faculty members elsewhere on the campus?” See, it did have a strong educational psychology wing. It’s the strongest wing of the school—and most of those members could comfortably—a couple of them had joint appointments anyway. Most of those members could comfortably go into the psych department. Well, it wasn’t quite so evident for others in the school, especially those interested in the pedagogy of education. Where would they go? Well, the budget committee was sufficiently enthusiastic about this that they began active looking around for where faculty members might be placed. But that didn’t get anywhere. Heyman decided he was going to go for an aggressive dean who would undertake to try to address the kinds of problems that the school had. And Bernie Gifford was the guy that he appointed.

I think he’d been at Cornell. I’m not sure. Came out. And Gifford and I sort of got to know one another pretty quickly. Gifford turned out to be chosen on the Board of Trustees of Head-Royce School and we overlapped there for a while. We became sort of friendly.

20-00:26:27

Rubens:

Did you help pick him? Did you suggest him?

20-00:26:28

Smelser:

No. I wasn't on the search committee. But the search committee was pretty enthusiastic and he had some brave ideas and he tried some reforms. He told me he didn't like the conclusion of our report but he really liked the analysis and he said he leaned on it a lot in his own efforts. He wanted to get some really solid programs. He wanted to try new mechanisms for unifying the school, which was totally decentralized. He wanted to have some initiatives, getting involved in regular departments in the university, which didn't really quite get anywhere but nonetheless he had ideas of this sort. And the school, I would have to say, improved some under his leadership because he was so aggressive. But nonetheless, its basic flaws I say remain today as part of the story of graduate schools of education. They've got nationally such a really bad name. They are not respected on any campus on which—even when they're superb. I know that the Stanford School of Education and the Harvard School of Education, the two strongest probably, don't have much of a reputation. University of Illinois killed theirs. University of Chicago pretty well discontinued theirs. Pennsylvania was thinking about getting rid of theirs. It's one of these very sad stories that happen. And, of course, I wasn't especially popular with the School of Education faculty.

20-00:27:57

McIntosh:

Well, I wanted to ask if you were obligated to hear out any of their arguments for their existence and—

20-00:28:07

Smelser:

No. Our committee report was there. One of the features of committee reports is that once you submit them, they are totally out of your control. They're now the ownership of whoever appointed you. I made this point in my book on committees. You may as well not try to imagine that you have any ownership of what you did because it's done for somebody else and that's exactly what that was. They just didn't like the idea that it was such a hostile report and I was obviously the guy who was centrally involved in producing it. It got a lot of attention, quite a bit of national attention, I learned informally from conversations, the usual grapevine sources, and still kind of stands around as a diagnosis of the—

20-00:29:00

Rubens:

People can easily read it because you published it in your *Reflections on the University of California at Berkeley*. The report is there.

20-00:29:06

Smelser:

Yes. It was never published before. The chancellor's office didn't do terribly much by way of publicizing it. No, it gets around. It's a public report but it was in mimeograph form always. I decided to publish these reports in my *Reflections on the University of California*. Usually this is a slightly—

20-00:29:29

Rubens: This is a recent publication we should say.

20-00:29:31

Smelser: Yeah, it's a recent publication. I published this report. I published the lower division education report, I published the athletics one. I wanted to publish the review of the University of California Press. And Lynne Withey said she's not quite sure that was the right idea in a book published by the University of California Press. So I said, "Okay. I'll leave this out if I can put in my spoof report on Thanksgiving Day in England." So that's why that appears as the last chapter in that book.

20-00:30:06

Rubens: Bernie Gifford, would you tangle with him later? Because wasn't he a proponent of the American Cultures requirement?

20-00:30:15

Smelser: Didn't tangle with him directly. No, I didn't. I tangled with Heyman on that point.

20-00:30:26

McIntosh: Well, I was just going to propose that we—you've referenced the commission on the lower division a few times already today. So I was really intrigued about—

20-00:30:33

Smelser: We'll talk about that. The Lower Division Commission on Education. In the middle 1980s, three influential reports came out. One from the National Humanities. One was the Bennett report, William Bennett. He was head of the National Humanities Center just before this came out. There was an American Council on Education report and this third one, all of which were absolutely doom and gloom on universities and their responsibilities for general education. And that's what they agreed on. The doom. They didn't agree on reforms. One of them wanted basic skills, one of them wanted to go back to the humanities in a kind of a Hutchins model. That was the Bennett report. And another one wanted to teach breadth and wide coverage. So there's no real agreement on it but they stirred up a very big national discussion and criticism at that time. It was right in the middle of the eighties.

And we were not immune from this, of course, being one of the big major research institutions that were among those that were most attacked for neglecting undergraduates. So David Gardner got the idea of a major examination of the lower division.

20-00:32:07

Rubens: Now, David Gardner had come in in '85 as president of the system and right after he had been the author of a celebrated study on high schools. He authored *A Nation at Risk*.

20-00:32:20
Smelser:

Nation at Risk, yes. I first got to know David—he was going to do a little work on the Free Speech Movement and he came around to interview me even before he went to the University of Utah. And so we knew each other from way back and he had a role similar to mine in the chancellor's office here just after the Free Speech Movement. He had such a role on the Santa Barbara campus. So we were blood brothers in this kind of activist period. I also welcomed David Gardner to the Berkeley campus as chair of the Academic Senate when he was first president. He was at all of the campuses and I was his host here on the Berkeley campus. Well, anyway, he got there in the year that I should head up this—he defined the problem as a lower division problem, which was, I think, correct because that's the seat of the greatest problems of undergraduate education in the University of California. And he asked me to be chair of it. His agents were Cal Moore, who was associate provost in the system wide office and Bill Frazier, who was the provost in the system wide office. They were the ones who were responsible for the more academic side of things. But Gardner wanted me to do it and Gardner put personal pressure on me to do it. I agreed to do it. It turned out to be a very big job.

First of all, you had to have all of the campuses represented. That's already a problem. Then you had to have disciplines, different academic disciplines involved. That's a problem. You couldn't do it without engineer because there's where a lot of the problems of undergraduate instruction in general education occur. And there were considerations of gender, race, balance. It turned out to be a huge committee. Twenty-six or twenty-seven people. I don't remember. It was very big. And we were to come up with as good a report as we could on reforms of the lower division. Big job. And I certainly—

20-00:34:30
Rubens:

And how did they manifest? Why was Gardner—he was responding to the national discourse. But at Berkeley, was there a particular effervescence of—

20-00:34:39
Smelser:

No. He wanted it to be system wide. And the fact that I was at Berkeley—first of all, he had a ton of respect for me because of these past committees. I think maybe a little bit of word was getting around that I was a good report preparer because I already had these two previous reports that had received commendation and wide—some influence. And I think the fact that I was a university professor was very important symbolically, that I be chair of this committee because I am a member of the university wide faculty. I never got to his thinking, or their thinking about that, but I daresay that was one. Well, anyway, the first meeting—

20-00:35:33
Rubens:

Did you pick, by the way, the—

20-00:35:36

Smelser:

None of them. It was entirely chosen without my even consultation. I was handed a committee. And we had our first meeting over in University Hall, as I remember, and I knew that this was going to be a tough committee because of its diversity and because of the diversity of opinions that people might have on it. So I did very advanced homework on this. I have to say that I was probably—of all the things I've ever chaired, with maybe one exception of the national academies committee, I was the most aggressive chair on this committee, largely because of its diversity, because of the controversy surrounding general education, because of the—I would have to say the committee was never regarded as illegitimate but general education in the lower division is not on the top of many faculty members' minds. And so I really felt I had to take more initiative, organizing the agenda of this.

So I presented the first committee meeting—every member of it was there—I presented them with a few principles that I thought we had to observe. At the beginning of the meeting I said, “I just don't believe we can have more than a dozen recommendations because all these reports that come out have fifty-five recommendations and amount to nothing and they don't have any impact at all. They just sit there. And we have to have a limited number of recommendations, tightly developed and tightly defended.” That was the number one rule. Everybody accepted it. and then I began to lay out the areas where I thought—I did this rather than say, “What do people think we should do.” I actually laid down what—it wasn't the final table of contents, obviously, because that committee did real work. But it was kind of closely related to what came out in the end because I had a lot of ideas about general education by this time. I had done this work with the American Political Science Association on the undergraduate major. I'd done this work with Carl Kaysen on the book on teaching social sciences in a general education manner. So it was kind of cool. I was really kind of familiar with some of these things, so I just kind of laid out a large part of the agenda and people were happy enough in passing. There was all discussion, no question about an active discussion, because these were involved people and all highly committed to general education programs and there was a lot of diversity.

20-00:38:19

Rubens:

All of them were then—I was going to ask that, if it was a very engaged committee?

20-00:38:26

Smelser:

They were people who were chosen because of their past histories as good teachers or as advocates.

20-00:38:29

Rubens:

They wouldn't have taken it if they weren't going to—

20-00:38:31

Smelser:

No, no. It was a bit of a thankless job because we met for months and it was a lot of work and staff work and it just took a lot of time.

20-00:38:44

Rubens:

So president's support for staff, and I guess reimbursing people coming to meetings, but otherwise you're not paid for these?

20-00:38:51

Smelser:

No, no. Free work, all members, but we had I think three staff members. David said, "Let's do this royally so we don't cut any corners and so on." But at the end of the first meeting, Cal Moore came up to me. He came to all meetings because he was the key point man at the system wide level. He sat in on all the meetings. He came up to me at the end of the first meeting. He said, "You've got this commission in your hands." He made that observation to me because he had noticed this aggressive—kind of aggressive plan I'd taken. So we met. We met frequently. Matter of fact, I think we met, I can't remember, say seven, eight times over the course of almost a year. We had to write an interim report and that interim report was a—the agenda of the interim report was the agenda of the national reports. It was a response to the national reports and not a defense of the University of California but a citation of a number of the areas of activity and initiative that were going on on the different campuses. Frazer and Moore demanded this to come out in June. I think we were formed in January. This came out in June and then the final report came out in the following fall. And I remember working with the committee on all these points, getting suggestions, getting input, getting ideas and being very, very careful with it. I remember going with my family to our cottage in Twain Harte. We owned a country place that we would go to on weekends a lot when our kids were small. I remember taking that up there and spending one, just one—I didn't do a whole—one weekend and wrote the whole thing. Fifty-page report, pretty much in final form, and submitted it and we discussed that draft at the last meeting of the commission and really got it pushed through relatively—we didn't have any real cleavages in that. It was a very interesting committee in that they were active and they contributed a lot to the thing but I also decided I was going to write this whole thing myself, and in my own style, and, of course, it had to be approved by this commission, and it was. So I—

20-00:41:32

Rubens:

This is 1986?

20-00:41:33

Smelser:

1986. So just when I was finished, I turned it into Frazer and I happened to meet him at a cocktail party about two days later. He was just totally effusive about the report. He had read it. Actually, I was taken by surprise. I didn't expect this kind of immediate reaction. I said, "How can you tell?" That was basically my reaction to the task. "How can you know this?" I think he had such a positive attitude toward it.

The other thing I decided that I told the committee in that first meeting was that this report has to have a lot of analysis. We can't just go in and start talking about recommendations. We've got to get a lot of empirical information on what these students take, how big their classes are, what the role of TAs are and what the role of temporary faculty are. We really have to have our hands on top of things and we have to analyze what the problems are before we come up with any recommendations at all. So that report is just full of analysis. And I've always operated on a fundamental commitment that you don't have any impact if you don't have any analysis. Otherwise they just kind of process these benign recommendations. They don't quite know what to do with it and so on. And it got immediate attention.

20-00:42:59

Rubens:

Were there students on that committee?

20-00:43:03

Smelser:

We had one or two. But not too active. They were from different campuses. They didn't come to all the meetings. Can't blame them, actually. It wasn't exactly in their top priority to spend time on this committee. They were there and they spoke. Whatever they said, we listened. But it got immediate attention. Gardner immediately scheduled these two regents meetings to discuss it. Invited Ernest Boyer to come to this meeting.

20-00:43:38

Rubens:

Boyer had been a major figure in the national discussion.

20-00:43:40

Smelser:

He was writing on general education. He was part of the attack squad. He was one for redefining the whole notion of research, re-emphasize teaching. Forget general education—he was a national figure. He was very popular because he was writing at the time. He put out what became known as the Boyer Report. At various different times, scholarship corrupts education, scholarship and teaching.

So Boyer was there and he and I had an exchange. He was more radical than I am in his viewpoint but nonetheless he and I had an exchange in front of the regents. Then I had an exchange with the state superintendent of public instruction, whose name I will think of later [William Honig]. who was a member of the board of regents by ex officio. And he was kind of taking the line that, "We've got to train these students in the values of American society." He said, "We have to require Tocqueville for all our undergraduates," and so on. And he and I had a long exchange that David Gardner wrote up in his memoirs over different contrasting views of general education. It was really quite an interesting debate that we had. I took the line, "Look, we really don't think we should be in the business of dictating, or the content of values that these students should know about or have. We should expose them to all of them but we should also treat these students with a kind of dignity. Treat them with the capacity to make judgments. That's

what general education is all about.” He was more, “Give them the stuff. Give them the material. Give them the material that will lead them to respect this country of ours.” Right? This was his line and I was arguing more the process that—the liberal view of liberal education and so it just went on.

And the regents let us do it. Usually the regents go from one item—we sat there for forty-five minutes back and forth on this sort of thing. Tom Hayden got hold of it, the radical senator who had been an activist in the sixties. He was by then a member of the California legislature and he said he’d like the report, too, and he said, “Why isn’t the university doing something about it?” He turned it into whipping the university for not attending to what was obviously a valuable set of reforms. So he bent it to his own purpose. But that’s kind of the level of response that it had. And in his memoirs, Gardner listed this as one of his—when he was listing his accomplishments or initiatives—he spent a long time on this report in his own memoirs.

20-00:46:37
Rubens:

Why do you say Boyer was more radical than you?

20-00:46:39
Smelser:

Well, we did not come out with any hostility toward the research function of the university. He said, “Universities have gone cra—they’ve gone awry. They’ve gone wrong. They’re interested in only research—the creative research that scholars do on government money or with research grants and so on, and they’ve neglected undergraduate education.” We ought to recognize that responsible creative research goes into the teaching, of course, and we ought to recognize equally with other kinds of research. And so we never got into that priority. We never got into bashing other things in the university.

I got some criticism from a few colleagues just for this kind of report coming out. And these were kind of the research nuts in my own campus who said, “Look, are you trying to get the faculty—divert the faculty from its research or something?” Meaning we were calling for these reforms and more teaching of undergraduates.

20-00:47:41
Rubens:

Well, you wanted lead faculty to be teaching undergraduate seminars.

20-00:47:44
Smelser:

Well, freshmen/sophomore seminars was the number one recommendation that ultimately has gotten widely implemented in the whole system. That turned out to be popular and faculty liked it. But it wasn’t specific reforms. They said, “You’re undermining what we’re doing.” It was kind of an idea that—it was. They thought the research was a thing and they should be research entrepreneurs. In a way, these were faculty members that I didn’t agree with. We shouldn’t be spending our time with undergraduates. There was a part of the faculty culture that holds that view.

20-00:48:23

Rubens:

What I can't remember in the report is if there was room for student initiatives, for response to student's—

20-00:48:33

Smelser:

We did not include that logic of the Board of Educational Development into our report. We did lead the way in the discussion of diversification and teaching courses on globalization. Internationalization of the curriculum was one of the—we were on that tide.

20-00:48:55

Rubens:

And affirmative action. I think there's a plank in there.

20-00:48:57

Smelser:

Yes. That part of it we were very much concerned with issues of diversity.

20-00:48:59

Rubens:

And why was there a choice against the philosophy of the BED? Not including student initiative?

20-00:49:06

Smelser:

That's a very interesting question. I certainly, as I've indicated in earlier interviews, I do not regard the BED as a very special or influential episode in the history of the university and I was not terribly fond of the student initiative because it had been so politicized during that period. Maybe there was something unconscious going on in my mind about that.

20-00:49:32

Rubens:

Now the Decal classes had already been institutionalized.

20-00:49:39

Smelser:

Yes. We did not get into that, and the American Cultures was not yet in full swing. That was a couple of years, three years later, that that movement really took force. We certainly were in sympathy with it. We were very much interested in the improvement of the quality of TAs. We were interested in senior faculty getting into courses. We were interested in a lot of system wide reforms dealing with transfer. There were only 5,000 students transferring into the university in that year. It had gone down from something like nine thousand.

20-00:50:27

Rubens:

What accounted for that? Do you—

20-00:50:28

Smelser:

Well, we gave a whole lot of reasons in the report, one of which was the limited numbers of students in community colleges who take the preparatory classes. We had a lot of students in community colleges who were not particularly well prepared because there was a heavy minority population in the community college. We focused on procedural difficulties of transferring. That's why we got interested in the core curriculum. We got interested in

administrative facilitation of the students coming in. Not throwing up roadblocks of this particular course requirement, you have to repeat this or that, etc.

20-00:51:07

Rubens:

Heyman was pushing some of that, too, because I know Mac Laetsch was running a seminar out of the Center for Higher Education to work with community college administrators.

20-00:51:20

Smelser:

It was a real problem. And we had made some noise about the compromise of the master plan because of these dwindling numbers. And there were a lot of murmurs in Sacramento about the same thing because, of course, the representatives in Sacramento had big minority constituencies. They've certainly committed to certain populist views as to what the university ought to be doing and it looked as though we were neglecting this aspect of the educational mission. So we attended a lot to that. We attended to the training of TAs, and almost immediately after our report came out, Gardner appropriated some money to improve the training and we also required the English language—improved the English language capacity of TAs instantly.

So this set off quite a few number of reforms. The place where it didn't hit, and you can imagine why this would be the case, was in things that required more faculty initiative. That is to say we got very few takers, as far as I can see, on the recommendation that we get the best teachers in the system and we put them into big freshmen courses. Let the inspiring teachers teach the big course. They don't like that. Lead balloon, that one. And also our ideas about giving a lot more interdisciplinary capstone courses in the upper division and so on. We had a pretty eloquent little section on that. And as far as I can see, that didn't get anywhere either.

20-00:53:02

Rubens:

Capstone meaning do your own research?

20-00:53:04

Smelser:

No, integrative. Integrative thematic courses that would cut across disciplines. Tie things together. That's what capstone usually referred to.

20-00:53:16

Rubens:

In the last five years, the idea of students producing their own research has come to be the mission of a capstone experience—I think.

20-00:53:19

Smelser:

Student research? Maybe it's come to have that name. At the time we were writing it was more or less these advanced synthetic courses that would talk about big ideas and tie things together.

20-00:53:32

Rubens:

Sounds like a great idea. It would be a nice end—a real capstone.

20-00:53:33

Smelser: We got no takers on that.

20-00:53:37

McIntosh: So it sounds like there was support for reform up to the point at which it affected the teachers themselves, right? The faculty themselves?

20-00:53:45

Smelser: I guess I would go that far, yes. I went around after this report was out to several campuses. I was invited because it resonated amongst some faculty members and some senates on other campuses. And I would go down and I would talk to the senate leaders and I would talk to some administrators about this report. It really got a lot of attention and that's the sort of thing you don't always get with committee reports. And what I discovered—that on every campus there is a cadre of really committed teachers. It's a minority. And they don't get as rewarded as they should. But they really welcomed this report. But it wasn't the whole faculty who turned out. It was just this cadre of people who are committed educators.

20-00:54:45

Rubens: I saw that Ken Jowitt was on that.

20-00:54:46

Smelser: Yes, yes, yes.

20-00:54:47

Rubens: And he was such a leading light as a teacher in political science.

20-00:54:52

Smelser: Yes. He and I had a close relationship. He was on my lower division commission, as well. And later athletics. We would joke because his son was in the CPS school. He came up to me and he said to me, "Why don't we just take the CPS curriculum and transfer it up to first year at the university. That'll solve all our problems." So we had a lot of joking about that.

20-00:55:25

Rubens: I wanted to ask you also about an historian at UCLA who was concerned with education. Gary Nash.

20-00:55:30

Smelser: Oh, yes. He was a very responsible member of that committee. Full of ideas. He was really pushing a kind of US education, a standardized US history education, but he did not bring this into our work. Maybe it was before he was especially innovative in that topic. But no, he was generally very responsive and supportive of the committee work and supportive of me in my work with it.

So I'll just have to give you this because it was a direct outcome of this report. Two years later, three years later when the student—when the big initiative over American Cultures came in, I was—Heyman had asked me to serve as

chair of a follow-up committee to this lower division commission on general education in the UC system. I accepted it because I was Heyman's friend, right, and I knew I would be kind of bored because I'd done it all. I'd done this once. I don't like to do things again, a second time. I said, "How much are we going to deviate from these general ideas that I—" but I did it. I served. Then, when an American Cultures debate was going on in the senate, I got up and I spoke out against it and I said, "The aims of this initiative are absolutely admirable and I pointed to our own commission report on this subject of encouraging all the things that were being called for by way of diversity and globalization and other cultures and so on." I said, "It's not quite Berkeley's way to do this."

20-00:57:30
Rubens:

And did you mean the requirement or the—

20-00:57:32
Smelser:

The requirement. I meant the requirement. I said, "I don't want it required. I want it to be initiatives. I want there to be hundreds of initiatives in this area but not a requirement." And I made the prediction that the same thing was going to happen to this requirement that's happened in the American history and institution requirement. I said, "We're going to get multiplication of courses. We're going to get departments and units competing from this because they are bodies and bodies are tied to budgets and chairs know that and it's going to get corrupted." I basically didn't use that word. I believe that was the logic of my own—Heyman, who was so committed to this thing going through, and so much under student pressure—the students on this new committee that I was on went to complain about Heyman [about me] and he asked me to resign. I said, "Thank god." It was in my heart not to do it anyway.

20-00:58:25
Rubens:

Let me just be clear. What was the name of the commission?

20-00:58:28
Smelser:

It was one that Christina Maslach ultimately headed up. The Berkeley one -I can't remember the name. It was something on education. It had a high name on general education at Berkeley or revitalizing student life at Berkeley. Something like that. But Christina Maslach took over and there was a Maslach report came out. I was right. It wasn't that much different from our lower division report in terms of what they were calling for and I suspected that it would be much the same as when I had chaired it. But I didn't take Heyman's request that I resign personally in any way, largely because I was so happy to be out of it. I was—

20-00:59:09
Rubens:

How long did you serve then? It was right at the beginning of its work. But the students on that committee were so committed to an American Cultures requirement that they—

20-00:59:25

Rubens:

This had become part of your strategy of leading commissions, that you were saying from the beginning what some of the outcomes should be?

20-00:59:33

Smelser:

No, I didn't do that with the local—the campus one. I have to say that my heart wasn't too much in it. I was going to do it but I wasn't going to do it with the same passion as it turned out that I was involved in the system wide one. As it was, I had done it before. I didn't like the idea of doing something again. And also, I guess I realized that this was going to be a little bit bloodier, really, politically. Probably some of my distaste for Heyman's assignment came from that, as well, so I was relieved to get out of the fighting because the fighting was pretty severe. But Heyman really didn't like the role I played in that one.

20-01:00:20

Rubens:

Do we have some time left?

20-01:00:20

McIntosh:

Well, the close of that episode is probably going to be the close of the tape, as well.

20-01:00:24

Smelser:

Okay. I think I've said everything I have to say about that.

20-01:00:26

Rubens:

Should we elaborate why you said the Intersegmental Committee was weird?

20-01:00:38

Smelser:

Well, the point is that the interests of community college teachers and the interests of state colleges are so different from the university. Their political issues were different and I found myself—I couldn't resonate with them as a university faculty member. That was my problem, I suppose. But we managed to get through that.

20-01:01:02

Rubens:

Did it keep going, the committee?

20-01:01:06

Smelser:

It's a permanent fixture.

20-01:01:07

Rubens:

I think Karl Pister had a role in creating it. I read that in his oral history.

20-01:01:12

Smelser:

Is that right? Probably when he was chair of the senate. I saw that committee as kind of an octopus. Okay, we're finished for the day.

Interview #11 June 9, 2011

[Begin Audio File 21]

21-00:00:00

Rubens: Neil, hello.

21-00:00:02

Smelser: Good morning.

21-00:00:04

Rubens: You've been away, you're just back.

21-00:00:08

Smelser: From Arizona. Three weeks at the Grand Canyon, in splendid isolation and aesthetic wonder.

21-00:00:14

Rubens: What a great phrase. For one of the first times, being Sharin's spouse at a workshop –she was artist-in-residence .

21-00:00:20

Smelser: I was Sharin's helpmate. They called me the trailing spouse. That's right, and she gave several presentations, and I assisted her in this.

21-00:00:28

Rubens: What was the name of the program?

21-00:00:32

Smelser: It's an artist-in-residency program that several national parks have. It's the sort of thing that the government wouldn't really support, but it's a private association called the Grand Canyon Association, a voluntary organization which you join. They dreamed up the idea of having five artists a year at the North Rim of the Grand Canyon. They stay for three weeks. You live in a little cabin, fifty feet from the edge. You have this magnificent May and June weather. She enjoyed it. It transformed her. It was just a beautiful experience.

21-00:01:09

Rubens: And you got to work on your—

21-00:01:10

Smelser: I worked part-time. We did a lot of hiking.

21-00:01:13

Rubens: But you worked on your—

21-00:01:14

Smelser: Worked on my Clark Kerr lectures.

21-00:01:16

Rubens:

Which we'll get to by the end of these interviews. Speaking of art, we ended our sessions three, four weeks ago with your artistic efforts. We didn't film one of your cups.

21-00:01:31

Smelser:

Here's a sample. My only artistic talent is this cup. It's made with symmetrical designs. It has a Native American influence in design. Every one is different. Here are a couple of others that I did. Sometimes they're monochromatic, occasionally. This is the blue one. I do it according to the pens I have on hand. I always do it in committee meetings only. I don't do it alone.

A lot of people, they know that I'm a scholar and a teacher and everything, but they're most attracted to the idea that I do these cups. Various people throughout the nation have been supplied with these gifts that I've given to other people.

21-00:02:43

Rubens:

I think it was important to show them. That was a good segue. We're here today, I think, to begin talking about your work with the Committee on Basic Research in the Behavioral and Social Sciences, which was under the auspices of—

21-00:03:05

Smelser:

Of the National Academy of Sciences.

21-00:03:06

Rubens:

You were not a member yet?

21-00:03:11

Smelser:

No, no. In a way, this certainly brought me to the attention of the Academy, but I wasn't a member until 1993. I was called upon. They do call upon nonmembers for specific assignments. This was a very interesting decade of service I gave to the National Academy without being a member.

21-00:03:34

Rubens:

Let's begin talking about it. How is it that you are called in 1980 to serve?

21-00:03:40

Smelser:

There was a political background to this. In 1980 and '81, shortly after Ronald Reagan came into office, he had a staff member, a very influential staff member, named David Stockman. Reagan and Stockman got the idea that the social sciences were of no utility and no use. Reagan had spoken of this. As a matter of fact, in some of his public utterances as governor of California, he'd actually picked out sociology as being kind of a mischievous subject, largely because he thought it was full of activists and left-wingers. So he didn't have a positive view of the social sciences in general. Maybe economics. I don't know. There was a move that was initiated early in his administration to undertake really severe budgetary cuttings of the National Science Foundation

and other agencies and the social science budgets. Up to 75 percent cuts were anticipated. They didn't touch genetics, and they didn't touch other kind of health-related life sciences or some branches of social sciences that were more applied and less controversial, but here was this effort. And they meant it. They wanted to go right ahead with it. They were preparing the budget to do these kinds of cuts, including economics.

This created a political momentum within the social sciences of great alarm and political momentum. Actually, they formed an organization called Consortium of Social Science Associations, which both economics and psychology joined. I thought these disciplines might not join. This became a Washington-based lobby. It still exists. It's supported by voluntary funds and many universities, including UC Berkeley, give money to this organization that still occupies a place in Washington as a lobby. COSSA, it's called. That's the acronym. Anyway, as part of this, the National Academies wanted to set up some kind of, you might say counter-move, to demonstrate the utility of the social and behavioral sciences. I had been active earlier in the C-BASS reports in 1968, a long time before, so I was not exactly unknown for being a kind of spokesman for sociology, and, to some degree, for the social sciences in general.

There was a man, Gardner Lindzey. I'd spoken of him before. He was my undergraduate dissertation advisor at Harvard. He was my guardian angel for my entire career. He would alert me to different assignments and he had all kinds of support and friendship for me. He was director of the Center at Stanford for fourteen years, before I was director, and he was always getting me involved. We'll talk about that later. He was the one who appointed me to the special committee of the NRC to deal with the use and utility of the behavioral and social sciences. The chair of that committee was Robert Adams, a very eminent anthropologist, somewhat senior to me, from the University of Chicago. The staff person was Donald Treiman, who had taken leave from a faculty position at UCLA. He was a staff person. I was only a member of this committee. I wasn't its chair.

21-00:07:35

Rubens:

How big was the committee?

21-00:07:37

Smelser:

Oh, I'd say it must have been fifteen to eighteen people. We convened in Washington. The assignment to the committee was to prepare a volume demonstrating, by example, usually, useful applications of the behavioral and social sciences. We talked. We had an initial division in the committee. By division, I mean conflict. One side of it was represented by a man named Peter Rossi, a sociologist from University of Massachusetts in Amherst. Quite incidentally, Rossi had been a member of the oral examination committee on my undergraduate essay at Harvard. He was, in one respect, a big supporter of mine. He pushed my thesis for publication at the University of Chicago Press

after he became aware of it. But he was also an extremely critical man. A very sharp, acerbic sort of guy. He would often attack me in ways that almost stepped over the line of a collegial relationship, so I had a very ambivalent relationship with Rossi. But Rossi became the spokesman. He said, “We want some really applied, sort of survey technique, quantitative, usable examples here.” I actually turned out to be the kind of leader of the second group that opposed Rossie and said, “No, let’s go across the board. Let’s talk about family research. Let’s talk about a wide variety of things. Let’s talk about general perspectives that are useful, rather than simple facts or techniques.” So there was this back-and-forth division.

21-00:09:39

Rubens:

Mainly around these two poles?

21-00:09:41

Smelser:

These were the issues we were fighting about. Rossi was a spokesman for the first, and I turned out to be kind of a spokesman for the second. It turned out the committee went in my direction in terms of the stances that we chose. Rossi was really infuriated with the outcome of this, and he resigned from the committee. He said, “This is not the kind of committee I want to be on.” So he just simply left and someone else came on and took his place. You normally don’t enter into conflicts of that sort. I haven’t been in too many in my whole life. This was one where there’s a sharp political division, and I just—

21-00:10:19

Rubens:

Well, based around real disciplinary and intellectual—

21-00:10:22

Smelser:

This was an intellectual fight and priorities. I was pretty sure what I believed in this case, and so I was able to be articulate and win the support of most of the members.

21-00:10:33

Rubens:

Is it worth mentioning who were on your side or other—

21-00:10:37

Smelser:

Not really. I don’t think I can even remember. The chair, Adams, was clearly on my side. That made a big difference in the outcome. Anyway, I was a member. I was the sociologist in it. I was kind of the main spokesman for sociology. It was disciplinarily organized. There was a very interesting inner history of the committee. As we were beginning to develop, we gave a little vignette of each field. We wrote our own and then I volunteered to write a more synthetic view of what the social behavioral sciences were in general. I wrote this essay. This essay made a very positive impression on the committee and on Adams. During the course of the work with the committee, Adams asked me to be a coauthor and coeditor of the report. It wasn’t in the cards already. He was going to be the editor, and Treiman, the UCLA man, was going to be the drafter and staff person who’d put it all together. That’s the pattern in the National Academy committees. You have a chair and then you

have a staff person from the Academy, engineering the whole thing through. But I kind of rose up through the ranks of the committee in general, and then Adams and Treiman and I went to his Colorado country place, not too far from Aspen, one weekend. We did this drafting and planning and so on.

21-00:12:21

Rubens:

How long had the preparation been? How long had you been meeting?

21-00:12:25

Smelser:

That committee lasted about a year, I think, before we started the writing phase. This was at the writing phase instead of the committee meetings. Interestingly, Adams and Treiman also developed a kind of conflictual relationship. They had a big fight there right at Aspen while the three of us were there. I sort of stepped in as a mediator between them and said, "Look, we're not here to fight. We're here to"—

21-00:12:54

Rubens:

What was the basis of the—

21-00:12:57

Smelser:

It seemed to be kind of personal. It might have been intellectual. But it got sharp. It sort of polarized, and that's when I sort of stepped in as a mediator. Anyway, this book came out.

21-00:13:10

Rubens:

This is 1982.

21-00:13:12

Smelser:

'82. Edited by Bob Adams, myself, and Treiman. It appeared. I went to Washington a couple of times. We talked to different agencies. It got some attention in Washington.

21-00:13:32

Rubens:

This was *Behavioral and Social Science Research: A National Resource*?

21-00:13:36

Smelser:

That's right.

21-00:13:38

Rubens:

And put out by the National Academy? Is that the publisher?

21-00:13:41

Smelser:

Published by the National Academy. It was clearly a political document, meant to counter the initiative of the Reagan administration to cut back. They managed to cut the budgets some. Not nearly to that extent that they had planned. Within five years, they were all back to the normal contours of support. This document played something of a role.

21-00:14:08

Rubens:

That's what I want to hear about. How big was the report?

21-00:14:16

Smelser:

The report was, I'd say, a thin volume, maybe 120 pages.

21-00:14:20

Rubens:

Is there a way of just kind of summarizing what the case was that you made?

21-00:14:23

Smelser:

Yes. First of all, we presented the social and behavioral sciences, describing each one and its essence as best we could in a short period of time. Then we went to a series of highly focused examples of the usefulness. I can remember talking about family stability. I can remember a little section on alienation of labor. I remember a section on demography. There was one on the nature of formal organizations and the vulnerabilities of formal organizations. These were for substantive topics. We just presented them as being useful knowledge for people in positions of decision making.

We also had a program for how the behavioral and social sciences could use augmented public support. So there was a propagandistic or a direct policy recommendation element to it. It came out at the wrong time, in a way. It served its political purpose, but it certainly wasn't the time for heady expansion of the behavioral and social sciences budgets in the National Science Foundation or anywhere else. But anyway, it kind of helped at the time. It was one of the efforts that the social and behavioral sciences made. This, through the National Academy, but as I say, COSSA formed, and there were others. It was a politically active time, and it was very important that the economists and the psychologists who are among the largest and prestigious of the social sciences joined in. We presented a united front. Reagan got attacked. Reagan and Stockman got attacked in the press for this idea of uselessness. Editorials around the country were written, saying, what are you talking about? Are you talking about the U.S. census? Are you talking about all the tremendous amounts of data of the social sort that the government depends on? What's this "useless" talk? So there was an onslaught kind of outside of the behavioral and social sciences as well on this particularly savage initiative that Reagan and Stockman were putting forward.

21-00:16:47

Rubens:

Was this report looked at?

21-00:16:53

Smelser:

Well, who knows what happened. It filters into all the agencies. It gets read. Its influence is unmeasurable. It was certainly out of our control as to what impact—

21-00:17:08

Rubens:

We're not sure if the press was reading this. The popular press, probably not.

21-00:17:11

Smelser:

It got written up a little bit, as I remember, but it wasn't exactly a blockbuster document. It went the way of all documents. It seeped into the system.

Probably had some influence. Let's put it that way. Anyway, it was regarded as sufficiently successful as an enterprise, that the National Academy said, let's continue this work. They decided to say, let's keep this committee alive, with altered composition, and let's now take a look at the history of the behavioral and social sciences over the past fifty years and pick up some additional evidence of the kind of contributions that the behavioral and social sciences have made over that period. It happened to be the fiftieth anniversary of the Hoover Commission Report in 1933 that Hoover had—

21-00:18:13
Rubens:

The survey.

21-00:18:15
Smelser:

A survey that was William Ogburn's survey. They turned to me immediately to chair it, because I had kind of played this unanticipated but important role in the first volume. They asked me to chair it alone, with the staff member Dean Gerstein. He and I co-edited that one.

21-00:18:44
Rubens:

'82, you become chair, but it's anticipating the fiftieth anniversary.

21-00:18:52
Smelser:

It's anticipating the conference that we had. It anticipated the fiftieth anniversary. We called it *Fifty Years* something. The title may have indicated the Ogburn report. I'm not sure.

21-00:19:04
Rubens:

Behavioral and Social Science: Fifty Years of Discovery.

21-00:19:08
Smelser:

That's right. I wrote an essay for this myself, on Ogburn's view of the social sciences and how they changed over that fifty-year period. Gerstein was a sociologist. As a matter of fact, he had been a student of Parsons, years after I was. He and I had kind of an affinity with one another. We held a conference in Washington. We kept trying to pick up some interesting historical developments. We had one chapter, for example, that Amos Tversky and Daniel Kahneman had put together. This very important work on heuristics and cognitive psychology. Wrote a chapter on that. There was a chapter on our new understandings of exactly how law enforcement works and doesn't work. There was a whole thread, a whole—

21-00:20:05
Rubens:

Crime was particularly going up in that period. No one had a handle on it.

21-00:20:07
Smelser:

That's right. We were interested in whether or not the crackdowns work. There was a lot of good criminological research going on, and it was a good example of the evolution of knowledge over this time. It was a kind of companion volume, the first one focusing on the present, and the second one focusing on past trends. That came out, I believe, '83 or four.

- 21-00:20:41
Rubens: Let's double-check. '86, actually. So it took a couple years to—
- 21-00:20:46
Smelser: It matured. Gerstein and I did the editorial work on it, and pretty much I was in charge of that volume. It didn't create exactly the same kind of stir as the first one did, because, well, it wasn't a crisis moment. That had passed. But it was in fact noticed and reviewed.
- 21-00:21:10
Rubens: Was there a similar committee?
- 21-00:021:12
Smelser: Yes.
- 21-00:21:13
Rubens: Was that more cohesive than the first one?
- 21-00:21:16
Smelser: We didn't have any fights at all. The mission was quite clear. We were simply attempting to gather a series of notable and useful traditions. There wasn't too much disagreement as to—
- 21-00:21:39
Rubens: Where did you write that? Did you have anyone's cabin to—
- 21-00:21:43
Smelser: No, no. Gerstein and I did it. He visited here a couple of times and I visited him once regarding the approach to the preparation. It was a pretty smooth operation. National Academies Press published that. Then there was a third initiative. Those first two were regarded as important by CBASS, The Commission on Behavioral and Social Sciences and Education. That's one of five or six such commissions in the National Academy, in the National Research Council. It was under their auspices that we worked.
- 21-00:22:28
Rubens: You were explaining this to me earlier, but just for the record, the National Research Council is the research arm of the National Academy itself?
- 21-00:22:38
Smelser: That's right. The way it works is, often Congress will say, we need a study on this or that topic. Global warming, compulsive gambling.
- 21-00:22:55
Rubens: Even terrorism? Is that—
- 21-00:22:56
Smelser: That came later. Went through the same thing. Terrorism, immigrants and their economic impact, et cetera. There are hundreds and hundreds of these reports that come out. A couple hundred a year. It was a very active group. Congress was also an initiation. Sometimes foundations will give money.

Sometimes there will be an internal initiative on the part of a National Research Council committee, say the Committee on Statistics, which is one of the standing committees in CBASS. They will approach Congress or approach other funding sources for a summary assessment of the policy implications. Maybe a study of the census. Things of that sort. It's just a huge flow of research reports that come out, that are thought to be timely and serving the nation. It's a beautiful model for research, because once Congress decides it wants a report on a topic, and once it appropriates the money, Congress is out of the picture. It's a completely independent body. It's got a culture of nonpartisanship, a culture of breadth and balance and judgment in its own reports. It does not regard itself as a partisan committee in any regard, even though some of its reports, like on marijuana use and on global warming, turn out to be very controversial. The effort is to be scientific.

21-00:24:34

Rubens:

And primarily academics who are engaged in this?

21-00:24:36

Smelser:

Yes. Sometimes you'll get applied experts who are not academics. It's mostly an academic endeavor. There's always a mix on these committees, between Academy members and non-Academy members. Here I was, already taking a kind of leading role in that organization, even though I wasn't a member of the Academy. There was always a sense there should be some Academy members on these.

21-00:25:10

Rubens:

So we're talking about the third one.

21-00:25:13

Smelser:

This was regarded as successful, so they formed a third rendition of this, called the Committee on Basic Research in the Behavioral and Social Sciences, which was now to take a look at the future. They wanted me to be chair because I had just kind of established myself as being an effective person, I suppose, in the preparation of these earlier reports, and was in good stead with CBASS. However, there developed a little hesitation in CBASS, I was told about, I think by Gardner Lindzey. I'm not sure. This was going to be such an important committee. It was expanded. This idea of the future of the behavioral and social sciences meant a document for the government in terms of future support. They said they wanted a second chair, a person who was a member of the Academy and who was in a different area of the social and behavioral sciences. I was interdisciplinary, and that was recognized, but obviously I was more on the social psychological side. So they chose, after talking with me, but it was their choice, Duncan Luce, a cognitive psychologist in decision making from Harvard. A very brilliant man. He had been a member of the Academy for quite a while. Older than I by ten years. I welcomed him, even though he had a reputation of being somewhat sharp. Not exactly a collegial type. I had heard this by reputation, but I didn't have any direct data—

21-00:27:10

Rubens: What was his specialty?

21-00:27:12

Smelser: Psychology at Harvard. Statistician. Quantitative, kind of formal type, and clearly in the mainstream of cognitive psychology at the time. This was a bigger committee.

21-00:27:30

Rubens: How many?

21-00:27:30

Smelser: Twenty-five. Then we had Gerstein continue as a staff officer, because he was an indefinite staff person of the National Academy. He was the continuity, and I was the continuity, but he was the continuity with me. Luce and I agreed we would take turns chairing the meetings of this group, which had a very eminent body of people. We were a super group there. As a matter of fact, it proved to be kind of a difficult group because they were so smart and so well-known. A lot of big egos on the committee.

21-00:28:21

Rubens: Is this all men?

21-00:28:23

Smelser: No, there were two or three women, but not more. There was an anthropologist there. I think there was a woman from psychology. Yes, Rochel Gelman was in it. It was mainly male.

21-00:28:38

Rubens: I'm sorry to interrupt. Who's picking the members of the committee?

21-00:28:44

Smelser: They have a whole machinery in the National Academy. I didn't pick the members. The whole thing was done by a committee on committees. It's not named that, but that's what happens. They get a balance of Academy members and non-Academy members. They do a lot of consulting—it's something I wouldn't want to do—as to who might be the best people to be on this committee. It turned out Gardner Lindzey was on it. This time, I was his chair. And Kenneth Prewitt, Eugene Hammel. Really an eminent body of people. We undertook to present a third report on what we would call the leading edges in the social and behavioral sciences, areas of ferment. To give you a couple of examples of what was ultimately chosen was the behavioral basis of disease and disorder in social and behavioral sciences. Behavior as a causal factor in medical disorders, as an example. We had a chapter on globalization. We had a couple of chapters on cognitive psychology.

We had two big issues in deciding on how to break this thing up into parts. We had one contingent on the committee that said, we will make this disciplinary. What do the psychologists have to say to the world? What do the anthropologists have to say to the world? And so on. I fought this. I said, we

don't want to do that. We want to choose topically exciting areas, hopefully those to which more than one discipline has contributed, but not necessarily. There might be some technical economic issues that we want to put forward, which the other social sciences have not interested themselves. Fine, we can include that, but not break it down: here's a section on economics, here's one on sociology, here's one on anthropology, and psychology, and so on. I didn't want it. The majority of the committee was on my side on this one.

21-00:31:00

Rubens: How often did you meet?

21-00:31:02

Smelser: We met every couple of months, I think. Three months. I think there must have been five or six meetings in Washington, and we had one in southern California. I think there were five of them.

21-00:31:17

Rubens: So you prevailed? This view.

21-00:31:19

Smelser: I prevailed. I also suggested that the way to do this was to survey maybe 300 to 500 social scientists around the country, get them to identify what they thought the most exciting areas were, and we would use this as a database for our own decision as to what the chapter headings and topics would be.

21-00:31:46

Rubens: So you had some staff to do this?

21-00:31:49

Smelser: We were assigned a budget by the National Academy. We had Gerstein and a crew of three or four people that served as staff members to it.

21-00:32:05

Rubens: One of the topics was race.

21-00:32:11

Smelser: Yes. I think one of the chapters in that was on a changing conception of race relations. They were timely. There were thirty-one topics in all. It was an ambitious book. It turned out to be much larger than either of the first two. There was a second division in which I played a role. That is there were two members—more than two. There were maybe a half-dozen members. Among them were both Gardner Lindzey and Kenneth Prewitt.

21-00:32:47

Rubens: Who is Prewitt?

21-00:32:49

Smelser: He was a political scientist who was a survey expert at the University of Chicago, at the National Opinion Research Center, for many, many years. He was president of the Social Science Research Council for a period of time. He

was director of the census under Clinton for a year or two, for conducting of one of the censuses. He was a man of standing. I had been on one committee after another with him around the country. He and Gardner Lindzey, my former mentor, said this ought to be a highly focused report. We ought to pick up one or two lines, like survey research and what it contributed, and we should really dedicate this committee's work to a few selected lines of this sort, and push it, and that's it. I was much more interdisciplinary and comprehensive in my view, and other members of the committee were, too. We had a big public fight about this. I didn't often cross swords with Gardner.

21-00:34:00

Rubens:

When you say public, do you mean within the committee?

21-00:34:02

Smelser:

Within the committee.

21-00:34:03

Rubens:

As opposed to working it out.

21-00:34:05

Smelser:

We didn't work it out. Gardner and Prewitt brought it up publicly. Said, this is what we've got to do. I spoke firmly against that. Said, no, we have to be more comprehensive. We have to be more interdisciplinary. We just can't advertise some little corner of the social sciences and think that will do the trick. I just said this is out of the spirit of what we were commissioned to do. That began a public debate, and they withdrew that position. Gracefully. It wasn't personal at all. It's all collegial, but pretty sharp. That was one of the divisions. At that point, we began. We did this survey, which was of some help. If you survey outstanding academics about what's the most important thing going on, many of them will cite their own research. We got a certain amount of self-promotion that went on under this survey, but at the same time, we got some very thoughtful responses.

21-00:35:12

Rubens:

I assume they were interviewing some applied professionals, not just academics.

21-00:35:20

Smelser:

These were academics.

21-00:35:21

Rubens:

All academics?

21-00:35:22

Smelser:

This was an academic committee, yes. We didn't try to get into, say, survey firms or industrial psychologists. This was an academic piece of work. The leadership was taken by Duncan Luce and me and Gerstein. There was a woman named Sonya Sperlock, who ended up being a coeditor of it, who was extremely helpful. We, on our own initiative, thought that she should be recognized as a coeditor of the volume.

21-00:36:05

Rubens: What does she represent?

21-00:36:06

Smelser:

She was on CBASS. She was an Academy employee as well. She was part of Gerstein's staff. She turned out to be so helpful that we included her. The planning was done by us four. I had a relationship with Duncan that was mutually respectful. He's quite opinionated. More opinionated, I would say, than I, in terms of the flexibility with which he would push his own opinions. But nonetheless, we had a cooperative relationship. He wasn't an especially effective chair of the committee, because he had a hearing problem. It was one of these problems where if a chair scraped on the floor or someone coughed—it was one of these things, you can't distinguish between a field and context. Everything was equally loud. It's a hearing disorder, and he had it. Sometimes he would actually call on me to take over the chair in meetings which he was officially chairing. He got somewhat impatient with some of the, quote, "softer" areas. He said, "What's this globalization stuff? Do we really want to have this? Is it rigorous enough?" I tended to be, again, more on the inclusive side, and he was more on this hard science—let's deal with the stuff that's really rigorous and scientific. That, of course, was his outlook on the world. We never had any serious disagreement. However, he and Gerstein had a bristly relationship. I once again was called upon to be a—

21-00:38:05

Rubens: Gerstein sounds like he's a little—

21-00:38:08

Smelser:

A little feisty. He was feisty. Smart man and extremely—Luce had the idea—it had to do with the ordering of names of the editors. Luce wanted to push Gerstein and Sperlock way back, and so there was a fight. It was a personal fight about recognition and status and so on. I think I had another diplomatic assignment to work that one out, too.

21-00:39:29

Rubens: So it looks like it's Luce, Gerstein, and Sperlock.

21-00:39:38

Smelser: And Smelser.

21-00:39:39

Rubens: Where does your name come?

21-00:39:41

Smelser: As I said, "with." It turned out to be alphabetical. That was my view. Luce wanted him and me to be first, and the others to follow on. It would have broken the alphabetical—

21-00:40:41

Rubens: Alignment.

- 21-00:40:03
Smelser: I prevailed on that one, too, I believe.
- 21-00:40:06
Rubens: It sounds like a logical solution.
- 21-00:40:08
Smelser: Alphabetical order. Duncan seemed to be too self-interested. I think he was a little ashamed of his position after a while.
- 21-00:40:17
Rubens: So how was this one received? It came out in 1988.
- 21-00:40:24
Smelser: This one was given a lot more fanfare. There was a whole dinner at the National Academy, for which Duncan and I showed up, and we would greet the public. There was press there, and there were interviews, and a lot of Academy members showed up. Frank Press, who was president of the National Academy of Sciences at that time, said something which I regarded as irresponsible, but nonetheless was noted. He said, "This is like the Vanevar Bush report of 1945." Oh my god. What an exaggeration. What a complete fantasy. But nonetheless, it was very praised. I visited different Washington offices afterwards. Wanted to be interviewed by the National Science Foundation, one of the major funding offices, as to some of the topics. We had a second volume, edited by Luce and me and Gerstein, I believe, called *Leading Edges in the Behavioral and Social Sciences*.
- 21-00:41:20
Rubens: That's a Russell Sage publication. You consider that a second volume?
- 21-00:41:31
Smelser: It was a companion volume for the major report.
- 21-00:41:40
Rubens: Luce and Gerstein are on that?
- 21-00:41:41
Smelser: Yes. What we did was we got every member of the committee to form another subcommittee of people in their areas, and to elaborate on the contents of the major—
- 21-00:41:56
Rubens: That was smart. Was that your idea?
- 21-00:41:58
Smelser: Yes. I went to a couple of these meetings and talked to the people as they were working out their essays that appeared in this book called *Leading Edges*. It was meant to be a deepening, more circumstantial statement. It kind of paralleled the content of the major report, but led to a much more circumstantial discussion of exactly what research had gone on. There were some sessions in learned societies. I went to the meeting of American

Sociological Association that was dedicated to this report. It was generally regarded as a notable production. It shows up as being cited in the literature even now. From time to time, I come across references to it. I considered it certainly an important commitment on my part, because it is so—

21-00:43:00

Rubens:

Did you write the introduction to this? I know Jess has studied it more carefully than I.

21-00:43:06

Smelser:

I think an introduction was written by the three of us. I did a lot of the drafting. Probably the major drafting of the book, and a rewrite of those drafts that Gerstein and Luce put together. I tend to be the stylist for the writing, which was a kind of typical role. Editorial background, and I regarded myself as being a pretty good writer of prose. Luce is a very brilliant man, but he tended to write more technical stuff. This was not meant to be a technical book. It was meant to be as accessible as possible, so that was a—

21-00:44:00

Rubens:

Not the companion piece, but when the major third report came out, the political climate had changed. This is 1988. Reagan is out of office.

21-00:44:11

Smelser:

Sure. This was the beginning of the Bush one administration. The aura of the social sciences was no longer this object of polemic attack. That era had passed. Funding was okay. It was on the normal course. We had hoped that it would stimulate new programs. Budgets are so rigid and sort of year-by-year that there's only so much room for innovation. But nonetheless, it proved to be an intellectually consulted, and I think respected, document.

21-00:44:51

Rubens:

This almost full decade of service and engagement, was this fulfilling for you? Intellectually stimulating?

21-00:45:01

Smelser:

Yes, it was, and it was also stimulating from the standpoint of the—I felt, really honestly, without engaging in too much self-congratulation, that I played a pretty effective leadership role in it. That there were a lot of these subdivisions and tremors and faults in our own fields, and some of them surfaced, and I summarized those ones I took a role in trying to either resolve or trying to get my way in this. In the early nineties, quite a few people would tell me, “You’re Mr. Social Science, aren’t you?” because I had done this. I became head of CBASS (later renamed DBASS because it became a Division rather than a Commission within the National Research Council) later in the nineties. I was on the board of the Russell Sage Foundation. I was the director of the Center. I was also again on the governing council of the Social Science Research Council. So I was in every one of these things, and taking a kind of leadership role in many of them.

21-00:46:22

Rubens:

You're writing a textbook throughout the decade. You've written one by '80, and then it's going to be revised four times throughout the decade. Did you find it useful, the kind of—

21-00:46:36

Smelser:

Oh, always. Not that I—

21-00:46:38

Rubens:

Research that you had done and synthesis that you had done, to employ it in your text?

21-00:46:42

Smelser:

The point is, these things mutually feed each other. If you write a textbook and try to cover your own field, or even some other disciplinary aspects, this is obviously raw material for anything you do having to do with the general view of the social sciences. My exposure to these twenty superstars on my committee did the same thing, and kept informing me of what was going on in all their fields and their perspectives and so on. These things build on themselves. My actual writing, I regard—we'll talk about this—I regard my writing in the 1980s as something as a lull in my—

21-00:47:25

Rubens:

It's more synthetic.

21-00:47:27

Smelser:

A lot more synthetic work. I did the text. I did quite a few articles and so on. We'll talk about those later. I inched ahead on my major research project on social history. I was in my fifties. 1980, I turned fifty years old. This was kind of, in a way, a mature expression of what my talents were. It was synthetic and inclusive. It was consistent with my style. But it was a very different thing from being a lonely scholar writing books.

21-00:48:12

Rubens:

I'm not sure what topic we should take up next.

21-00:48:15

Smelser:

I think we've finished this.

21-00:48:16

Rubens:

We talked about taking up your role with the International Sociological Association. We'll pick up CBASS later in the nineties.

21-00:48:28

Smelser:

Yes, I become chair of that I think in '96. That works right into my work on terrorism. We can turn to the ISA.

21-00:48:41

Rubens:

Let's do that.

21-00:48:44
Smelser:

Actually, in 1954, I believe it was—I would have to correct the date—I was in England at the time. Maybe it was 1956. I was working on my doctoral dissertation. I went to the second meeting of the International Sociological Association in Amsterdam. I kind of became a regular member of it from that time on. I didn't go to Spezia when I had just come to Berkeley, but I went to the meeting in 1966. I went to Bulgaria in 1970. There was a meeting in Montreal, one in Toronto, and then one in Sweden. I was a regular attender. I would usually be involved in one or more of the intellectual sessions of it. I was already known. My writings had gotten to be somewhat known internationally, so I began to develop linkages through the International Sociological Association.

There was one particular area in economic sociology in which I took a rather more special role. There was a colleague from Toronto named Harry Makler, who later moved to a research position at Stanford, who was very much interested in pressing economic sociology within the International Sociological Association. There was not a special research group in economic sociology at that time. He was a scholar of financial institutions, with special emphasis on Latin America. He happened to be a friend of Fernando Henrique Cardoso, who later became president of Brazil, but at that time he was a sociologist from São Paulo. He was the main spokesman for a movement within the field of economic development called dependency theory. He was sort of the spokesman for it, Cardoso. Kind of a left-wing, neo-Marxist, neo-Weberian movement that protested against modernization theory. He was one of the intellectual leaders and highly respected.

Makler got me and Cardoso together. As a matter of fact, Cardoso will later come to visit Berkeley, and I hosted him in my interdisciplinary seminar on comparative work when he was here. We became independently friendly, aside from Makler's efforts to form a kind of intellectual union. Makler's aim, and it turned out to be our aim in the end, was to form a special research Committee on Economy and Society in the International Sociological Association, and Cardoso and I became co-chairs.

21-00:51:49
Rubens:

This starts in 1980?

21-00:51:50
Smelser:

About 1980 is when that began. He and I served as co-chairs for a couple of things.

21-00:51:54
Rubens:

Until '86?

21-00:51:55
Smelser:

Yes. He became the president of the International Sociological Association. I became a candidate for president, and I'll tell you that story later. Anyway,

we'd go to all the meetings. The one that I remember most vividly —because he was there, and we of course chaired these different sessions on subjects in economic sociology at the meetings. He and Alberto Martinelli and I and others went to a Bellagio conference and edited a volume that came out of that conference on economic sociology. It was a big engagement, a big international engagement. I had a lot, of course, international traveling with it. Then at the Delhi meetings, which I believe were in 1986, there was an opening on the executive committee for the ISA. I think they tried a couple of people who wouldn't serve. I got this phone call from Delhi, saying, "Would you be on the executive committee?" Which is about twelve people who basically run the association in the interim periods between their international congresses, which occur every four years. This one was in '86. They called me up and asked me if I wouldn't serve. I didn't go to the Delhi meetings, so they called me here. I thought about it for a few minutes, said it's fine. I've had this history of involvement. I may as well undertake this. That executive committee met every year, in different—usually Europe.

21-00:53:41

Rubens:

How many on that, about?

21-00:53:42

Smelser:

Oh, fifteen to twenty. It was a governing body, basically, of the International Sociological Association. It was ungovernable, basically. The ISA is organized on the principles of the UN so that every nation has one vote in the council. It's full of fighting. The politics of the ISA were totally dominated by the Cold War, through the eighties. The Russians were always wanting parity recognition with the United States. They were absolutely adamant, and they were disruptive. In a way, the politics of the Cold War was just mirrored in the ISA. The Russians were courting third-world representatives. It was all very much Cold War politics.

21-00:54:42

Rubens:

I would assume there's also the sort of new revolutionary politics that maybe Cardoso represents. Maybe that's saying too much.

21-00:54:48

Smelser:

Well, I'd say the dominant tone of my economy and society group was some variation of a new left, or new Marxist scholars, mostly from other parts of the world. The culture of that was a left culture. I don't consider myself a right-winger, but nonetheless, I was much more moderate. Cardoso is a very catholic, small "C," guy. He wasn't about to pick fights or become imperialist or anything of that sort; that would have set a wedge between us. We had a very cordial and respectful relationship with each other. That was a nice adventure to work with Cardoso. Organize the intellectual—

21-00:55:39

Rubens:

I know we're talking about you moving to the executive committee, but I meant to ask, when you set up the Committee on Economy and Society, are you beating the bushes? Are you trying to find people who will come—

21-00:55:53

Smelser:

No. No. It got good membership the minute we formed it. We didn't have to do any campaigning. There is a minimum number that you have to have in order to form a research committee, and we got it without effort.

21-00:56:07

Rubens:

What is that, about?

21-00:56:08

Smelser:

Sixty people. It got a number who affiliated with it. Double that number, perhaps, in the International Sociological Association. So it was one of the vital ones. As a matter of fact, Alberto Martinelli, who translated my *Economy and Society* into Italian, who was a close friend of mine, from the University of Milan, who traded houses with me at one time, he and Cardoso both had their primary membership in the economy and society committee. Both of them became presidents of the ISA. Even though it was young as of 1980, it was a core group with a certain amount of clout and visibility to it. The executive committee was also torn by international politics. This included racial and ethnic and gender politics. The American Sociological Association has a lot of that. The International Sociological Association dwarfs the ASA from a standpoint of petty international politics. I went to these meetings, played an active role in the executive committee. I had this way of—it sounds a little self-serving, but it's true—of getting into the center, with even intellectual leadership. There's always an upward drift, I've experienced, in terms of my leadership and becoming a spokesman.

21-00:57:57

Rubens:

You like it and you had the skills.

21-00:57:58

Smelser:

I like it, but I don't say, here, how am I going to do it?

21-00:58:02

Rubens:

Yes, you're not doing it intentionally, originally, or strategically, it seems to me.

21-00:58:04

Smelser:

No, this happens —there's a certain amount of respect when I get there, and then—I like to be influential in organizations and it seems to happen, but I don't have a scheme of how to do it. Anyway, this happened in the ISA. I became a core member, even though I was there for four years. In the meetings at Madrid, I was nominated for presidency of the ISA.

[Begin Audio File 22]

22-00:00:00

Rubens: You were saying—

22-00:00:10

Smelser: I was nominated to be the president of the ISA.

22-00:00:16

Rubens: By this executive board?

22-00:00:18

Smelser: Yes. Or there's a nominating committee, I think. I was asked to, as it turned out, run for the presidency, because there were two others involved in the race. It was in Madrid. There was a Spanish sociologist by the name of Salvatore Giner, who was ambitious to be president and got a number of supporters. Then there was a third candidate, a guy with whom I was fairly close, from India, T.K. Oommen, who was also to be nominated. The three of us were nominated. The election would take place in the council, which is the representative body of all the countries in the world. A campaign developed. I wasn't part of it. Jiner was the one who started it. The campaign was mainly an attack on me. He badly wanted to be president. As it turned out, he was ineligible because he had served too many years on the council himself. There was a constitutional provision which said you can't be president after so many years. They went to a constitutional committee or something and they declared him ineligible, but he didn't accept it. He still continued to campaign.

The campaign was against me, I say. It turned out to be an anti-American campaign. An appeal for third-world sentiments, Eastern European—whatever. The politics of the Cold War, or the politics of international organizations. The attack had two threads. One, way back to my affiliation with Parsons. It had to do with an attack on functionalist sociology, which had, over the years, gotten a reputation of being politically conservative. In other words, here's this apologist for some kind of vague establishment somewhere, almost right-wing, that I was portrayed as. Furthermore, I wasn't sufficiently international. They attacked my text. *Handbook of Sociology*—where's all the international sociology in that handbook of sociology that you edited in the 1980s? It was that sort of provincial, "He's an American. We've had too many American presidents of this association." On and on. I didn't participate in this.

22-00:03:06

Rubens: You did not campaign in response?

22-00:03:07

Smelser: I didn't do a campaign. I didn't do any campaign. When Jiner was declared ineligible, then it was a matter of Oommen. Now, Oommen had a great deal of intellectual respect for me. He told me he wouldn't run against me. When it

became evident that he and I were the only two running, he said okay. He kind of ran on a third-world ticket, if you will. He was a pretty eminent sociologist.

22-00:03:42

Rubens: Did one have to give a talk?

22-00:03:44

Smelser: No, no, it was all in the corridors and the rooms. A lot of campaigning. A Spanish group came to me and said, "Would you support Spanish becoming a third official language of the ISA?" A lot of politics and so on. It turned out I lost. The vote was kind of substantial, a majority for Oommen, and he became president. There was a custom in the ISA that the runner-up in the presidency could basically choose the vice presidency that he wanted. There were three vice presidents. One for the program of the next meeting, one for coordinating the research groups, and then a third. I was disappointed not to be elected, but I was not wounded to the point of saying, okay, that's it. I said I'd be happy to be vice president of the program. I was elected to that. My opponent in that was a Russian. Yadov, his name was Vladimir Yadov. We were very close to each other. He was a kind of a Gorbachev-type scholar. A moderate, a liberal. We liked each other a lot. He's a very admirable person. We hit it off very well. I was elected.

22-00:05:38

Rubens: Again, it's the council? I'm surprised the vice chairs are elected.

22-00:05:43

Smelser: For the four-year term, but I was on the council again, of course. Then I chaired the program committee for the meetings that were to be held in Bielefeld in 1994.

22-00:05:56

Rubens: This is where?

22-00:05:57

Smelser: In Germany. It was at University of Bielefeld. It's in northern Germany. Kind of an experimental university. We held that in 1994. It was just as I was coming in to be director of the Center.

I remain on the executive committee this whole time, but I also headed the program committee. That was my main commitment, to develop the intellectual theme and organization of the next session in Bielefeld. I continued to go annually to these meetings, from '86 to '94. Every year, I'd go to a meeting. I was both on the council and chairing the program committee from '90 to '94. Actually, I thought we had a very good program in Bielefeld.

22-00:07:04

Rubens: Did you have a goal and a theme?

22-00:07:07

Smelser:

Yes, it had a theme. It was on diversity and contestation in social sciences. Organized several plenary sessions around that. Normally, the International Association, 85 to 90 percent of the program is fixed already because of the research groups doing their own things, but you have plenary sessions. I chaired one of those and organized several others.

22-00:07:32

Rubens:

Did you feel compelled to be in concert with the critique that had been made of you? That you wanted more internationalist—

22-00:07:46

Smelser:

That campaign went over the waterfall and it was all over. There was no tension about my being vice president. Let me just give this as a matter of speculation. Eight years later, in 2002, I was given the Dogan Prize. The first recipient of the Dogan Prize for career accomplishment in sociology. That was at a meeting in Brisbane, in Australia. I think those who had supported my presidency—a lot of colleagues. There were many former students. There was a Polish scholar, Piotr Sztamka who later became president of the association. He was my student. He came here to study with me. Martinelli pushed me very hard. I think by that time, Jeff Alexander was on the council, so there was a big push to give me this prize, the Mattei Dogan Prize. There was some sense, on the part of my supporters, that my not being elected was a travesty, just in terms of the intellectual leadership in the field. They thought that that was wrong, that I should have been recognized. So I think that prize was a kind of movement. Even that was contested. It was contested by a group of women who had the idea, these people got all the recognition all their lives anyway, and they wanted a woman to be the winner of this first prize. My friends on the council kept me informed about the debates that went on. But they couldn't agree on a woman sociologist, so they chose a woman, an anthropologist by the name of Mary Douglas. A really outstanding English anthropologist who I think would deserve presidency of anything. But the point was, why are we electing an anthropologist? Why an anthropologist for this prize? So my supporters prevailed.

22-00:10:04

Rubens:

How did the prize originate?

22-00:10:10

Smelser:

A gift. A gift from a man named Mattei Dogan, a French sociologist, who turned out to be a great admirer of mine, and wrote a lot of stuff citing a lot of my own work. He and I later planned a meeting in Paris on American hegemony in the world, but it got canceled on account of 9/11. They felt it would be too controversial. Dogan gave this prize. It was a prize of, I think, \$5,000. It was meant to be in honor of a sociologist for a lifetime achievement, in honor of my work. I was highly pleased with this honor. I gave an acceptance speech on comparative sociology at the meetings in Brisbane when it was awarded. On cross-cultural, cross-national comparative

work. One of the fields which I was known for. It was published in the *International Sociology*, the journal. It was a very nice end to my history with the International Sociological Association.

22-00:11:33

Rubens: How do you speak about the status of the ISA? Who is the US representative? How is that person picked?

22-00:11:49

Smelser: I think by the ASA, American Sociological Association. I was not that. I was always on it as an at-large member between '86 and '90. I was an at-large member, and then I was a member by virtue of being a vice president, so I was never the national representative.

22-00:12:14

Rubens: But that's saying something about the relationship between the ASA and the ISA. It seems that it was important. It was a large organization and that it really—

22-00:12:26

Smelser: Oh, yes. Well, it's an international organization with great standing. The ASA, as a matter of attitude, thinks that the ISA is not nearly its intellectual peer.

22-00:12:39

Rubens: That's what I'm asking.

22-00:12:40

Smelser: They kind of look down on it. It's a kind of anti-foreign feeling that I never shared. The ISA is a totally disheveled and disorganized organization that has no money at all. It lives by its conferences and a few publications. It's fraught with these UN politics. It's not a very pleasant organization to be in, because everything is fighting. As I said, you're sometimes on the receiving end of unjustified attacks, just by virtue of the fact of being your own nationality. I have all kinds of ambivalence about the organization, even though I played a pretty big role in a certain period of its history. I always have had a certain ambivalence about it.

22-00:13:34

Rubens: But it's a distinguished engagement that you have.

22-00:13:37

Smelser: Oh, yes, no question. It was a happy relationship that I had. I appreciate and am flattered by the recognition I got. But all you did in that organization was fight over some international, ideological, symbolic point.

22-00:13:56

Rubens: Do you think we did enough on that?

22-00:13:58

Smelser: Yes, I think I've said everything.

22-00:14:00

Rubens: There are parallels of ranking among the various professional history associations.

22-00:14:28

Smelser: One of the points that I'm going to make in my Clark Kerr lectures, I've already decided, is to characterize higher education as the world's leading system of creating second-class citizens. I'm going to talk about inequality and stratification in higher education as being one of the core organizing features to understand how it works. It's not going to be a condemnation.

22-00:14:56

Rubens: That's interesting. And the second-class citizens is—

22-00:14:59

Smelser: They're associate professors, assistant professors, lecturers. Everything is ranked. Universities rank each other rigidly among themselves. They don't like state colleges. They don't like junior colleges. Everything is stratified.

22-00:15:17

Rubens: I'm wondering, is this too forced a transition? We were going to talk next about your being acting director of the Center for the Study of Higher Education {CSHE}. I know it was a placeholder, but nevertheless, it's—

22-00:15:34

Smelser: It can be rather brief.

22-00:15:36

Rubens: This is in 1987. Who appoints you?

22-00:15:40

Smelser: The chancellor's office appointed me as director, that is Mike Heyman appointed me as director at that time. It was an evident fill-in after Trow's departure. I had been active in the Center for the Study of Higher Education all along, as a committee member. I was affiliated with it when I wrote my work on growth and conflict in California higher education. I'm a very close personal friend of Martin Trow. He involved me in everything. Sheldon Rothblatt, who was another director later, very close. We had a tight-knit group. There was a group of people who studied higher education, who were not in the education school. This included Fred Balderston, Budd [Earl] Cheit. It was a kind of group there. We all had a relationship with Clark Kerr. I always referred to that group as Clark's boys, because he called on us all the time for advice and so on. Anyway, I was active and known. I guess it was Joseph Cerny, who was vice chancellor for research, who actually carried out the operation of organizing the search for the director. I insisted that I wanted to be acting director. It was my second year in the senate presidency, so I was totally busy, and all these other things were going on in my life in the eighties. I said okay, but I said I'm going to be acting. I wanted to do one year. It turned out I did a second year, because they asked me to do the second year. I

regarded my role in the Center as a continuity with the leadership that Trow had given it, and I did not have an innovative plan, because, well, I said I would do it. I did it out of a sense of duty. My heart wasn't in it. I needed some money. It was very interesting, because my son was in Oberlin at that time, a very expensive institution, and my daughter was getting ready to go to college. That was expensive too, and a summer salary was helpful in paying the tuition.

22-00:18:18

Rubens:

Why do you say summer salary? Because it went through the year?

22-00:18:21

Smelser:

No, the summer salary is in addition to your own annual salary, by two ninths. In other words, they pay you for June and July, where you're normally on a nine-month salary. They just add two months to it. So that's a significant augmentation of your academic salary. Oberlin is one of the high-cost institutions of the country. We weren't going broke, but it was a way to ease his tuition.

22-00:18:53

Rubens:

I haven't asked you throughout this period your relationship to the department of sociology. Of course, you're a university professor at the time. So are you teaching also during all this period?

22-00:19:06

Smelser:

When I was vice president of the senate, I got a course off of an already light teaching load. When I was the president of the senate for that year, I didn't teach anything. I got course relief for being president of the Berkeley division. My teaching load in the eighties was down.

So I received a summer salary. They're adding to stipend. Just like chairman of the department gets a month or sometimes two months of summer salary. It's their way of giving you an administrative stipend.

22-00:19:53

Rubens:

It's not so much, comparatively, if you're director of a center.

22-00:19:57

Smelser:

No, no, no. It's significant in that it is nearly 20 percent of your salary, but it's not what you'd call a doubling or a big consultation-type thing. It just helps you out. I believe I sometimes would get part of a summer salary when I was associate director of the Institute of International Studies for many, many years. This was a little of a bonus. In the meantime, the CSHE was under attack when I was there. Cerny turned out to be not a friend of the center. The reason he wasn't a friend is because we didn't behave like a big-time, income-generating, organized research unit, the way those in chemistry and physics and the natural sciences, and maybe even in environmental, they are big money gatherers for the university, and the university gets a lot of overhead. The center was small potatoes by comparison. First of all, that kind of money

wasn't available. Trow had established the center as part organized research unit, part international seminar. He had a definite style. If it could be an Oxford College, he would have made it an Oxford College of conversation and contribution and visitors and so on, and not going after big grants from the Department of Education and conducting empirical studies and getting staff and getting overhead, whatever, the way that big, organized research units do and are valued.

Cerny kind of came after us from the standpoint of budget. They said, basically, you're not a very important center. I sort of summarize those two years, even at the time, as fighting for the life of the center. Fighting for it. I kept the budget where it was, which I think was a tremendous accomplishment, given the aims of the Berkeley administration on it. But I didn't innovate much. I continued its style pretty much as it was. As you can imagine, I was fulltime at everything else. So the amount of time I actually spent at the center was more limited than I probably would have done if I weren't so involved.

22-00:22:36

Rubens:

Did you have an associate director's daily—

22-00:22:39

Smelser:

Yes, there was a small center staff.

22-00:22:44

Rubens:

You were located in that little building, almost a basement-like stand alone structure.

22-00:22:45

Smelser:

South Hall Annex. It's since been torn down because it collapsed. I don't regard that as an especially luminary moment in my career, largely because I played a holding operation role. Important enough, to be sure, because if I hadn't, it might have suffered as a center. I've continued my relationship with the center. I was close friends with Heyman when he became acting director of it. Karl Pister was there for a couple of years, and now Jud King. I knew all of these people closely. I'm engaged with the center. It's through the center that my Clark Kerr lectures are going to be given. That and Institute of International Studies were my two intellectual homes outside the department.

22-00:23:47

Rubens:

Any relationship at all to the Berkeley's School of Education? You had done that report—

22-00:23:54

Smelser:

Constructed a report. No. My main relationship, formally, with the School of Education was that I sat on a lot of doctoral dissertations over time. That was a bit of an uneasy role because I found myself, as an outside member, often wanting to impose stricter intellectual standards than I thought the school was doing. It was a very uncomfortable role for an outside member to do that and

maintain collegial relationships. Martin Trow was in the school for a while. He resigned. Burton Clark was in the school for a while, and he resigned. He went to Yale. After the report came out, I developed a relationship with the new dean. Even though he didn't appreciate my recommendations about it, he and I developed—Bernie Gifford—a good relationship, and he turned out to be on the Board of Trustees at Head Royce School with me, as I mentioned in a previous interview, so we had another basis for interacting with one another. I didn't have much of a relationship—as a matter of fact, the Center for the Study of Higher Education doesn't have a very good relationship with the School of Education. Again, it's one of these ranking issues. There are only a few people over there that they had any respect for, in terms of being scholars. The feeling was that this group that I mentioned, of Balderston and Trow and Cheit and me and Rothblatt, were really doing the valuable work in higher education, and there wasn't any valuable work—

22-00:25:38

Rubens:

And had come out of disciplines. Had your own scholarly—

22-00:25:41

Smelser:

We came from the arts and sciences departments, and business school for Balderston and Cheit. But nonetheless, we had the feeling, and there was no objection to it, that we were a more serious intellectual crew than the kind of work that was going on in the School of Education. For better or for worse, that was the view that we shared, and probably true.

22-00:26:08

Rubens:

I didn't know if, as a result of you being there, that put you into a position for working with state assemblyman Vasconcellos.

22-00:26:16

Smelser:

No, no, no. We'll start on the Vasconcellos thing in a minute. I was kind of primed to maybe take over the CSHE when I came back from Stanford in 2001, but I let it be known that I wasn't interested. I wanted to retire. I wanted to do my research. I just let it be generally known, so I wasn't even asked to do it. That's, I think, about the time Heyman came in.

22-00:26:47

Rubens:

Yes, when he came back from the Smithsonian.

22-00:26:56

Smelser:

When I was down at the Center in Stanford, for four months, I was director of the Center for Study of Higher Education when there was an interim.

22-00:27:03

Rubens:

Again?

22-00:27:04

Smelser:

Again. For four months, all they did was come down and talk to me about a few staff—talk about a holding operation. I just was a figurehead with whom they had to clear certain budgetary items and so on. The staff members of the

CSHE, in interim between directors, came to talk to me. I just now remembered that.

22-00:27:21

Rubens: You're directing the Advanced Center—

22-00:27:23

Smelser: Yes, directing CSHE from the center down there.

22-00:27:25

Rubens: It's pretty amazing to me how much you were doing in those years, '87 to '88, '89. You're finishing up the third volume of the Academy study, and then the companion volume. You go to Russell Sage in '89?

22-00:27:43

Smelser: Yes.

22-00:27:43

Rubens: You're also, by the way, during all these years, a clinical supervisor for UC Berkeley's psych clinic.

22-00:28:00

Smelser: Graduate students in clinical psychology. By virtue of my long involvement in the Psychoanalytic Institute and Cowell Hospital, the people in the clinical training program in psychology said, "Won't you be a supervisor for our first-year students who are just going into clinical work?" So I said yes, I'll do it.

22-00:28:24

Rubens: That's nine years.

22-00:28:26

Smelser: Yes, I was nine years on it. It was a very, very gratifying byway in my life. I must say I loved the world of psychoanalysis and psychotherapy. These students were first-class students. They were just beginning. I was kind of old hand, in a way, relative to them. It's a beautiful relationship, this kind of supervisory—

22-00:28:52

Rubens: How does it manifest itself? What is the form of that—

22-00:28:57

Smelser: They came. I only had one student each year, and they came once a week. They would report on the progress of what they were doing with the different patients they were seeing. So they would summarize the report and I would—

22-00:29:14

Rubens: That's classic clinical supervision.

22-00:29:17

Smelser: Yes, it's clinical supervision. I would guide, raise questions, suggest, maybe try to offer insights as to what might be going on that they were not exactly

totally aware of, so on. With every one of them, I developed such a collegial relationship and mutual respect. I had a pretty good supervisory style. I didn't criticize very much. Though the import of what I said was critical, I tried very much always to be very supportive of these people, who were just beginning their work. It was a happy little side of my life, that whole business. I actually keep in touch with a couple of the people who were my supervisees.

22-00:30:00

Rubens:

You give it up because you go to the Advanced Center?

22-00:30:02

Smelser:

Yes, I quit almost everything at that point. I couldn't have carried it on.

22-00:30:11

Rubens:

When you were talking about being the outside examiner for School of Ed Ph.D. candidates—you also served that role quite a bit for the School of Social Welfare.

22-00:30:22

Smelser:

Social Welfare, I had a few candidates there. I was an outside committee for quite a few historians, particularly early in my career, because of my own historical work. Political scientists. Business school. Outside member on an awful lot of dissertations in different departments, largely because my work touched on those. Some in psychology.

22-00:30:50

Rubens:

I keep thinking about your schedule, how you kept up with everything. Each year was—

22-00:30:59

Smelser:

Pretty chaotic. A lot of traveling. What happened to me was a blessing. In the late seventies, I was on research grants. Pretty much on the beginning of my historical work, I got a research grant from Russell Sage Foundation. For two or three years, I was able to support a fulltime secretary on grant money, soft grant money. My second year as director of the Education Abroad Program, I received this very huge offer from Princeton to be the first sociologist in the Woodrow Wilson School.

22-00:31:59

Rubens:

You mentioned that only in passing—I think that it was the only time you were back in the US during your two years with EAP. I meant to get back to that.

22-00:32:00

Smelser:

Maybe I didn't mention it. In '79, Don Stokes, a man I worked with in the Political Science Association, organized this super offer that would have been half time in the Woodrow Wilson School, half time in sociology. The first sociologist to join the Woodrow Wilson School. It was all economists and political scientists. It was one of the two trips that I had back to the United States when I was director of EAP in London. I came back to Princeton to be

interviewed and be courted by Princeton. I was really interested in it. I called up Heyman and told him about this offer.

22-00:32:49

Rubens: I'm sure he wasn't too happy.

22-00:32:51

Smelser: No, no, no. He wanted to make sure I had continued my own scholarly work, and told me he wanted to do everything he could for me. My salary by that time was very high. I didn't feel as though I wanted to keep bumping my salary the way that happened in all my other offers from outside. I would always get some kind of an advancement in rank and salary. So I said, "Mike, how about trying to make my secretary regular, on the university payroll?" Of course, he blanched at that. He saw 2,000 professors coming to him, wanting a personal fulltime secretary. So I said, "Does it make any difference, Mike, that I'm a university professor? Couldn't we try to get this financed out of system-wide money as part of that system-wide appointment?" He hadn't thought of it. He went to the vice president at system-wide, and they said yes in a minute. So she became my secretary. You asked how I spent my time.

22-00:33:58

Rubens: Yes, how you managed your schedule.

22-00:34:00

Smelser: She scheduled it. She organized everything. She was a fantastic person.

22-00:34:04

Rubens: Where was she, literally? Where did she sit?

22-00:34:07

Smelser: She was in Moses Hall, by virtue of my connection with the Institute of International Studies. I had an office in Barrows and I had an office in Moses.

22-00:34:18

Rubens: You mentioned that you had a secretary there. I thought it came by virtue of being head of the IAS.

22-00:34:27

Smelser: No. She was personally attached to me. Indeed, when I went over to the system-wide to be the advisor in 1993, I took her with me. Then our relationship, which had lasted a long, long time—sixteen, seventeen years—I could not take her to Stanford.

22-00:34:49

Rubens: What's her name?

22-00:34:50

Smelser: Christine Egan. The people in the International Studies Center and I managed to place her as an administrative assistant in demography, which was an advance for her, from being a secretary for a faculty member. Then she went

and headed up the administrative staff in the history department for years. I told her the best thing that ever happened to her was my leaving. She was a wonderful person.

22-00:35:22

Rubens: How you could have managed the schedule otherwise would be very— computers are just starting to come in.

22-00:35:29

Smelser: It was just the beginning of computer time. She was a fantastically efficient person. That was a big answer to your question. I had her to organize my schedule. I tried to keep my mornings as free as possible. It turned out to be impossible, during that period of time. My life was pretty chaotic.

22-00:35:50

Rubens: No wonder Russell Sage would be sort of a real—

22-00:35:53

Smelser: A big relief. I couldn't make much progress on big intellectual projects. I wrote some, and I edited *Handbook of Sociology* in the period, '88. That could be part of a story, too.

22-00:36:10

Rubens: We'll do that.

22-00:36:13

Smelser: I guess it was kind of superhuman.

22-00:36:16

Rubens: Now, the same time you become acting director of the CSHE, you also become a chancellor's fellow.

22-00:36:25

Smelser: Yes. That was a little program on the part of the administration to award teaching. Heyman was still chancellor. It was to reward teachers, outstanding teachers. Heyman said, "I'd like to appoint you chancellor's fellow." I said, "What does that mean?" Develop a special course for undergraduates. Some kind of crème de la crème course for undergraduates. It was for a couple of years only. I always thought, well, it's a big name for a rather small assignment. I always felt it was a gesture in the direction that the Berkeley campus is paying attention to teaching.

22-00:37:20

Rubens: That was part of what you had argued for in the report on—

22-00:37:24

Smelser: Yes. He made me chancellor's fellow. I designed a course called Problems of Contemporary Civilization. I would pick out a series of topics. I picked out race, I picked out family, I picked out a few topics. I gave a general undergraduate course, which was quite well attended. There were maybe 200

people in it, and I had several assistants. I taught it for two years. I didn't find it too taxing to teach. It was pretty much in keeping with the same kind of teaching experience that I'd had in my theory course. Then I had also taught in the department a course called Sociology 10, which was for freshman, but it was for non-majors. It was an alternative to Sociology 1. It overlapped in content, to some degree, with a course that I taught as chancellor's fellow title. I taught that for three years. Found it somewhat engaging, intellectually. It was a one semester, one-shot course. I taught it in lieu of the departmental course. It was offered by the college rather than the department. It was an L&S course, not given by the department. That was an interesting little episode. I enjoyed it.

22-00:39:01

Rubens:

In the same years, '87 to '89, you become a consultant to the California Taskforce to Promote Self-Esteem and Personal and Social Responsibility. Who names that?

22-00:39:15

Smelser:

Here's how that unfolded. John Vasconcellos, who was a very powerful man in the California legislature, headed the Committee on Ways and Means, meaning budget, meaning university budget. He got a bee in his bonnet by virtue of his own personal history, psychological history, about the importance of self-esteem in social life and what an asset that was. He and a few likeminded people, not all of whom were in the legislature, got the idea that if the level of self-esteem of the ordinary citizen were raised, and people had better self-regard, that this would somehow or other be politically and socially significant in reducing serious social problems. He developed a list of social problems that he thought would be alleviated if people had higher self-esteem. Crime, drugs, chronic welfare dependency, teenage pregnancy. These were the kinds of problems he had in mind. He had the idea that the state government ought to involve itself, in some way, in the promotion of esteem among California citizens. He got the legislature, because of his position of power, to form a commission on self-esteem. Many people thought it was a joke. Many people thought it was a stupid thing for the state government to get involved in because of privacy matters and so on, but he kept pushing. He got the state government to form a special commission on self-esteem that would promote and push and so on.

As part of this enterprise on his part, and he was very religious about it, he contacted David Gardner, president of the university at the time. He said, "How can the university help us?" As you can see, the head of Ways and Means Committee coming down and putting his fist on the university didn't leave Gardner—Gardner couldn't tell him to go fly a kite, so he didn't know what to do. So he turned it over to Bill Frazer, who was his vice president. Frazer, who's a physicist, was buffaloes by the whole thought about what to do. He didn't know what to do. He turned it over to Calvin Moore, his associate vice president, who was a mathematician and knew even less about

the issues involved in self-esteem. Gardner told Cal Moore to call me for advice. What can we do? So I got this call from Cal Moore. He said, "Here's our problem. What can we do about it?" I said, "Give me a little chance to think about it." I sat and thought. I got back to him and I said, "Well, the only thing we can do is what the university can do best. We can take these problem areas, these six problem areas that the Vasconcellos commission has singled out, and we can locate faculty members throughout the UC system who will have expertise and will throw light on what actual relationship self-esteem has to these problems. Is it closely related to child abuse, for example, which was one of the areas? And we'll put together scholarly work, assessing the research on the relationship between self-esteem and various kinds of problematic behavior and social problems." They thought that was terrific. He said, "You do it." That was the reward I got for bringing up this idea.

I had such admiration for David Gardner. Social psychology—it's an area in which I felt not unfamiliar with the kinds of issues that would be involved, both theoretical and methodological and empirical. So I said, "Yes, okay, I'll do it." They gave me a little staff. I did a search, a thorough search, of all the campuses, of people who were working in different—some in schools of social welfare, some in academic departments, and I gathered together a group of six to eight scholars that formed a little university counterpart to the self-esteem commission in the legislature. So people from all campuses, or six or seven campuses. Multiple campuses. We went to work. I commissioned them each to write a review article. I could pay them. They gave us a budget of something like \$50,000. I'd give them a few thousand dollars to prepare an essay for this volume. I wrote the introduction to bring together a synthetic statement of this problem. I invited John Vasconcellos and Andrew Mecca, who was the head of the California Commission on Self-Esteem, —a religious leader of some sort. Mecca—I thought it was a very good name. Anyway, I invited them. We worked together.

I actually assembled a very good group of scholars, each of whom wrote me a very coherent essay on research studies that have actually referred to self-esteem and the relation to racial segregation. That was another topic, inequality and race. That was another self-esteem issue. It was a pretty respectable intellectual product when I got these essays together. But it was a mixed product. It didn't exactly agree with what Vasconcellos would have wanted. This was the big master variable, self-esteem, that would cut welfare dependency down and so on and so forth. All very mixed. Very mixed results. In some areas, self-esteem seemed to play a very decisive role, and in particular, I remember child abuse was a particularly important area because of abused children becoming abusers. There was quite a bit to be said. However, in my writing of the introduction, I was very circumspect, and considered, and didn't make big claims about this big master variable. I raised a lot of methodological problems about how it's measured, about how a single cause interacts with other variables, and how it really couldn't be regarded as a—so I gave a sensible introduction to it. Then I thought, how about inviting,

as a matter of statesmanship, Vasconcellos and Mecca to write introductory chapters along with me? A preface. So I invited Vasconcellos to write on the work of the legislature in creating this commission. I invited Mecca to write a brief essay on the work of the commission. They would be coeditors, even though I'd done all the work. It was a gesture. You might say a political or diplomatic gesture. They both were flattered.

They both wrote essays that were totally unacceptable. I really had a problem. Vasconcellos wrote about his own personal history, very lurid, in fact, and tasteless in some places, and how he became committed through Rogerian psychotherapy, and even sexual history. It was totally unacceptable. Mecca wrote an article saying that self-esteem was recognized by Plato, and then Aristotle, and then he sort of went through this very superficial skating through Western history, claiming it was the unifying variable of philosophy. Also very second-rate stuff. So I had a problem. UC Press had the book reviewed. The reviewers were hostile to their contributions, saying this book should be published, but it shouldn't be published with those in it. So I had a real problem. I called up David Gardner. I said, "David, here's what my problem is. We have these two unacceptable essays by two powerful people. We've got a problem." David said, "You have a problem." He said, "Work with it, Neil."

22-00:48:30

Rubens:

What did you think he could do?

22-00:48:33

Smelser:

I just wanted to tell him how troubled I was. This was potentially embarrassing. I didn't want him to do anything. I kept him posted. Maybe he had some ideas for me. I just took the bull by the horns and I wrote Vasconcellos. I said, "This won't do. You've got to rewrite this. This is not a personal matter that we want a statement about." And I wrote Mecca the same thing. I said, "This is not acceptable." I said, "This is unacceptable. Revise."

22-00:49:06

Rubens:

Did you give them some suggestions?

22-00:49:08

Smelser:

Oh, yes, right when I originally asked them to write. None of all this cosmic stuff that they were going through. Vasconcellos said, "Okay, I'll do it." He sent me back a revised edition which was no better than the first. He just didn't respond to me. Mecca, however, retreated completely and wrote a descriptive account of what the Commission on Self-Esteem was doing. It was satisfactory, it was fine. Just what I wanted. I still had Vasconcellos on my neck. He's a highly irascible person. I knew him from a long time ago with my previous work, previous study. He's not the sort of guy you want to boss around. I said to him, "Vasco"—everybody called him Vasco—"This won't work. We've still got to do more work." I said, "Would you let me try my hand at rewriting it?" He said okay. So I gave it to an assistant on my little

staff. I had two people, I think. I said, "This is what I want you to rewrite." Butchered it. Totally butchered it. I really was nervous when I sent it back to him. He got it. He said okay.

22-00:50:27

Rubens: He didn't say thank you or great?

22-00:50:29

Smelser: No, he said, "Yes, that's okay, do it." We basically denuded that whole thing and put it in as a characterization of how he got this legislation through and so on and so forth. It was very interesting. I had a feeling that he behaved like a pupil. When I went to him as professor and criticized his work, he just gave in. He was bossy and a complete totalitarian man in his role. Push people around and threats. A big politician, a mean politician. But he caved in on this one. I was so pleased that he did. Then the book came out.

22-00:51:18

Rubens: Well, he had a vested interest in having this—

22-00:51:21

Smelser: He wanted the book out. Obviously it was to his interest to have the book out. He might have been reasonable to give into me for that reason as well. Anyway, that turned out to be a most delicate episode that I have been in in my life. It came out. In the meantime, I went to the Russell Sage Foundation. It was published when I was there—or it got into the hands of the press, when I had just arrived in New York, in 1989. Vasconcellos held a press conference when the book came out, basically trumpeting how this supported all the work he had been doing. He used it for his own purposes. The reporters were not stupid.

22-00:52:11

Rubens: They actually read it?

22-00:52:12

Smelser: They read it. I began getting calls in New York about it and they'd quote these things to me, in which I was being very hesitant and conservative. They were baiting me to get into a debate match with Vasco over the book and start some fight. The press would be very interested in this. They were inviting me to talk about the contradictions. Can he really claim this? That kind of thing. I really played it cool. I just spoke about how rewarding an operation it was for me, and how Vasco and I had certain differences, but these were matters of shading. So I didn't really take the bait. Besides, I was a little hard to get to because I was in New York, and the reporters kind of gave up. It didn't become an issue which they played up in the response to that book. The book is still cited. It actually was a pretty good scholarly piece of work, and I was quite proud of it. I went to several meetings afterwards. The self-esteem was kind of a movement among therapists, among schoolteachers, some nurses, nursing associations. So I went to Florida and I went to a couple of meetings in California to talk about this work. It had a few aftershocks as well. It's, I

guess, cited in both some psychological literature and some more popular works on social problems.

22-00:54:04

Rubens: Did it serve its purpose in the legislature? Was there—

22-00:54:07

Smelser: No. When Vasco left the legislature, the whole movement died. It was a one-person thing. I think he got it through only because of his particular political clout. In national circles, the thing was a bit of a joke. They did have self-esteem commissions in Florida and a couple of other states, but I'd have to say the movement died.

22-00:54:33

Rubens: Do you think if it was labeled something else—I can't come up right now with what it would be, but it was an awkward name for a commission.

22-00:54:45

Smelser: Well, I don't know what I would have called it, given what the substantive preoccupations were. I think he picked up the self-esteem from the psychological literature and from these psychotherapeutic schools which influenced him. He was a troubled man, and he lived a troubled life. He sought help.

22-00:55:05

Rubens: So he's not looking for a social science solution?

22-00:55:10

Smelser: He turned what he regarded as an important personal turnaround in his own life history into a larger social cause. That was, I think, the most effective explanation of the origin. I think he picked up the self-esteem from Carl Rogers or another therapist, and that became the flag that he carried. I don't know that if he called it something else, it would have gotten—it was a very interesting thing for the state to get involved in.

22-00:55:39

Rubens: The issue of the relationship between the individual and social responsibility is very compelling and critical.

22-00:55:48

Smelser: I did not have a hostility to the importance of self-regard in conduct of various sorts. The point is, if you're trying to turn it into social science regularities or laws, you're in deep trouble, because there's a lot of vagueness about the concept. It overlaps with ego strength and all kinds of other depression and other kinds of concepts. It's unwieldy. That's one reason the research is so indecisive. It's an unwieldy concept that's never been properly operationalized. Self-esteem was a very big variable in talking about black kids in mixed schools, and the impact of mixed schooling on kids' self-esteem, and some controversial psychological findings that it was adverse effects on black kids who integrated, because they had these new comparison

groups. It made the comparisons between whites and blacks more vivid and closer. Some good social psychologists did research that seemed to indicate that, in some cases, that negative relation between school integration and self-esteem actually held. Aside from social justice issues, the psychological level, it was much more complicated a problem for the kids who get into these integrated schools and get shipped away to the neighborhoods, then come back home to the ghetto. Kids do a lot of informal sorting and also scapegoating. It was a mixed story. But anyway, it's important to mention in the whole integration experience.

22-00:57:37

Rubens:

It sounds like it was an intellectually challenging and satisfying experience.

22-00:57:44

Smelser:

I didn't appreciate all the delicacies I had interacting with Vasconcellos. It was simply uncomfortable sometimes. But I had a good intellectual relationship with that committee. They were all very responsible and serious. Rose to the opportunity.

22-00:58:04

Rubens:

Good. I think maybe this is a good place to stop for today. We're going to pick up with your textbook and the handbook and some other articles next week.

22-00:58:24

Smelser:

We'll go right into the Russell Sage Foundation.

22-00:58:26

Rubens:

Then we'll go to Russell Sage and your big book. The decade of the eighties was so jammed packed for you. We do have your work with the national labs to pick up. There is the German-American Theory Association. When is that? I don't have a date.

22-00:59:11

Smelser:

That's '82. There was a—

22-00:59:13

Rubens:

And there's three volumes that come out. So we have a lot of ground to cover.

22-00:59:17

Smelser:

Yes, that comes out of the research. It was a collaborative committee between the American Sociological Association and the German Sociological Association. We got money from various places to hold conferences on social theory. I was in the center of all three of them.

22-00:59:34

Rubens:

So I think we'll talk about that. That sounds important, and also fits in with the kind of turn to more substantive intellectual that we'll talk about next week. You've just always had amazing stamina and energy.

22-01:00:04

Smelser:

I've basically never been sick in my life. I had perfect attendance in high school. Never missed a lecture in college. Only once did I fall ill and miss a lecture at Berkeley. It was food poisoning. Crab enchilada did me in. I've had some health problems in the last few years, but during my entire academic career, I was not only totally free from illness, but energetic.

22-01:00:45

Rubens:

You weren't a runner, though? Did you say you were a tennis player?

22-01:00:52

Smelser:

No, I jogged, until my knees—I jogged through the sixties and seventies and into the eighties. I jogged over to the playing fields at the former School for the Deaf and Blind.

22-01:00:12

Rubens:

When did you fit that in? When you were through with the morning writing?

22-01:00:17

Smelser:

Afternoons, usually. Late afternoon, I'd go jogging. I just wanted to keep myself in shape. I didn't play competitive sports. I did in the sixties. I played on a scrub basketball team.

22-01:00:30

Rubens:

I think you mentioned that.

22-01:00:31

Smelser:

Heyman was on that team, and my brother, and various other people. I exercise a lot now, but that's my post-cardiac nature. I had some heart episodes, but beginning in '98, I had evidence of blockage, but it didn't call for any surgery, so they treated it with medication. Then in 2002, I had—what's it called? Angioplasty. Then in 2005, I had a stent put in. I've never had a heart attack that damaged my heart.

22-01:02:24

Rubens:

So this isn't stress, this is age, really?

Interview #12 June 17, 2011

[Begin Audio File 23]

23-00:00:01

Rubens: Good morning, Neil.

23-00:00:02

Smelser: Good morning, Lisa.

23-00:00:03

Rubens: Nice to see you. This is interview twelve. It's the 17th of June, 2011. Today we thought we would continue with activities of the eighties. There were quite a few that were interesting and disparate, but with good stories to tell. Did you want to start with your relationship to the Subcommittee on Humanism of the American Board of Internal Medicine?

23-00:00:32

Smelser: Yes. That led into a long relationship with ABIM, and it was a very interesting set of episodes. It began in the early 1980s, maybe 1980, '81, just after coming back. I had done a little bit of advising for the San Francisco campus on the place of the social science departments there. They had, over a long period of time, become more or less autonomous Ph.D. programs and had really kind of defeated the purpose for which they were originally established. That is, to feed into the medical education. They had hived off into being almost arts and sciences departments. Julie Krevans, who was the chancellor there, was very much interested in reintegrating the departments in a more meaningful way to medical training, so he had me as a kind of general social scientist come over there and talk to them. I met a hail of opposition on the part of the social science departments, who loved their autonomy and didn't want to have anything to do with the medical school, where they felt they'd been treated rather badly by the doctors' establishments. They were little enclaves, and they wanted their enclaves: medical anthropology, sociology, the school of nursing, and some other programs. In a way, that was a kind of wasted consultation on my part. It ran up against a turf situation. But I got to know Julie at the time within the medical profession, and within the ABIM, the American Board of Internal Medicine in particular, which regards itself as the most enlightened specialty in the medical profession.

23-00:02:16

Rubens: Is it an independent association?

23-00:02:19

Smelser: It's one of the certifying boards. There's Board of Internal Medicine. There's Board of Surgery, Board of Family Practice, and so on. They're one of those. They include cardiac. It's a huge and very comprehensive board. They are kind of intellectual leaders in the medical profession. The medical profession over a long period of time was, of course, under scrutiny with respect to the doctor-patient relationship. Was it becoming corrupted by third-parties, by

insurance schemes, by HMOs? Was technology coming in and wrecking the traditional culture of care and responsibility that physicians regarded themselves as having for patients? A variety of different activities within the medical profession were addressing themselves to these developments and this line of criticism. So the American Board of Internal Medicine, under Krevans' leadership, formed a Subcommittee on Humanism, they called it. Funny title, but nonetheless, they were taking the—you might say the non-clinical, the non-medical aspects of relationships between doctors and patients, and to a lesser degree, between doctors and nurses, and doctors and hospital personnel, and so on, and subjecting them to analysis and recommendation, with the charge that if we came up with some kind of meaningful recommendations with regard to improving the non-clinical aspects of the relations between doctors and patients, these would get into residency programs. Residents would actually be judged, and they would become criteria for moving them on for certification in their own specialties. So it had some meaningful context in medical education.

What happened is Krevans decided there should be some kind of representation outside of physicians themselves on this commission on humanism. By virtue of this past relationship I had had with Krevans, he recommended to his own committee, which he chaired, that I'd be a member. There was also one philosopher on the committee who was especially interested in medical ethics. A very relevant appointment as well.

23-00:05:01

Rubens: Where was he from?

23-00:05:02

Smelser: University of Washington, I believe.

23-00:05:04

McIntosh: Do you remember his name?

23-00:05:06

Smelser: Not at the moment. It may come back to me.

23-00:05:07

Rubens: We can fill it in.

23-00:05:10

Smelser: He and I were the two non-medicals. I went there with an anticipation, much the same as when I went into the National Laboratories, that I was a minority voice, probably a token, and didn't expect that I would be listened to by the physicians. I sort of had the idea that they had a sort of culture of arrogance. That's the stereotype about the ABIM people, is that they're arrogant and they're proud of it. So I said, okay, it might be an interesting enterprise anyway. We had several meetings, and our charge was to create a document specifying what the core values should be in physicians' caring relationship with patients. It turned out to be a very interesting intellectual enterprise.

There was a double aspect to it. There was a real aspect to it in the sense that these are real relations, which in some sense had deteriorated, or at least were said to be deteriorating in the face of very massive social changes. At the same time, there was a public relations aspect to it, that the medical profession was very interested in carrying on this idea of service and care and so on, and countering other kinds of views that were abroad in society about them.

The point is my anticipation about being a token and not listened to was completely wrong. As a matter of fact, I early had the feeling that I'd sort of gained the respect by bringing in various kinds of considerations that they hadn't thought of. Certainly a person who's been trained in medicine, practiced medicine, been in the medical administration and so on, loses contact with the rest of the world and the kind of ideas that are there. In a way, I was sitting there, making these sociology 101 points.

23-00:07:27

Rubens:

Like, for instance?

23-00:07:30

Smelser:

I'll give you one example. They were interested in a certain point. When it got to the point of introducing these criteria for judging residents as to whether or not they were respectful to patients, the kind of responsibility they felt beyond their own careers, and so on—these were measures they were trying to develop that they would get through interviewing and observing residents. One of the doctors raised the question, "Look, I know that we'll get this into the residency program, but we're never going to bar anybody on the basis of these humanistic criteria. So they're going to be empty." He was sort of wringing his hands. I raised a fundamental point out of Emile Durkheim about crime and punishment and enforcement. Durkheim made the point, that the actual punishment of the criminals is an incidental part of the criminal establishment. What's there is the symbolic unity of society that's expressed by the passing of laws on this given subject. So I basically gave a little exposition of Durkheim's theory. This was like they'd never heard of anything like this. It suddenly added a new dimension to this literal idea of rules and their enforcement and what it means. I brought some sociological insights into it. That's an example of how I would say things that hadn't occurred to them, and they made sense. I suppose I expressed them in such a way that they were understandable to the physicians and seemed to be new light on a subject that they'd been thinking about in a certain way, but could be thought about—

23-00:09:36

Rubens:

Countered their propensity of saying, no, no, we don't—

23-00:09:42

Smelser:

They just raised the skepticism about this, and I said, well, here's a new way of looking at it, basically.

23-00:09:46

Rubens: How many were on the committee, about?

23-00:09:48

Smelser: Seven. We met regularly. We met at the San Francisco campus most of the time, because that was Krevans' headquarters, but we met in Philadelphia, where the ABIM headquarters are, and various places. We spent at least two years on this project, and wrote a report that turned out to be meaningful and was adopted by almost all major residency programs in the country. Numbers of residents were not certified on the basis of flunking out on the humanist—they were just pills. There are pills in any training program. Even that apprehension that this would not be enforced in any way turned out to be mistaken. Some medical schools actually blocked or changed the direction of the medical trainees into pathology or other specialties that don't have so much to do with human contact.

23-00:10:53

McIntosh: For those who might not be familiar with the issues that you all were discussing in terms of the values of doctor-patient interaction, what were some of the values being discussed, other than the obvious one of the doctor should be concerned for the health of the patient?

23-00:11:09

Smelser: Respect. Integrity. Taking the point of view of the patient. Inquiring into the context of the patient's situation, above and beyond the specific symptomology that was being presented. You can call it bedside manner, if you want, as a shorthand. We put it into terms of general attitudes. We didn't really get into the conflict of interest issue, but later on, that became—

23-00:11:43

Rubens: I imagine cultural diversity as well.

23-00:11:45

Smelser: We wanted to talk about that. Very interestingly, I wanted to press their relations with other medical personnel, like nurses, but I didn't get too far on that. They've got very strict hierarchal arrangements. This was not fair game, but I thought that what I knew about the role of nurses and other ancillary personnel in the treatment of patients, that these had to be incorporated into that whole complex of what kind of humanity is brought into the setting of medical care.

23-00:12:25

McIntosh: Did this go as deep as trying to redefine what a disease is and what a symptom is for the doctors?

23-00:12:32

Smelser: No. We stayed away from straight clinical diagnoses. These were the ancillary social and ethical and interpersonal aspects. That's really what humanism came to refer to. As I say, it's not a terribly good word, because it doesn't necessarily connote those qualities, but those are the ones we began to explore

and articulate. It was a fairly eloquent document. We spent an awful lot of time on simply the style and the presentation of the document because we knew it had to be accepted by the larger board.

23-00:13:08

McIntosh:

Was this similar to sort of the trajectory of other coauthored documents that you've worked on, where ultimately you were the one doing most of the writing and most of the—

23-00:13:20

Smelser:

No, I didn't have that active role, but in the actual wording, my suggestions and wording and my editorial work, and my going over and over this—I played a major role in the style. I didn't write it, but I played a major role in the expression. Looking for conciseness, clarity, non-overlapping. The usual kind of things that I would bring to bear, I was able to, and played an actual role in the drafting of it.

23-00:13:47

Rubens:

Do you remember the name of it?

23-00:13:50

Smelser:

The Subcommittee on Humanism. I can't remember the exact title of the report. We issued it in something like '83. Six years later, we were reconvened and we revised it after a somewhat less intensive series of meetings. The committee was reconstituted, pretty much with the same membership that it had before. We came out and revised a version later. That led to a much more permanent and longstanding relationship I had with ABIM. I was asked to be on several subcommittees: One on clinical measurement and diagnosis; one on kind of reframing the general document. Kind of reframing the orientation of the ABIM in the light of technological changes. Not much came of that. But then I was put on a board. The American Board of Internal Medicine had an advisory board made up about half of non-medical people. We would meet at the same time as the board met every year. We would basically go over the agenda of issues that the board was considering, including conflict of interest, including a great deal on exactly what the certifying examination should do and be. We would, in a way, sit in advance of the board meeting, discuss all the issues. The chair of that committee then would sit with the board and be present at the board meeting. I was first a member in the early nineties, and then I became chair of that. I think in total, I was six or seven years on that advisory committee to ABIM.

Then, subsequently, I sat on two extremely important commissions of the American Board of Internal Medicine, whose reports came out around 2000. So I'm jumping ahead a little bit, but in continuity. What happened is that they formed a relationship with corresponding boards of internal medicine in different European countries. As a matter of fact, there's a European Federation of Internal Medicine that is made up of Spanish, French, English, German, other medical internists in those countries. The ABIM formed an

international commission, which was to develop a charter for physicians for the twenty-first century. It was to spell out what the commitment—again, it was not the humanist emphasis, but what the general commitments of internal medicine were in terms of new developments such as insurance programs, conflict of interest, technology, and so on. We met in New York, and then in Spain, and one other place I can't recall—Berlin, I believe—and produced the Charter for Physicians, it was called, which was published in *The Journal of American Medical Association* and received an enormous amount of attention in the medical profession. I was a member, I think one of the very few non-medical members. Maybe one or two others. Made up mainly of physicians from these countries that were involved. I played an active role in the drafting of that, the language involved in that, as well.

After that, I was put on a special commission of the ABIM on conflict of interest, in which new lines of preventing conflict, particularly with pharmaceuticals and medical device companies who had just invaded the entire medical profession, doing entertainment and getting them to endorse products. There were all kinds of scandal. Sort of a scandal of conflict of interest. Not gone away yet.

23-00:18:57

McIntosh:

I was going to ask what your current take on that was.

23-00:19:02

Smelser:

I'll give you that in a second. I was on that commission. Then, as a result of my role on that commission, the American Association of Medical Colleges in Washington asked me to be on a parallel commission on conflict of interest with them, a couple of years later. I got involved in the kind of inter-politics of the American Medical Association via this longstanding relationship with ABIM. I felt I contributed to their discussions of conflict of interest. In a way, I was tougher-minded than many of the physicians, because the profession itself was very divided on that whole issue. If you look at surveys of physicians, they all say, of course I see these representatives from the pharmaceuticals, and of course I talk to them. Of course they give me samples. Of course they take me to dinner. Of course I've been to their continuing education programs. But that hasn't influenced my judgment. Hasn't influenced my medical judgment. And of course, most surveys show that it does. That somehow or the other, they do lean toward those companies with which they have interaction. Furthermore, among some physicians, the receipt of these perks from companies has come to be regarded as a legitimate part of their own income. There's nothing wrong with it.

ABIM had some difficulty in reforming itself. I was always more of a purist on this, in a way, than most of the physicians who were present. In the last commission, there were actual representatives from the pharmaceutical industry on the commission that I was on. At a certain point, they sort of withdrew. They knew they weren't going to have their way, so they sort of

withdrew. Nonetheless, I didn't have the constraints of the medical personnel, so I was, in a way, able to talk a harder line about issues of enforcement, issues of exactly what is to be prevented with agents, what the role of the medical training centers are in this whole thing, and so on. And exactly the ways in which it contrasted with professional values. I played a role in defining the relationship between conflict of interest and professionalism in medicine. That has carried over in the lectures that I'm going to give in the Clark Kerr series. I'm going to talk about the emerging conflicts of interest because of corporate ties of academics. It's arisen in our field as well. There's no reason why it shouldn't, and every reason why it has. I'm going to bring my experience on conflict of interest to bear in those lectures as well.

23-00:22:08

McIntosh:

This is a bit of an aside, but I focused in on your statement from the last interview—I looked at the transcript—about the university system being one of the top systems for creating second-class citizens. Do you see the kind of corporate infiltration as contributing to that?

23-00:22:25

Smelser:

Oh, absolutely. It's a continuation of the same effects that federal support of research has had. Generally speaking, when you get some outside constituency, like the federal government or like state governments or like corporate interests, and they form an alliance with universities, first of all, they generally choose the best universities because they want the best talent. So it increases the wealth and prestige and fame and centrality and influence of those that are already at the top. That way, it's a reinforcement of the stratification system among institutions. Of course, the bioengineers and the biomedical people are the recipient of the 80 percent, the lion's share, of these sorts of things. It just furthers that skewing in the favor of the natural and life sciences that carried on through the whole support of federal research, which was mainly medical and mainly defense-related or hard sciences in any event. The stratification within the universities, by which the humanists, and, to a lesser extent, the social scientists, in a way became less privileged by way of salaries, perks, summer salaries, income, whatever. The corporatization, as it's now called, of the university has continued to reinforce these patterns of inequality.

23-00:24:17

Rubens:

It sounds like suffusing all of this long service is a real sense of having an influence. That you could and did have an influence.

23-00:24:22

Smelser:

Well, it was. Once again, I got sort of taken by surprise of getting into a foreign atmosphere in which I really did not consider myself to be an expert. I did no research on medical sociology, for example. My training as a psychoanalyst played a role in, I think, my being chosen. Most psychoanalysts are medical doctors. I had gone through a training program with almost entirely medical people, so that was, I think, one of the considerations that

Krevans and his colleagues must have had in mind in choosing me. Of course, that served me well, because it was a source of greater familiarity with the professional world of medicine.

23-00:25:07

Rubens:

Also, being in a position to have some real impact on direct services, almost from a purist point of view. Shaping values and—

23-00:25:17

Smelser:

Well, yes. I know there's a gratifying effect when you have developed ideas on a subject and you're able to get those ideas into a practical context or into a foreign context to your own—I felt intellectual gratification in this particular involvement.

23-00:25:44

Rubens:

Do you have something to say particularly about Krevans, just because he was such a powerful figure in the Bay Area, in the UC system? I don't know much about him.

23-00:26:00

Smelser:

He's still alive, I believe. Retired. He was an effective chancellor, an aggressive and innovative chancellor at the university. He was kind of—I wouldn't say single-handed, but he was a leading pioneer in, you might say, the ethical side, the ethical and social side, of medical practice, and a leader within the ABIM on this score. He was an aggressive leader of the committee, but not to the point of dominating everybody.

I had an interesting personal relationship with him, which I suppose I can say something about. He obviously had a high regard for me, or else he wouldn't have recommended my membership on the committee. He had told the committee that he wanted someone who was huggable, meaning a personality that was not off-putting. He said that was a criterion. He confessed to me one time. At the same time, he had some prejudices about the social sciences. Sometimes they would come out in somewhat abrasive ways at dinner conversations or over drinks or something like that. He would utter these unfounded stereotypes about social science and how soft they were and what are they really up to, so on and so forth. It kind of irritated me. We had a good relationship. Never broke it, but he had a bristly side.

23-00:27:38

Rubens:

Did he play a role in these later commissions?

23-00:27:46

Smelser:

No. No, he was not on either of those two commissions. He'd stepped down by that time.

23-00:27:56

Rubens:

Do you think we did enough on that?

23-00:27:58

Smelser:

Yes. I'm glad to have been able to get that into our oral history.

23-00:28:03

Rubens:

Yes, especially your thoughts on conflict of interests and parallels with the university.

23-00:28:07

McIntosh:

Just before we move on, could you summarize what the *Physician Charter* proposes, that you helped write that was published in 2000. Could you just summarize what the charter lays out?

23-00:28:24

Smelser:

Well, yes. It had three principles. I believe it was commitment to service. The one that interested me most was commitment to autonomy of the patient, which is something new in medicine. It goes sort of completely against the idea of doctor's orders. That everything that goes on should be in consultation with and with the permission of and consent of the patients. This was something that really struck me most of all, this new element.

23-00:29:14

McIntosh:

There's a sort of negotiation between expertise and experience there, right? The person experiencing it versus the expert.

23-00:29:19

Smelser:

Yes. Part of respect is to honor the patient's preferences in this regard. I actually see it in my own personal observations with physicians that I now have. They'll give their advice, and I'm sure that some physicians—they have said to me privately that respecting patient autonomy takes a lot more time than just deciding what's going on clinically and deciding what to do as a physician. In a way, everything is negotiated. We talked about conflict of interest.

23-00:30:19

Rubens:

Did Kaiser have a representative?

23-00:30:22

Smelser:

No.

23-00:30:24

Rubens:

Because the HMO world was so about the autonomy of the patient.

23-00:30:30

Smelser:

That was part of it. Maybe I should tell you, in regard to this, and with respect to the composition of this committee between Europeans and Americans, the Americans tended—and I excluded myself from this—the American physicians tended to take a more or less completely moral approach to the practice of medicine. A matter of commitment and calling, almost religious. Putting everything on the shoulders of the practice of physicians. I always lectured my American colleagues, saying, look, you may serve purposes, but

it's not the way it works. You're operating now within a system of medicine, and social constraints are such that the good intentions and morality and expertise of the physician are only part of the picture. That was one of the messages I completely and always took. The Europeans, you could regard them as cynical, but look, they're in state systems. They're close to being state employees. The morality, in some sense, is embedded in their situation of practice. They thought we were a little bit weird. It's sort of like how the Europeans perceive our attitudes toward political conduct among leaders. In other words, we go crazy when some guy shows nude pictures, whereas they sort of have the idea, that's part of it, this is the way they behave. That was an interesting element of that.

23-00:32:20

Rubens:

The eighties and nineties are really stepped-up times when the issue of national health care is on the political agenda as is the proliferation of HMOs.

23-00:32:31

Smelser:

Well, the medical profession has suffered so many compromises of its own autonomy and its own professionalism by the presence of third parties, by the presence of interested groups of consumers who put all kinds of pressure, by the government, who is very much interested, who is always standing out there, for example, ready to intervene into issues of conflict of interest if the medical professionals don't take care of themselves. There are all kinds of laws about conflict of interest. The medical profession absolutely dreads the feds coming in and telling them how to do it. This initiative to define and force their own standards of conflict of interest, for example, is an adaptive, and, in some respects, a defensive stance on the part of the medical profession.

23-00:33:32

McIntosh:

I just have one last contextual question about the issues that you're dealing with here. The 1980s seem to me to be a period in which issues of genetic engineering and bioethics really come to the fore for the first time. They become public issues. Did any of that discourse influence your work on the ABIM?

23-00:33:57

Smelser:

No, not so much. We knew the revolution was going on, and I've since learned that the eighties was a real watershed with respect to universities, because the passage of this Bayh-Dole legislation, by which universities could profit from their own patents. That was 1980. That was, of course, a response to our great national hysteria about foreign competition, especially Japanese. The federal government wanted to encourage much greater research and collaboration, and bring the universities into the international competitive arena. That legislation was quite a clear response to international economic concerns, and, of course, opened the flood gates for this kind of collaboration. It just so happened that the huge advances at that time were in genetics and

bioengineering, so-called, so that the flood of funds and the recipients and most of the scandals that have emerged have been in those areas.

23-00:35:05

Rubens: This is an aside, but as a practicing clinician, as an analyst, you could not prescribe medicine?

23-00:35:16

Smelser: No.

23-00:35:18

Rubens: Were you coming to look at psychotropic drugs as making a significant difference?

23-00:35:28

Smelser: The interest I took in this issue was to observe what was happening in the medical schools. The administration of medical schools in this period, even beginning somewhat earlier, drifted away—and I'm talking especially in psychiatry—from, you might say, the psychotherapeutic emphasis, which was the heritage of the Freudian revolution, to the more manipulated, mood control, pharmaceutical approach. I just observed in the occupation of deanships and leadership positions in the medical schools that drifted really over to that. This, of course, was a collaboration with the Institutes of Health, who themselves were in the same drift of technological control of medical symptomatology. That was the main interest that I took into it, partly deriving from my own interest in the rise and consolidation and fall of psychoanalytic world view.

23-00:36:42

Rubens: Did you find yourself being persuaded at all, or finding in your practice that you would—

23-00:36:47

Smelser: No, no. Whatever practice I did, I remained pretty much psychotherapeutically oriented. I didn't have the expertise.

23-00:37:00

Rubens: Or work with somebody with whom you consulted about medication?

23-00:37:04

Smelser: No, I never got into that world, personally.

23-00:37:08

McIntosh: There was a perspective floating around in the forties and fifties, maybe even the sixties. I see it as embodied specifically by people like Harold Lasswell, a sort of a Freudian state, a therapeutic state, that needs to be designed in order to release these negative impulses and things like that. Do you see that as shifting? Is there a new definition of a therapeutic state which is not a release of libidinal forces but rather a sort of suppression of them through mood stabilizers?

23-00:37:46

Smelser:

A big movement in the psychotherapeutic world, especially psychoanalysis in that period, beginning in the fifties, I would say, was the development of ego psychology in the United States, which, in a way, was a turn away from the Freudian emphasis on impulses and catharsis and acting out and so on. More into the mastery, you might say, of psychic conflict through adaptive ego processes. Heinz Hartmann was a big leader in this. As a matter of fact, Erik Erikson was the biggest figure in it. As it sort of took over the psychoanalytic movement, the impulse side of it faded in relationship to issues of mastery and control. Some people thought this was a distinctive American adaptation of it, into more in the mastery direction rather than pessimistic aspects of classical Freudian psychoanalysis.

23-00:38:49

McIntosh:

Are there other things that you want to cover from this period?

23-00:38:53

Rubens:

How about being offered the position as chancellor at UCSC?

23-00:38:59

Smelser:

Yes. Here was a discrete event in my history. It came just after I was president of the system-wide academic senate. I would like to preface this by a statement about William Frazer, who was vice president for academic affairs. Frazer was a fan of mine. He was responsible, in large part, for my being head of the general education commission. He was the one, single-handedly, who engineered my appointment into the National Laboratories Advisory Committee. He just liked my style. I think he thought I was harmless at worst, and helpful, in a positive sense of the term. He encouraged a lot of these involvements in service, especially in the university. He was also responsible for my being chair of this special committee, system-wide committee, that was constituted to investigate Chancellor Huttenback of the university campus at Santa Barbara, and to recommend whether or not his tenure should be denied as a professor.

The background is that Huttenback was appointed chancellor in 1979. I remember that well, because he came to visit me in London when I was in the second year of the Education Abroad Program. He was closely linked with Education Abroad, being in Santa Barbara, and Bill Allaway, the head of the program, sent him to see me in London. We took him and his wife out to a fantastic picnic on Hampstead Heath, which he forever remembered. We became friends with Huttenback at that time.

23-00:41:14

Rubens:

What was his discipline?

23-00:41:15

Smelser:

He was a colonial historian. He had had previous academic experience on the Irvine campus, but he had been recruited to the chancellorship. He'd been an administrator of some sort on the Irvine campus. His chancellorship was

marked by quite a bit of success in some fields. The advancement of Santa Barbara's physics department, for example, to a national standing, and a few other academic innovations. Also, the Julia Child School of Cooking. He loved eating. He loved banquets. That's why he liked that picnic we took him on so much.

A scandal developed during his chancellorship, and that had to do with his spending public money on improving his home. There was even an insurance scandal about some silverware that had been claimed to be lost, and they claimed insurance on it. It was a scandal that broke, and within a matter of months, he was fired as chancellor by the regents because of the mismanagement of funds, really was what it was. In a way, you might think the firing him as a chancellor was ample punishment. But public outrage did not cease. In particular, the system-wide office was barraged with I don't know how many complaints, but with a stream of complaints about what a scandal it was to have this man even on the faculty of the university. He, of course, had a tenured position in the history department. The system-wide office responded to this pressure first by trying to set up a committee of faculty members on the Santa Barbara campus to investigate this issue of whether or not he should be denied tenure there. They couldn't fill it. People didn't want to serve on that committee. Frazer had to turn to a different strategy, so he appointed a system-wide committee that would meet on the Santa Barbara campus and deliberate, and interview, and, in a way, hold hearings, and come up with a recommendation as to whether or not his tenure should be discontinued. A very big issue in the academic world, for sure. Frazer asked me to chair it. This was in '88, '89. I agreed. We went down there and we held hearings. It was very embarrassing to me, because Huttenback and his lawyer insisted on being present at the entire hearings. Here was this guy that I'd had this friendly relationship with in the past, and I continued to see him at regents meetings when I was on the regents. It was a nice sociable relationship that I had with him, but here he was, sitting there. I was conducting these hearings and getting opinions about this man's fitness to be a faculty member.

23-00:44:44

Rubens:

Were the committee people all from other campuses?

23-00:44:47

Smelser:

Yes. I don't remember the membership. There were maybe one or two emeriti from Santa Barbara, but that was all they could get. It was basically a system-wide committee. After these long hearings, we recommended to revoke his tenure, which I was certainly for, even though it wasn't an easy kind of decision to come to. In fact, his tenure was revoked. Their main defense was this committee has no business making recommendations on this because the criminal charges against him were still on appeal.

23-00:45:31

Rubens:

That's what I was just going to ask. Were there criminal charges?

23-00:45:33

Smelser:

Yes, there were criminal charges going on at the same time. His lawyer was there and kept hounding us, “You have no business making any recommendation whatsoever because the determination of these criminal charges are not resolved yet.” We didn’t take that argument. We just decided to proceed autonomously. That’s really all I have to say about the episode, unless you have some questions.

23-00:46:03

McIntosh:

Did they accept the decision or did they appeal it?

23-00:46:06

Smelser:

He was terminated.

23-00:46:08

McIntosh:

He didn’t sue or anything like that?

23-00:46:09

Smelser:

No.

23-00:46:10

Rubens:

Had there been precedent for this?

23-00:46:14

Smelser:

Not to my knowledge. There had been earlier cases in which tenure has been denied, often controversial academic freedom cases in which outside agencies have come into the universities, and from time to time they have buckled and fired people, and it often leads to a lawsuit or censuring of the university by the AAUP and so on. But this was a little bit different. It didn’t get into that range. This just had to do with a man’s fitness to be a member of the Santa Barbara faculty. There are stipulations in most faculty bylaws about gross misconduct and moral turpitude and things of that sort. We didn’t refer to any constitution. We just sort of did our work and made our recommendation, knowing that it wasn’t binding, but in fact it was accepted by the administration.

23-00:47:18

Rubens:

Later on, I guess, in the early 2000s, there would be the issue of the chancellor of Santa Cruz, whether she was misusing university funds for improving her house. Tragically, later on, she killed herself. [Denice Denton]

23-00:47:39

Smelser:

Oh, yes. There was that, and then there was this conflict of interest with Marcie Greenwood. This occurred mostly in system-wide. The appropriation of funds; also an issue of nepotism. Marcie Greenwood—I became an acquaintance and somewhat of a friend of hers through the National Academy of Sciences. She was on council with me there.

23-00:48:20

Rubens:

She was supposed to be quite an accomplished person.

23-00:48:23

Smelser:

I had tremendous admiration for her mind and strength of mind. I once told her the story that I had been offered the chancellorship at Santa Cruz and had determined that it was ungovernable, and that if she had asked me whether she should take the job, I would have said no. She teased me about that forever. Apparently she was an effective chancellor. I felt very bad about what happened when she went to UCOP.

23-00:48:58

McIntosh:

I don't know if it's very productive to get into counterfactuals about the UCSB episode, but I'll just do it anyways. Do you think if there had not been a sustained public protest against him remaining on the faculty, that his tenure would have been revoked?

23-00:49:18

Smelser:

Probably not. If Frazer hadn't formed that committee, there wouldn't have been any action taken. As a matter of fact, I'm confident to answer that counterfactual. The decisive element was public protest.

23-00:49:39

Rubens:

Was that being orchestrated, do you know, by—

23-00:49:41

Smelser:

I have no idea. I just knew it was there, from Frazer, but I got no details as to exactly how deep it was or how widespread it was or what groups, if any, were behind it. I don't know.

23-00:50:10

McIntosh:

So it's not that there was a crisis of confidence in his scholarship, his capabilities as a scholar.

23-00:50:18

Smelser:

No, he was an established scholar, no question. The idea of whether or not this person should be on the faculty teaching students, the role model issue came up. His general ethical conduct came up. Those were the decisive ingredients.

23-00:50:37

Rubens:

What they just talked about with the House of Representatives. Bringing shame and disquiet.

23-00:50:43

Smelser:

There's some parallel there with the recent resignation of Anthony Weiner.

23-00:50:58

Smelser:

How about going onto this expert testimony? A rather discrete idea. There was a firm in Santa Monica that wrote term papers and sold them to students, which is an ancient industry. More than that firm was at it, but this was a fairly organized firm. It made the mistake of printing out ads for term papers and putting them on the windshields of students in a parking lot. I think it was the San Diego State campus. This became known publicly and it got into a

legislator's hands, and a legislator began raising a huge stink about the thing, about this commercial sale of term papers. It's actually against California law. The education code specifies that it's a crime to sell scholarly products for use in courses. I believe it was San Diego State University brought a lawsuit against this firm, trying to close it down, in a court in southern California.

I went there. I was called in as an expert witness. Once again, it was right after I had been chair of the academic senate. I don't know how I got there. I don't think Frazer was responsible for this, but anyway, I was called to come in and testify basically to the corrupting influence of this practice. Corrupting of students, corrupting of other students, corrupting of the faculty-student relationship, and so on. So this was my testimony, and it was an orchestrated testimony that the prosecution—I was on the prosecution side. They wanted to close this place down and they wanted to fine it. They wanted to throw the book at them, really. One of the interesting things is I made contact with the lawyers who were on my side of the case. One of the interesting aspects of it was that I was asked to go to the physical premises of this firm, which was an old, closed-down motel in Santa Monica—it was really pretty seedy—and to read these papers. To read a sample of these papers. I read about fifty of them. The court ordered it. Disclosure. It was part of the whole disclosure thing, so I was able, as an expert witness, to go down and actually look at these papers. One humorous byproduct of it is I found a term paper on my own work, on collective behavior, that had been prepared for sale by students taking courses. I read that. It was a scandal. It didn't represent my work very well.

I came to learn, by reading that and a whole range of other papers, that these papers were not being aimed at the top students. As a matter of fact, they were being aimed at the "C" students to get them a "C." A "C" or a "B." These were athletes. These were marginal students. Fraternities had files with these papers around. These were the audiences, were really students who were not especially committed or ambitious, but to get them through the courses. Furthermore, I think they were nervous about giving these "A" students "A" papers, for fear that the professor would take an interest in them. This is sort of my conclusion about the thing. Now, when I came to testify, the defense, at one point—it was generally a fairly civilized procedure— but the defense took out after me, asking me if I could really tell the difference between a term paper and a research paper and a journal and a scholarly publication. I had testified, yes, these are term papers. These are written in the form of term papers. So they said, how do you know? Sort of the legal kind of probing. At one point, they gave me a copy of a manuscript, and they said, "Is this a term paper, or is this a research report, or is this an article?" It turned out that I knew the paper. It was a paper that was printed in *The Journal of American Medical Association*, and I even knew the authors, even though the authors were not—it just was a sheer coincidence. But I did not reveal that I knew it. I said, "This looks like a scholarly paper, probably from a medical journal." It was a fluke testimony that I fell into. Otherwise, I might have had some difficulty. They are overlapping in their—

23-00:56:29

Rubens:

What was their intent in giving you that kind of subject?

23-00:56:35

Smelser:

They gave me some kind of sample of a piece that was not a term paper, trying to get me to say it was a term paper. They were discrediting the witness. Really that was the aim of this. It so happened that I had a leg up by sheer accident on that.

23-00:56:50

Rubens:

I was afraid they were going to ask you if you allowed Fybates for your courses. [A Berkeley business that sold notes of course lectures.]

23-00:57:00

Smelser:

I had a very interesting episode on that in that theory course. Fybate people came around and approached me to take notes on the course. I said, okay, I'll give it to them on an experimental basis. I interviewed a student. It turned out to be a student who had taken the course the year before and flunked. They were so stupid. So I said, no, I'm not going to do this. Then they sent a better student, and I interviewed this student. I said, okay, I'll do it on a trial basis. This student took absolutely and completely superb notes. Better than I could have taken. I mean, really fantastic. They sold them the next year. I got worried about attendance for the course. It was somewhat different from what had been taken, but there was overlap. So I got really nervous about it. After that one year, I said, let's quit this. It's not really in keeping with my intention. But that's different from term papers that are faked and bought.

23-00:58:17

Rubens:

One argument for them was that the student could listen and not worry about taking notes.

23-00:58:21

Smelser:

Or the student didn't have to come to class. And I thought it was quite important that the students come to this class. That was a reason I did it once and then said, okay, I don't think this is the way to go.

23-00:58:34

Rubens:

I used to get them for several classes, but I would go to class. Many of us—

23-00:58:38

Smelser:

Some people use them as sheer supplements, and they were probably helpful. I don't know whether I object to those as being illegal—not illegal, but wrong. Just they didn't seem to work for my class. It was a situational judgment.

23-00:58:53

Rubens:

Lasted for quite a long time. I don't know what happened to it.

23-00:58:57

Smelser:

The issue of selling term papers is still endemic.

23-00:59:02

Rubens: Of course they are available on the net.

23-00:59:05

Smelser: As well as sophisticated ways of locating fake papers have also developed using computers.

23-00:59:13

McIntosh: The notetaking episode has also kind of advanced a little bit. Now that lectures are posted online often, the audio from lectures will be posted the day after. For a lot of students, the motivation to come to classes is low.

23-00:59:27

Smelser: Oh, yes. All these things undermine the traditional pedagogical understandings of classroom.

23-00:59:34

McIntosh: I've been wrestling a lot with whether or not that's an okay thing to embrace or whether it is a loss. It is a very interesting topic.

23-00:59:42

Smelser: My view is we're going to have that. There's no way to prevent that from spreading.

[Begin Audio File 24]

24-00:00:01

Rubens: I think our next topic is your involvement with the German-American Theory Association.

24-00:00:13

Smelser: Yes. This began in a co-operation between the theory section of the American Sociological Association, of which I had been chair at one point, and the corresponding section of the German Sociological Association. It makes sense. The German sociological tradition has been dominated by theoretical considerations. A couple of the contemporary world leaders in sociology, Habermas and Luhmann, were Germans. In a way, there was a drift of serious theoretical thinking toward the continent of Europe, away from America, in that whole period after Parsons. It was just one of those internal developments of the field with both French and English and German theorists picking up the explicit theoretical divisions more than Americans did after the seventies. Anyway, they decided that it would be a good idea to set up a collaborative series of conferences on selected theoretical topics, which hopefully would result in publication of books indicating this collaboration and throwing light on the theoretical issues involved.

I'm not sure exactly how I got involved in it directly. I believe maybe Jeffrey Alexander my student, was in close contact with a couple of the German scholars in the Theory Association. I was not, at the beginning. Alexander brought me in to be on planning and executing of the first conference, which

was on micro/macro link, which in the field of sociology takes the form of relating structural aspects of society and the processes that can be characterized at the structural level—political movements, political institutions, family, macro-type organizations of all sorts—and the social-psychological realities of individuals who populate these structures, and then the systematic interaction between the psychological and the social processes going on. This was an integrative interest that appealed to both sides. It might throw new light on the functioning of social phenomena and collective social action. I had been involved in this area myself a great deal because most of my early work was on the macro side of social and historical changes of institutional sorts, but I'd also gotten into the social and psychological side by collaborating with my brother on social and psychological systems. I had gone into psychoanalysis. I was very much thought of as involving a lot of social-psychological dimensions, so the topic made good sense to me. I was strategically placed to help contribute to it.

The German who was most active in it was Richard Munch who's at Düsseldorf and was a close friend of Alexander until they later had a theoretical falling out with one another. But at that time, those two were very important. They got me, and they also had—I think it was Bernardt Giessen. He was at the University of Giesen, which was a very funny little play on words. Later went to Konstanz. We were the kind of four, and we ended up being the editors of it. We had our meeting in—it wasn't Cologne. One of the German cities. I was the most strategically placed. These things do not get automatically published. They're somewhat esoteric in many respects, even though this was a fairly lively theoretical issue in sociology at the time because of what they call a microscopic revolution of the 1970s, in which the social psychology of Garfinkel, symbolic interactionists, and others really began to become front and center stage. The micro side became much more evident. It turned out to be one of the points of assault on macro sociology of Parsons and others, and Merton. It was intellectually alive. There was a lot of ferment going on in this area.

We entered the stage, you might say, post-vindictive stage of the literature and were interested more in more positive integrative statements. I wrote an introduction with Jeff Alexander and one of the theoretical chapters on how different models of micro process might articulate with understanding macro. I was the one who took the lead in getting it published, because of my longstanding link with the University of California Press. I wasn't at all confident they'd be interested, because this was not a big seller, these kinds of proceedings. There was a kind of move against readers, a move against edited volumes, in the publishing world, because they had had their day and they were not generally good sellers. For some reason, Jim Clark, he was probably the one who pushed this, and so UC Press published it. That was the first of the volumes.

24-00:06:28

Rubens:

So the conference was in '86, and then it was published the next year?

24-00:06:31

Smelser:

Yes, in '87 or '88. I was very pleased about that because it was an intellectually meaningful and fruitful conference. I was happy to take that particular role. It was generally thought to be a sufficiently successful conference that we should repeat. However, we didn't. We needed money. These two associations have no money at all. I'm not even sure how the first one was paid for. I think out of some miscellaneous German funding. We had to get money for the subsequent two. As a matter of fact, I helped raise the funds. I think from the Ford Foundation, won a modest grant. The second one was on modernity, theory of modernization, which of course had been a front and center preoccupation in the fifties and sixties, and then it became more of a—I don't want to get into all the intellectual niceties of this distinction. I don't, in fact, understand what the point was. Modernity was, particularly among European scholars, thought to be a kind of state of civilization that had evolved. It was, of course, a prelude to all kinds of thinking about post-modernization. So this was taking up the thread of modernity and bringing in scholars who'd given thought to exactly what it entailed, what were the philosophical bases of modernity, how did it come about, what were the social processes involved, and so on. I worked most closely with a German scholar. This was Hans Haferkamp. It had a tragic side.

He and I organized the conference together, which was held in Bremen in 1987 or eight, I think. At the University of Bremen, where he was a faculty member. He and I were the two organizers of this one. Not others. I wrote a chapter on external and internal impulses to modernization, which is the last chapter in the book, and I wrote an introductory piece with Haferkamp. He happened to die just in the last stages of our writing the introduction together. He died in a boating accident in the North Sea. He was a young man. Very vigorous. A very kind of wonderful guy to deal with. He was out in a boat and cracked his head or something, and fell in the water and drowned. It was obviously still a co-edited volume. I had to take charge of the logistics of publication. The UC Press also decided—I think by that time, they said, well, the first one was okay, and we'll take on the second one. Once again, a somewhat successful enterprise.

24-00:09:51

Rubens:

Could you just summarize a bit what you were saying in that chapter on external and internal?

24-00:09:57

Smelser:

It was a review of the history of thought about social change. In a way, it was a very ambitious article. I started with Spencer and the social evolutionists, whose ideas were that these were mainly unfolding internal tendencies of societies, beginning with the most primitive and leading up to the most modern. I then took the shift away from that evolutionary point of view and

the works of Marx and Engels and Lenin and Hobhouse, who were really much interested in the internationalization, the global aspect. They were the first theorists of globalization, really, to take seriously. I did a kind of dialectic analysis of the swing from internal to external, and then came back to the sort of reintroduction of the internal dynamics with the theory of modernization. That was a distinct product of the—there was a period of dearth of interest in social change, interwar, First World War and Second World War. But after World War Two, particularly with the end of colonization and the merging of new nations, the notion of internal dynamics and internal stimulus to change was the dominant feature of modernization. What are the values of the society, what are the social barriers to modernization, and so on. They're all internal. Then came a swing toward dependency theory and world systems theory, which was a vicious critique of modernization, and once again revived the Leninist world view of capitalism and imperialism. Of course, that's the informing frame of those schools of thought, dependency theory and world systems theory, which they themselves have fallen on harder times. So we're getting, now, I'd say, more of a synthetic series of investigations associated with a broader concept of globalization.

24-00:12:07

McIntosh:

Last interview, you were talking about your affiliation with Cardoso. In '86, '87, dependency theory, as I understand it, hadn't yet fallen on the hard times of it—

24-00:12:22

Smelser:

No, it was sort of a heyday.

24-00:12:24

McIntosh:

And so was your thinking kind of aligned with Cardoso's at that time or not?

24-00:12:29

Smelser:

I took a distance from all these. My own work on modernization tended to be in that phase, but on the other hand, I wrote this as a critical intellectual history. I didn't take sides, in a way, on that particular dialectic. I thought it was an interesting intellectual contribution to characterize it. I did speak about strengths and weaknesses of the different approaches that had evolved, and tried to make a sort of synthetic statement at the end. Surely, the link with Cardoso was already there. I was very much aware of him as one of the leaders of the dependency school.

24-00:13:15

McIntosh:

Within the group itself, were there clear fault lines of—

24-00:13:20

Smelser:

No, no. The Germans sort of dominated this in the sense that they were interested more in the philosophical niceties of what modernity really might be. What are its implications and so on. A lot of that got lost on me. I'm not quite a full German in my theoretical interests. Too many hairs are being split often. My piece was a kind of an independent contribution. It wasn't at the

center of the intellectual interests of that conference, but I thought it was a worthwhile thing to do in connection with the topic.

24-00:13:59

McIntosh:

It certainly seems to tie in with the works that you begin to produce in the late eighties and early nineties. I'm thinking specifically of the Simmel lectures and the interest of the macro and the global—

24-00:14:12

Smelser:

Very much so. I will have to say, though I have never put this into words, that those three German-American conferences, in a way, set me up intellectually for the book on *Problematics of Sociology* that were the Simmel lectures.

24-00:14:28

Rubens:

Let's make sure we do the third conference.

24-00:14:31

Smelser:

The one on culture.

24-00:14:33

Rubens:

Yeah, which produces *Theory of Culture*, again by UC Press. Published in '92, but the conference is when? It must be—

24-00:14:42

Smelser:

'88, '89. The last two took place in the very late eighties. I cannot give you dates. That one took place in Dusseldorf or another German city. The second one, on modernity, took place here in Berkeley. I was sort of the host of it. Had a dinner here of all the participants in this house. The third one was in a German city. It really had to do with the lingering topic of the relevance of culture as a sociological variable and the stirrings of interest in culture that had come out of the British school on mass culture, and culture as domination, and culture as expression of everyday life, the very intellectual status of culture as an explanatory variable. All of these threads were coming together, and particularly Munch was a very strong intellectual influence. Munch and Alexander, who were going in that direction themselves, were very strong. Many influences in that conference. It also resulted in a series of first-class articles. I wrote a general methodological paper on culture as construct. Most of the papers and most of the thinking were about culture as an entity. So I said, no, culture is something that, in a way, we bring to our own studies. I tried to explore all the implications of culture as an intellectual creation on the part of the investigator and what that meant as to what we made of it as a part of society. It was a critical article, in many respects, of the anthropological traditions of culture. That was my chapter.

24-00:16:54

McIntosh:

It's interesting to see these three conferences come together, because, at least from what I understand of the micro-sociological revolution, which I think, in the Simmel Lectures, you also said was a failed revolution as well in the

seventies, that culture was seen as a site of symbolic generation and the creation—

24-00:17:17

Smelser: Construct of interaction and meaning. Meaning systems created by interactive essentials—

24-00:17:28

McIntosh: Which seems to at least ascribe a bit of agency to individuals, as opposed to seeing culture just as domination. Why do you think that, in the third conference, it was the culture as domination model that was being kind of pushed by a number of the participants?

24-00:17:50

Smelser: I have a feeling that was the residual influences of Habermas—he was not there. Habermas was not there. The residual quasi-Marxist, neo-Marxist implications of Habermasian thought, to say nothing of Marcuse, were present. And Gramsci, especially about culture's hegemony. It had informed European thought in general during this era. I have a feeling if you'd asked me the question, that I would appeal to that legacy as being present at the conference.

24-00:18:32

Rubens: Now Habermas had moved beyond a more simple Marxist—

24-00:18:40

Smelser: Oh, yes, yes, yes. Habermas was a very complex thinker. No question. He did—

24-00:18:46

Rubens: Does he become a foundational person in deconstruction?

24-00:18:49

Smelser: Well, yes. Not so much as the French. He really was a political philosopher more than anything else and was very much interested in the changing significance of culture and ideology and the patterns of dominance in, I'd say, post-industrial West. The part of Marx that he never lost was the idea that there was a dominant class and there was an exploited class. He built it into his whole notion between the bureaucratic, technological structure on the one hand, and the life world on the other. But it really was an echo of the Marxian world view, even though he rewrote both Marx and Freud extensively in his own work. He came here. I got to know Habermas. He came here once in the eighties, and I was sort of his host, in the sense that I had been the theoretical teacher in the sociology department forever. Habermas was jointly appointed by the sociology and the philosophy departments, so I saw a lot of him. We had several meals together. I brought him into our discussion group at the institute during his stay. I went to his presentations and became sort of friendly with him. I found a kind of lack of intellectual communication or synergy with him, largely because—

24-00:20:40

Rubens: Did he speak English well?

24-00:20:44

Smelser: He was a very difficult man to understand. His presentations were lost on most of the audience, and parts of it were lost on me. He was so normative in his outlook, rather than, you might say, analytic-empirical, that we would try to discuss things and he would always take an evaluative point of view. I was perfectly happy to talk about the evaluation aspects of it, but I always would tend to frame it in terms of, how do we understand this? Throw some light on it. Habermas was forever thinking of it as either shocking or corrupt or some other evaluative mode. It was a kind of tension in our relationship. I wouldn't say we really properly interacted with one another intellectually, even though the personal relationship was one of mutual respect.

24-00:21:39

McIntosh: His project, as I understand it, it's an ethical project.

24-00:21:46

Smelser: Yes, to create a good society out of the life world.

24-00:21:50

McIntosh: Right. That's why it's interesting that you classify him as a political philosopher, because I see parallels between Rawls's project and his, which is, let's create a normative model which we can then use to judge particulars by, whereas you've always kind of been a little more empirical.

24-00:22:12

Smelser: He had a kind of notion. There was a kind of general will imagery. He's quite romantic about what the life world—he might say the real world of real people—interaction they're really capable of. I always was suspicious of his endowment of that kind of positive power to the masses, if you will. I picked up Habermas and I used him. I assigned him in my theory courses. He and Bourdieu, I routinely assigned in my theory classes.

24-00:22:45

Rubens: I was going to ask you just now about Bourdieu. To what extent does his kind of web of social experience, the habitus, get taken up at the last conference?

24-00:22:57

Smelser: This is a German-American conference. I daresay that the substance of Bourdieu's work, while certainly we were all aware of it, it wasn't a major thread in this. His work on culture and culture capital did not play a role.

24-00:23:24

McIntosh: I guess just to get back to the culture as domination thing, that's a clear legacy of Habermas's Frankfurt school era and the—

24-00:23:32

Smelser: Neo-critical.

- 24-00:23:33
McIntosh: Model. Right. But it is interesting. It seemed like, within this conference, you were trying to keep alive the other side of things.
- 24-00:23:44
Smelser: Yes, yes. It was an eclectic conference. I wouldn't want to say that there was any domination of any sort. So that was the episode.
- 24-00:23:54
Rubens: How was that book received? It comes out in '92.
- 24-00:24:00
Smelser: Oh, who knows? You write these books and they go on to the big world and you don't know how they're received. You get occasional reviews. It was attended to. Put it that way.
- 24-00:24:13
Rubens: This is another tangent in reference to French thinkers. Foucault was on campus.
- 24-00:24:32
Smelser: Oh, yes, he was here. I didn't get to know Foucault. I have to say that I never resonated with post-modernists much at all. My basic attitude is skeptical and remote. I think that is enough on the German-American thing. It was a good episode.
- 24-00:25:23
McIntosh: Are there other specific episodes that you all would like to cover? Otherwise, I have a kind of more contextual question.
- 24-00:25:32
Rubens: You were going to talk about the trip to Russia before we got to the text.
- 24-00:25:35
McIntosh: This actually ties in right to that. We're right at 1989. In rereading the transcript from last time, you were talking about the actual difficulty that sociologists had communicating with one another during the Cold War. I think you were talking specifically about some International Sociological Association—
- 24-00:26:00
Smelser: Yes, the Cold War politics.
- 24-00:26:02
McIntosh: Right. 1989 is obviously a watershed date for that. I'm wondering if international communication within the discipline became—I don't want to attach any sort of preemptive description to it. Did it change at all after 1989?
- 24-00:26:23
Smelser: Oh, yes. I will talk about my trip to Russia in this regard. It's directly relevant. In 1987, I was asked by the University of California to head up a delegation to

go to Leningrad State University, which it was still called at that time—we called it LSU in jest—to set up an exchange program between Russian and American students and faculty, between the University of California and Leningrad State University. It was on the model of the education abroad program.

24-00:26:56

Rubens:

Who asks you to do this?

24-00:26:58

Smelser:

Frazer was behind it. Frazer was the original head of that committee. He got waylaid by some very urgent university-wide business and couldn't go, so he asked me to take his place. This was a blow to the whole proceedings, because as status-conscious as the Russians were, they felt a little bit insulted at the beginning, because the head of their delegation was the rector of Leningrad State University. Number one person. Frazer, of course, was something of a corresponding—no, no, Frazer was okay. But here I was, professor, right? Frazer had to engage in a public relations campaign with them to convince them that I was a big superstar and head of the senate. All this stuff. They finally gave in and I was accepted as a true leader of our delegation. There was a politics side to this visit. I'll tell you, it was very funny, because it really was a super imposition of the Russian model of decision making on the whole procedure. At the negotiations for this exchange program, which we hammered out all kinds of details—it was a productive meeting, and we established a program that turned out to be short-lived.

Anyway, here was Merkuriev, who was the rector, and here I was. We were sitting across a long table, at the center of a long table, looking at each other. They had a little American flag pointing at me, and a little Soviet flag pointing at him. Our delegations were lined up to the sides of each of us, on each side of the table. The rules of the game that we discovered were that it was up to Merkuriev and me to do the serious talking, and not the other people to talk with one another. That there was this chain of communication on my side of the table, a chain of communication on his side of the table, and we would then do the serious business. It was this very authoritarian sort of assumption. Of course, that's the way they set it up, that's the way we did it.

24-00:29:15

Rubens:

How do you know this? By observing what they're doing and then—

24-00:29:20

Smelser:

It seeped into our procedures without much consciousness. They knew what they wanted and so on. Anyway, we got this hammered out, more or less. They ended up by leaving a few I's undotted and a few T's uncrossed so they could have a delegation come to Berkeley later so we could finish it off. They wanted a trip, I think, to the United States, which happened later. They did come here. What struck me in the informal conversations which I had with the faculty members at Leningrad State University, mainly, and a few of the

administrators, was how free they were in talking with us. '87, Gorbachev time. Already the ice was melting. In particular, they were interested in my status as an economic sociologist. This was a kind of field that was, somehow or other, assuming almost a kind of reform movement. I was kind of regarded as a representative, if not leader, of American economic sociologists. There was a lot of interest.

In 1989, I was actually interviewed at the airport by a reporter about economic sociology. Somehow or other, it had become part of the Gorbachev kind of mentality as an avenue to reform. Reforming economic institutions. Anti-Marxist in its orientation. There was a woman by the name of Zaslavskaya, who was the intellectual leader of this, stationed in Novosibirsk, but then later came to be Gorbachev's adviser. She was an academic economic sociologist. I met her. We talked together. I was especially struck by the beginnings of overt anti-Marxism among the people present, which, of course, they'd been subordinated to for decades. This was this great birth, and there was almost this whole idealistic commitment to some kind of free inquiry into economic institutions that was going to be, somehow or other, the basis of reforming of Russian society. Very, very great enthusiasm and so on.

Then, in 1989, I was invited to go back to lecture. There was a lecture team set up by the Social Science Research Council, with the Russian Academy of Sciences. They collaborated. They chose something like four or five American lecturers. Harvey Molotch from Santa Barbara was one of them, Barbara Hynes from NYU was one of them, and I was one of them. I gave several major lectures in Moscow on American sociology, on American society, and was attended widely by Russian scholars and students in the Moscow area. It was held in the Komsomol, which was the old Communist youth organization. It was a very elegant building. They spent a lot of money on training young Communists in the Communist era. We used that— Sharin took a picture of me in which I look like a little ant lecturing, with a statue of Lenin behind me that was about fifty feet high. It was very funny.

That was ironic in that I had a wave of anti-Marxism that was present in that audience. It was unbelievable. Because I included Marx in my talking about the history of American sociology, and the marginality of Marx, and where he showed up and where he didn't show up. The audience was just foaming at the mouth in terms of its rejection of Marx. I made a joke, because this was at the end of the solidarity period in which Eastern Europe had sort of broken away. I made a joke at the end of the question period that the remaining Marxists in the United States were to be found in the graduate student population of Berkeley and Columbia. The rest of the world had given him up. That was a wholesale, almost, I'd say, irrational, turn against the Marxist world view which characterized, of course, prelude to the rejection.

24-00:34:01

Rubens:

Which you could see through the question and answer period?

24-00:34:04

Smelser:

It was not hard to see. They were trying to persuade me to be more anti-Marxist than I was. That was one of the agendas on part of these questions. Long question periods. Actually, the sessions were typically Russian. They lasted six hours. You'd lecture and then you'd get into a discussion, and you lecture, and discussion, and so on. I got plenty of input from the audience.

24-00:34:30

McIntosh:

Just for the sake of documentation, can we be explicit about why the sub-discipline of economic sociology is seen as being anti-Marxist? Is it because it takes social forces and the human personality as being primary before the forces of capital?

24-00:34:49

Smelser:

It does not commit itself to any special mode of domination in society. Economic sociologists themselves are radical and anti-business, and regard the business-industrial-political complex as being decisive and worth exposing, but that's just one thread. By and large, the impulse of economic sociology has been critical of orthodox economics, not Marxist economics, largely for its artificiality and assumptions about rationality and assumptions about atomism of the individual actor and that social institutions play a role in dictating taste, behavior, conflict, market processes. It's more of a polemic, in a negative sense, on the world view of economists, and an attempt to substitute a world view that personal interaction, social institutions, cultural understandings play a role in dominating economically relevant action. As a matter of fact, industrial sociology, which was an earlier version of it, in the thirties and forties, tended to be sort of pro-capitalist in that it took the worker situation and took a somewhat manipulative point of view as to how to handle worker motivation to get greater efficiency. That was rejected post-war. Economic sociology became primarily analytic and did not have a single ideological thrust to it. It's diverse and eclectic from the standpoint of perspectives.

24-00:36:40

Rubens:

Is that what you'll argue in the book you finished at the Russell Sage Foundation?

24-00:37:04

Smelser:

Oh, no, that's a different orientation, different tradition. It could be said to have an economic dimension to it, but it wasn't in that thread of discourse that I just summarized. The Russians picked it up. They gave it a special anti-Marxist twist, as you might expect.

24-00:37:24

McIntosh:

It seems like economic sociology as a discipline occupies this unique space in the post-World War Two era of being both anti-Marxist and kind of anti-free market. Or at least having the potential—

24-00:37:36

Smelser:

I wouldn't call it anti-Marxist. Marx doesn't play much of a role. Marx came in and really began to inform the reaction against industrial sociology in the sixties and seventies. I have to say that many of the people who called themselves economic sociologists at that time, including Michael Burawoy my colleague, really did take on a more radical, more Marxist point of view. That Marxist-Weberian splurge in the seventies was very short-lived. Economic sociology, intellectually, is one of the strongest fields in sociology. A lot of really talented people have gone into it. My own criticism is that they don't pay enough attention to theory, but they tend to go in their own eclectic directions. I've spoken about that in several essays. Nonetheless, it and cultural studies and gender studies have been the vibrant sub-fields in sociology since 1980.

24-00:38:49

McIntosh:

To get back to the issue of your interactions with the Russian sociologists and with your international presence as well, would you say that there is a convergence among sociologists internationally in the late eighties and early nineties towards—

24-00:39:06

Smelser:

Yes. As the Cold War was winding down, you still got these divisions, but they were shifting. I'd say a new alliance was forged with Russia and that it would play a role in late eighties. After that, it began to play a much more minor role in the international scene, from a scholarly point of view, even though the field was liberated in Russia by the revolution, in a way. By the nineties revolution. The internal politics of the International Sociological Association are more—actually, the third world has assumed a larger role in things, and the degree to which European sociologists align themselves with third world ambitions. Then we have the standard social movements of race and ethnicity and gender playing a big role in it as well, which is not a national expression so much as an international social movement expression.

I'll just say one thing about the rest of that 1989 visit. I went to Novosibirsk to give a presentation. It was a very funny presentation because Zaslavskaya had been there and she then went to Moscow with Gorbachev, but she had trained this generation of economic sociologists, all of whom showed up for my lecture. Fifty, sixty, seventy. My book had not been published in Russia and was not really bona fide. My text on economic sociology came out in the sixties. But they had gotten it, so I was a known figure in their lives. As a matter of fact, Zaslavskaya, her political enemies, mostly on the left, accused her of not being a bona fide economic sociologist, but that she stole my work. She was attacked, publicly in Russia, for basing her own work on economic sociology on mine. I heard about this when I talked with these people in Novosibirsk. It was a very weird experience for me to go out to that part of the world.

- 24-00:41:31
Rubens: Was she a generation younger than you?
- 24-00:41:33
Smelser: No, she's my age. I think she's now dead.
- 24-00:41:38
McIntosh: Well, true or not, there's still a kernel of fact there that your influence was widely felt, it seems like.
- 24-00:41:49
Smelser: The reception I got on those two trips indicated that I was a known figure.
- 24-00:41:56
Rubens: Did you publish these lectures that you gave on American sociology and American society?
- 24-00:42:00
Smelser: No, none. No, I didn't. They're still sitting somewhere.
- 24-00:42:07
Rubens: I bet they're very interesting.
- 24-00:42:09
McIntosh: Yes, they should see the light of day sometime.
- 24-00:42:12
Smelser: Maybe. Maybe I'll look them up.
- 24-00:42:15
Rubens: Do that. I don't know if they filtered in some way into *The Handbook of Economic Sociology* or *Handbook of Sociology*.
- 24-00:42:23
Smelser: No, no, they were discrete pieces of lecturing that I gave and then didn't do anything more with.
- 24-00:42:32
Rubens: We don't have a lot of time left today. So do you think we'll do short shrift of the text or should we start at least talking about your text?
- 24-00:42:39
Smelser: I can tell you about the background of the text in the time that remains. It was not initiated by me. I came back from the Education Abroad Program. The people at Prentice Hall, where I'd been adviser for fifteen years—as a matter of fact, that was in London that they informed that I was no longer adviser. They had decided to economize. I was getting 2 percent of every book that published in my series. They just went without them. They used ad-hoc advising on books. The first and only time I've ever been fired, I think, from any position. When I came back, Ed Stanford, who was the sociology editor, approached me, obviously with some trepidation, because he didn't think I

would do it, to write a text. A text that would find the niche in the market as being an intellectually serious text, but not so advanced that it wouldn't be used in introductory courses. Probably aimed at the better colleges and universities in the country, I would say. That was the way they advertised it. He'd obviously prepared a big case to convince me to do it. Think of all the royalties there are going to be, and all of that. He had it all worked out. He really wanted me to do it.

I kind of took him by surprise, and maybe myself by surprise, saying, yes, I'd do it, more or less immediately. If you look at the academic world in which I've lived, the text is an odd product. No intelligent social scientist, maybe no intelligent academic, would want to build his reputation on writing a text. It's regarded definitely as a second-class type of publication. Useful, maybe. Helpful. Enriching, possibly. But not serious scholarship. It's kind of like being an editor. It's relevant to your career, worthy of doing, but not prime product. I don't know. It's the sort of thing one would accept if you feel pretty well-established anyway. In other words, I didn't envision it in the way of extending my reputation, nor did I think my reputation was such that I would damage it by doing a text.

They had an idea of what they were then calling a managed text. This was being done by a lot of publishers. Of getting the author to provide the subject matter, but it would be handled in the office by professional writers that they had. This was thought to be a way of getting yourself articulated into the undergraduate market. It was foreign to me. I didn't like the idea. I said I'd do it, but I want to be responsible for the text. That was the big condition. I'm willing to accept rewriting. My philosophy in life is that people who rewrite generally help, particularly scholars who are in their own worlds and are not especially good at communicating. So generally, I've been very friendly to editing of my own work, and have usually thought it does it good. But I didn't want it written for me. They bought that. We established this kind of open-ended idea that I would be open to editing but I was going to write the text. I had taught general sociology. I generally made an effort, more than most sociologists, to cover the field, so it wasn't as though I was going to have to—kind of revolutionary conquest of whole new literatures. I felt I was positioned in such a way that I could do it.

It's interesting that, for the first and only time in my life, I wrote this text by dictating. All oral. I'm not sure why I did that. These dictation machines were just coming into effect. I had a fulltime secretary who transcribed it all. I thought it might be an efficient way to do things. It was a terribly exhausting way to write a book. Terribly exhausting. To have that many things in your head, to organize it. I wasn't doing it to make it conversational. I was just doing it for efficiency purposes, and that's the way I did it.

24-00:47:35

Rubens:

You had an outline?

24-00:47:37

Smelser:

Notes. I outlined in my mind of what I was going on to. But I would dictate sometimes even on a trip to a conference. I would dictate a whole chapter. I redid it, re-edited, and transcribed, the whole thing. It was a book that was done in that mode. Nothing like anything I've ever done since or before. I think maybe I got so tired in doing it that I decided not to do it again. Prentice Hall made a big thing about promoting this. They sent me to Florida for their sales conference. I appeared and I talked to them. Hyped them all up the way these publishers do. They get their own sales representatives hyped up. I told stories about writing the book so they could tell people they were trying to sell it to that they had met me and all that kind of stuff. Kind of a PR element there.

I discovered that in writing a text, there are constraints about what you can put into it and how unique you can make it. Those constraints lie with those people who teach introductory courses. These texts all become pretty much the same. The chapter headings are almost all the same in the text. Levels and styles are different, of course, but there's kind of an orthodoxy in writing these texts and the topics you have to cover and the chapter headings you have to use. They were not foreign to me. They represent the field. But it struck me what an orthodoxy there is in the introductory-text world. My book sold well.

24-00:49:10

Rubens:

You wrote it in a year, I think.

24-00:49:12

Smelser:

Oh, yes. My decision to dictate turned out to be an efficient one. They gave me research assistants as well. I had a couple of research assistants that I could send to locate pieces of literature that I wasn't exactly familiar with. I knew they were there, but I was not confident in writing about them without knowing more about them, so I used my research assistants to help me out. That was a source of efficiency, you might say. Prentice Hall gave it quite a splash. It wasn't very long before they immediately wanted a second edition, and then a third, and then a fourth. I think the fifth—

24-00:49:55

Rubens:

'81, '84, '87, just for the eighties.

24-00:49:58

Smelser:

Yes, then '91, '95, maybe. The fifth edition was in 1995. I was thoroughly sick of it by this time. Nothing like rewriting things again and again.

24-00:50:09

Rubens:

And you did, for each edition?

24-00:50:10

Smelser:

Oh, yes, there were substantial rewritings and bringing of new materials to bear. Basic outline didn't change too much, because that was the nature of the product. It kept selling well enough that they wanted to have it. It fit into the

niche that they wanted it in. It began to get translated into other languages. It got into Italian. It was translated in the early nineties into Russian. It has recently been pirated and translated in Mongolian, I learned two days ago when a Mongolian scholar came to look me up and wanted to have his picture taken with me and give me a present and have me sign a book. I'm really well-known in Mongolia, if you're interested in that extension of your reputation. That was by virtue of the Russian translation. They translated from the Russian into Mongolian. This chap sort of hounded me to visit me. He's at the Mongolian National University. That text, I guess I would have to say, was meaningful. This guy from Mongolia was flattering me all the time about my text and what a great book it was, and I was a little bit embarrassed about this, because I don't consider it the best of my works. I still have the same idea towards texts that I've always had. Nevertheless, this turned out to be a successful enterprise that I stuck with. I almost said no to the fourth and fifth editions, or to get someone else to do it.

24-00:51:50

Rubens: To add their name.

24-00:51:51

Smelser: Add their name as coauthor. But it was always my book. At the end, I did say to Stanford, "I don't want to do anymore. I've had enough."

24-00:52:02

McIntosh: But a very successful run, it sounds like.

24-00:52:04

Smelser: Yes. I suppose I got more popular exposure through that book than I would have gotten otherwise.

24-00:52:12

McIntosh: As an author of a textbook, do you get data on what schools adopt it for their classes? Do you have any idea who is using it?

24-00:52:19

Smelser: Not systematic. I got it from time to time. It was where they wanted to sell it. University of Minnesota, University of Iowa. State universities with large undergraduate sociology enrollments.

24-00:52:32

Rubens: Was there a competing text?

24-00:52:34

Smelser: Lots. Mostly less advanced. I don't think this was used at all in community colleges. Rarely, I would say. I couldn't write a text at that level. I just was incapable. Prentice Hall knew that, so we struck this deal on getting to the upper range of the introductory market.

- 24-00:53:03
McIntosh: I read it, and it is very clear. This is just a curiosity, but I'm wondering if dictating it caused—I noticed that the sentences are all really pretty short. That tends to be a stylistic trait of yours in general, but it stood out to me even more in the textbook.
- 24-00:53:28
Smelser: That might have been traceable to the way I did it. Also, they did edit it. They're very professional. They ran it through. Have you ever seen this program on computers that will read text for you and tell you what mental age it addresses? It's pretty sinister, actually. It turned out to be higher than they wanted. I wouldn't call it dumbing down, but they did. I did get editorial rewriting.
- 24-00:53:55
Rubens: Did you have any choice over covers? I thought the fifth edition cover was so distinct from the—
- 24-00:54:02
Smelser: No. No, I didn't. By and large, authors, unless they raise a terrible fuss, don't have much authority over covers. I didn't raise fusses.
- 24-00:54:14
Rubens: There was an article that I was looking for that you wrote that I couldn't find. "The Textbook Dilemma." It was in a teaching newsletter of ASA.
- 24-00:54:26
Smelser: I forget what I said in it. It is in my bibliography. I think it has to do with representing a complex field to an uninformed audience.
- 24-00:54:51
Rubens: Did you want to say anything more on the text or do you think we did a—
- 24-00:54:56
McIntosh: I don't have anything else to ask about it.
- 24-00:55:00
Smelser: We'll talk about the handbook, a couple of other projects I did, and then we'll build up to this big monograph I wrote. I'm glad I got these miscellaneous bits that I remembered.
- 24-00:55:17
Rubens: Is there anything particular to say about the search committee for the SSRC?
- 24-00:55:22
Smelser: No. I was just on it and we chose a person. There was nothing memorable.
- 24-00:55:29
Rubens: Same thing with being a consultant to the Nobel Prize committee?

24-00:55:36

Smelser:

I was asked to be a referee in the appointment of Gary Becker, the Chicago economist.

24-00:55:44

McIntosh:

Controversial economist.

24-00:55:45

Smelser:

Very orthodox. They chose a wide range of people, I think, to evaluate his work. I evaluated it and recommended against his appointment. He's a target of economic sociology. I recommended against it, but the Nobel Prize committee did not take my recommendation. That was a one-shot thing. I don't think it's very important.

24-00:56:14

Rubens:

Well, all right. Let's call it a day.

Interview #13 June 21, 2011

[Begin Audio File 25]

25-00:00:02

Rubens: Hi, Neil.

25-00:00:03

Smelser: Hello, good morning.

25-00:00:04

Rubens: This is our lucky thirteen interview.

25-00:00:27

Smelser: Oh, okay, we'll try to make it special.

25-00:00:09

Rubens: Jess and I would like to I start with your service to the National Labs. You are first appointed in 1988. Who appoints you?

25-00:00:37

Smelser: This was a system-wide appointment. What was called the Scientific and Academic Advisory Committee to the labs, SAAC, they called it. This was in existence as part of the university's responsibility for managing labs that were in the contract with the Department of Energy for a long, long time. In the late 1980s, the renewal of the labs came up again. Of course, every time they came up, there was always a lot of stirring on numbers of campuses against it, that the university shouldn't be aligned with this war machine and so on. It came from the left. This was a time, in the late 1980s, of stirring of the anti-lab sentiment. I did not have a fixed position. I was not emotional about this issue, one side or the other. I wasn't in the Seaborg camp that thought it was absolutely, totally essential to the livelihood of the nation that the universities be responsible for this lab, nor did I have the left-wing disgust with the war machine. I thought it was a ticklish relationship, but I never got into the politics of it.

Once again, this was an act I would put at the hands of Bill Frazer, because he was the system-wide officer who was directly responsible for the supervision of the labs. Frazer had already seen me in action in the senate. He'd already appointed me to this review of lower division education. I was his boy in the social sciences, you might say. One of the complaints about the labs that came up in this protest period was this SAAC committee is a joke. It's just a bunch of physicists who go up there and have chummy talks with their physicist friends, and it isn't management or supervision at all. It's just our little gesture, a phony gesture. It wasn't the biggest issue. The biggest issue was the link with the Departments of Energy and Defense. But that was one of the complaints. Frazer decided to do a little something about this, so he asked me to be on the committee, the Scientific and Academic Advisory Committee to the labs, which met three times a year, one time for each of the three labs that

were under the university's supervision, and then, occasionally, more often, when a special issue would come up at one of the labs. We might be convened to deal with a special issue. So three or four trips a year. One wasn't a trip, it was right here. One was to Livermore and one was to Los Alamos.

25-00:03:31

Rubens:

The third one, here, being LBL?

25-00:03:34

Smelser:

Lawrence Berkeley Labs. It was a special case because it didn't do security work. The most ticklish area of academic relations in the labs was in the area of security, in my estimation. Anyway, Frazer chose me. I was ambivalent about taking this position. Not for the political reasons, because I didn't have those kinds of qualms about it, but I thought I would simply go out there and come to believe that that accusation was true, that the whole thing was a bunch of physicists talking physics with each other, and I would be totally out in left field, from what I could understand about what was going on. But I said okay. My work with the academic senate, beginning in 1987, required a clearance because the regents were responsible for the labs, and they were all cleared. I was, in effect, a regent, so they initiated clearance procedures on me when I became the faculty representative to the labs. High-level clearance, Q clearance, which is one of the highest in the federal government security system. Perhaps I can say a little bit about that along the way. But I decided to join. Also just kind of finishing up my academic senate stint at that time as well.

25-00:05:20

Rubens:

This is 1988?

25-00:05:21

Smelser:

That's right. In the first year, my apprehensions about what was going on turned out to be absolutely right. I was lost.

25-00:05:36

Rubens:

How many, about, on the committee?

25-00:05:41

Smelser:

Ten or eleven. I was the only social scientist at the time. Later, they extended this and involved the senate more heavily, but at this time, I was the only social-science faculty member on the lab committee. I basically didn't have anything to say. I'd learned about 10 percent of the content of what was being said and was quite interested in a few policy issues that spilled out, and I didn't keep my mouth shut the whole time. But I have to tell you, I was sort of lost and I wondered what was going on. I got into some very interesting material because the labs were involved in the Star Wars research. Heavily funded in the late Reagan administration. I learned a lot about federal defense policy and I had some things to say about it. The scientists themselves on the committee were somewhat critical of many of the lines of research that were going on in the defense against nuclear missiles. They thought it was,

scientifically, nonsense. So that was a very interesting dialogue that I got in on, but I couldn't contribute much to it because of the technical basis of the arguments.

25-00:06:55

Rubens:

Just one question of clarification. These physicists are not faculty members, they're from the labs?

25-00:07:01

Smelser:

No, they're UC members. Herbert York was one. He was a very famous physicist at UCSD. We had a chap from the University of Illinois. Some of them worked for Berkeley that weren't from the UC campuses. There was one from the honorary physical sciences society, Sigma Xi. It was a mixed bunch, but all physical scientists. The second year, 1989-90, I said to the labs, "I'm not going to serve this year because I'm in New York." Actually, I'll come to that, cutting off all my other ties at that point. So the second year, I said I wouldn't serve. During the second year, of course, it was the end of Communism. I didn't say that I resigned from the committee, I just said, "I'm on leave for this year. I'll come back." They said, "That's fine. We'll do it that way." I think they didn't think I was contributing much anyway. But the whole scene changed after the collapse. As you recall, defense spending just went like that. The labs had a moratorium on original research. The budget of the labs began going down because of their specific relationship to defense and warfare. They were experiencing an organizational crisis.

One day at the meeting, I said to the group—I already had a sense of humor about this—I said, "I think the labs are now coming around to me." Meaning that I'm a student of organizational crises, and maybe I could be of some help. As it turns out, I knew a lot more about organizations than these physicists, who spent their lives in other ways. They weren't uninformed. This was not their bag. My whole relationship with the labs changed as they began to meet new issues with respect to security, with the Department of Energy, as they began to try to diversify their research. That's one thing they did. Once they were losing all this defense money, they adopted a whole bunch of new strategies in laser research and medical research. They even got into the terrorism business later. Receiving money, designing deterrent mechanisms for terrorist attacks, and so on. They even set up a cooperative program that was ill-fated with private business, technology sharing with private business, for a while. It didn't work. Congress killed it after a while.

Anyway, I was in an area in which these policy issues were coming up, including ongoing bleeding issues about the relationship between security and publication, and employment of graduate students, which they basically did not do because they had to publish their dissertations. All these issues that were, in a way, very familiar to me, both from my way of thinking and from the kind of research that I had spent a lot of my life doing. I gradually came up as a kind of a person to be consulted with respect to some of these

organizational and policy issues. I actually, I would have to say, over time—I wouldn't call it a leadership role, but I assumed a very responsible role on the labs, just because of the changing circumstances of the labs. That's what I meant by my joke. They came to me, and they were now discussing issues that were really social science issues. Organizational adaptation. What are we doing here?

25-00:11:08

McIntosh:

So, in essence, your contribution was taking the labs themselves and restructuring them?

25-00:11:15

Smelser:

No, I didn't have any responsibility for their policy directions, but I had a lot of responsibility, as an oversight committee should have, for criticizing and raising questions about these, and throwing insight on some of the problems that these new involvements would cause. As you can see, there was much more of an articulation of my interest and my way of looking at the world, and what the labs were going through at the time.

25-00:11:44

Rubens:

Just to clarify, you do take a year off, '89-'90, and then you're back, but the name of the committee changes, but you're on the newly named entity, '92-'98.

25-00:11:55

Smelser:

Yes, that's when it became the President's Scientific Advisory Committee.

25-00:12:02

Rubens:

As just opposed to the Science Advisory Committee to the President? It's just a different name?

25-00:12:08

Smelser:

They expanded its numbers. Midway through—

25-00:12:12

Rubens:

They got some social scientists?

25-00:12:14

Smelser:

No, that was only a little bit later. That was, I'd say, '94, '95. I stayed on, even after I was at the Center. Then they wanted to appoint me to another five-year term, and at that point I said no. That's just too much. I was just taking over the encyclopedia at the time. *Encyclopedia for the Social and Behavioral Sciences*, which we'll talk about later. So I resigned. My service term had come to an end and I said I didn't want to be re-appointed. But they wanted me back. The scientists on the committee gave quite touching commentaries or speeches at my last meeting. I felt it was a thing in which I was able to develop an engaging and actually helpful role, even though it was a sort of foreign land. I wrote up some things in my essay on governance about the labs, particularly about the drain that they were on the university, and about

the difficulties of interpreting the term “management of the labs” from the standpoint of the university’s role.

25-00:13:38

Rubens: Do you think the university should sever its—

25-00:13:41

Smelser: They’ve already been highly modified since my time. The University of California has kept its ties there, but not exclusively.

25-00:13:57

Rubens: Did you advocate? You said something you thought the university should do, that it should weigh in its—

25-00:14:04

Smelser: I argued not that it should get out, though I wouldn’t mind if it did because of the administrative problems that it’s got itself into. Doesn’t get much money from the Defense Department. It’s, in one sense, a losing proposition. Some system-wide administration is dedicated to those labs. A tremendous amount of time is taken up in that. Then they get into these extremely difficult and confusing roles. I’ll mention only two of them. At one point, after I came back on the committee in the early nineties, the Department of Energy, which was itself under pressure from Congress, as these agencies that crack down often are, began sending out teams to inspect the labs. They called them tiger teams, and they were. They were tigers. They were especially interested in security, but they began checking everything. Safety. If a plug was three inches too near another outlet, they’d write it down. They were just driving the lab people crazy with this intervention. I likened it to the Soviet Union, these Communist Party groups that would come around and be always a second parallel tier to the organization. These tiger teams were very mischievous. In the end, the labs sort of rebelled and said you can’t do this to us. This is paralyzing us. Let’s prioritize what you have in mind. Let’s at least have a sense of the important things and the less important things. They achieved achieved some sort of truce.

25-00:15:59

Rubens: I meant to ask why the left would be critical of the relationship between the labs and the Department of Energy?

25-00:16:05

Smelser: Because the Department of Energy was the one that took over most of the funding for weapons development. A lot of it. It was under two agencies. One, Department of Energy, one, Department of Defense. They just didn’t like it, federal establishment in general. In a way, it didn’t matter what agency it was. We were in bed with a war-making nation. So they didn’t focus on the Department of Energy—the left didn’t.

This other case that happened just after I left was this physicist Lee, who was basically charged with treason. It was a security break. The feds came after

him like crazy. But the university got a lot of blame for it. The university was itself picked out as one of the mis-managers of the labs.

25-00:17:04

Rubens: He had taken a computer off the premises?

25-00:17:07

Smelser: He'd taken the computer files home. It worked out so that he got charged, he got tried, but the government handled it so heavy-handedly that the judge threw the case out. They convicted him, but he served no prison time. In any event, I said to the former director, Sig Hecker, when I saw him at a social gathering later, I said, "It seems to me this Lee case resembles the O.J. Simpson case." I said about the O.J. Simpson case, I quoted a joke that the L.A. police department was caught red-handed, framing a guilty man. He looked at me a minute. He said, "I think you're right." Anyway, the university got sucked into that thing and took a lot of flack, even though they were not responsible for the security arrangements at the labs. This question of shared governance came up. As it turned out, the university committee served a very valuable role in making scientific assessments of the work in the different departments of the lab. That's what we did. A lot of our time was to prepare, for the Department of Energy, reports on the scientific excellence of the research. Scored them. Actually evaluated them. That was, I think, the role that the university could play positively, and it was the right role.

25-00:18:43

Rubens: Did you have a hand in shaping those evaluations and writing them?

25-00:18:49

Smelser: No, I was not on the technical committees. I don't think they should have put me on the technical committees. I would have been incompetent to talk about laser research, for example. We did discuss them. I discussed the methodology of these rating systems, which was—yeah, I took a critical role. It turned out that the president's advisory committee tended to suffer from grade inflation. That is to say they tended to grade everything excellent, because they didn't want to give these other places a bad name. I raised criticisms about this. I said, "We're not doing our job if we don't make discriminations about quality." So we would have these discussions. Not that I had a big influence on it, but raised the flag on what I thought was not the right way to do things.

25-00:19:41

Rubens: What did you get out of doing this? Did you enjoy this?

25-00:19:45

Smelser: Well, yes. I have this philosophy. It's in the same way I enjoyed being with the medicals. Your life as a professional sociologist gets somewhat repetitive. You write and you do this. You teach. You know what's happening from year to year, and you don't get into much unfamiliar territory over time. Not entirely true, but largely true. Here was a case where I entered a new world, which I fully expected to be treated like a foreigner, and to which I didn't

think I could contribute anything. It was a reversal of those expectations that created a great deal of pleasure for me that I could play a positive role, that I didn't have to declare defeat because I wasn't expert in the areas in which these people were expert, but could contribute to the ongoing problems. Any organization will have organizational problems, and they all have policy problems, and they won't know what to do with them. They'll get all kinds of uninformed ideas floating around. So I felt I could play a role in this dialogue, and did. I think the idea that they get non-scientists on there was a good one.

25-00:20:59

Rubens:

Plus it was tied into such high-level policy issues.

25-00:21:03

Smelser:

I didn't really feel myself part of the power establishment. I suppose you're closer to the center of things than you are as a professor in Berkeley. That particular version of being involved in power—I didn't take special pride in that, no.

25-00:21:26

Rubens:

Is there a story about the clearance?

25-00:21:28

Smelser:

Oh, yes, the clearance. It was really funny, because my term as member of the regents was two years. My clearance came through two months before that period was over. In other words, the effective value of this clearance was two months, because I was technically not cleared during the period before that time. Every neighbor in this area was interviewed. My ex-wife was interviewed. I was interviewed. An extremely hostile interview, in fact.

25-00:22:12

Rubens:

Who are the interviewers?

25-00:22:14

Smelser:

FBI. About a third of the interview was on my father's politics, because he was a member of the AFT [American Federation of Teachers] when AFT was just beginning. He was very much in the liberal wing of the faculty at his own college. He was a very outspoken man. Some local barber or somebody had called him a Communist. So the FBI got all interested in my father's politics, and this, that, and the other thing, and interviewing me in a very hostile way about what his politics were. My basic attitude is I didn't know much about my father's politics. I knew what his general position was on things. Oh, but this didn't seem to impress them. Talk about guilt by association, because they were talking a lot about my father. They also had a complete misunderstanding of my role during the period of student protest. They asked, "Why were you seen so often in Sproul Plaza?" I tried to explain. Do you understand I was a peacemaker in this period? Either they were feeding me misinformation to see what I would do with it, or they were incompetent. One of the two.

25-00:23:36

McIntosh:

Were you forced to directly articulate what your politics were?

25-00:23:44

Smelser:

No. They didn't ask me where I was on party preference or anything like that. Some of the interview was outright stupid. One of the questions was, "Ah. We see that you've traveled to Bulgaria. Who did you talk to in Bulgaria?" I answered, "International Sociological meetings were in Varna. We talked to social scientists around the world." Then they said, "Suppose someone at one of the meetings asked you about the labs, seeking information about the labs. What would you do?" I said I'd report it to the State Department. The correct answer, obviously. Then this guy, who seemed to me to have a really low IQ, said, "Well, suppose they offered you money." I almost said, "How much?" But I said, "Oh, I'd report it to the State Department." This kind of thing. In the end, I was upset by the interview, and I called up David Gardner. I said, "Is this the sort of thing I should be subjected to for this?" He said to me, "If you feel strongly about this, you can withdraw your request for clearance, and it all dies at that point, and you simply won't be able to be present in those meetings they describe as classified." So I didn't do that, and about two months later, the clearance came through, this top-level clearance, that I was judged not to be an enemy of the country. I didn't like it.

Then I was re-cleared several years later because they had to renew the clearance. This was just into my term at the labs. The Cold War was over, but they got real interested in my trip to Moscow. "Did you befriend this person? Who showed you around?" "These were graduate students in sociology. One later came to Berkeley as a graduate student, and we saw her and her family socially." "Why do you see these people socially?" They were in the Cold War mentality still. I never came close to not being cleared, but I was discouraged by the way it went and the level at which it was conducted.

25-00:26:23

McIntosh:

It is a ridiculously invasive process. I've had family members go through clearance as well. They call up everybody in your past.

25-00:26:31

Smelser:

Oh, yes. They certainly justify themselves as an agency. I can't imagine what the cost of this clearance was in terms of manpower. I used to get approached a few times by the FBI about students of mine, too, asking me about their politics and their associations. I was so mad at this whole business that I almost always said, "Not to my knowledge." That was my absolutely standard response of being an unhelpful informant, because I kind of didn't like the whole business. Anyway, maybe I've said enough about the labs.

25-00:27:14

McIntosh:

I just wanted to, before we completely close the book on that, just clarify, your reservations about the university-lab relationships—they weren't political reservations. It was just—

25-00:27:27

Smelser:

They were practical, administrative, operational. I saw these problems that the university had, and the headaches the university was getting, and the small rewards they got for them, and the undeserved criticism they got for them. It did come up for renewal at the time I was on the regents, and of course I was non-voting, so I didn't get a chance—but I suppose I would have been a little torn. I think I've explained exactly what my basis for reservations about those ties was. I was not with the activists. I was impatient with the far left on that.

25-00:28:15

Rubens:

Well, should we turn to your years of service on the external reviews?

25-00:28:21

Smelser:

Yes. Here's an institution that departments have and universities have. Periodically, sometimes on a scheduled basis, every five years, they will have a review of a given unit. A department, a school, an organized research unit or something. Usually, this takes two forms. The unit will evaluate itself and submit a self-report, but on top of that, they appoint an external committee, usually made up of five to seven people, and made up of people from other campuses or other universities around the country. It's one of the routines that you expect to happen if you're a person of note in the field. You will be called upon to conduct these reviews. I did quite a few of these in my life. I wrote down the ones I could remember. San Diego and Santa Cruz campuses of the University of California, University of Washington, New York University, University of Iowa, University of Hawaii.

25-00:29:32

Rubens:

What about Harvard and Yale?

25-00:29:34

Smelser:

Those are separate. Those are not the simple, one-shot reviews. These are one-shot reviews in which you come for a day and a half and there's a standard set of interviews that you have.

25-00:29:45

Rubens:

You meet with a committee?

25-00:29:47

Smelser:

The whole committee. I usually chaired these committees, which seemed to be my fate. We would meet with the dean. On occasion, with the chancellor. Review what they saw as the problems with the committee. We'd meet with the chair. We'd meet with the individual faculty members on the committee and interview them on different subjects about their undergraduate teaching, their own views about the quality of the department and what needed to be done. We got all this input and then we would write a report. A few weeks after the meeting, if I were the chair, I'd draft the thing and send it around to my people and get it okayed, reviewed the usual way, and submit it then to the deans. Then you lose control. It's their business to do what they want.

We made some fairly radical suggestions. I will mention UC San Diego just as an illustration. San Diego was a department of sociology that came in on the tail of the original founding of San Diego, which was mainly hard sciences. But they'd wanted to be a general campus, so they developed departments and programs in political science, economics, and the usual range. Most of the social science departments were very what you call hard. Quantitative. Very much in the keeping with a scientific model. But for some reason, the San Diego sociology department had gone off in a soft direction. A social psychological direction with ethnomethodology and critical sociology playing a role. They barely were able to teach a course in research methods because of the composition of their faculty. It was my general view over time that a department never gets to be first-rate if it tries to carve out just a little niche for itself and excel in that. Stanford suffered for twenty years because they focused on small group research, on highly scientific methodology, and became very proud and very defensive of their leadership in this area, and they turned out to be a poor place for sociology graduate students to go to, unless they wanted that particular approach. My view was already formed, that any first-class department has to be a general department.

I got my committee members to agree with this. It wasn't a big job, but I did. We wrote a very forceful report saying that if this department doesn't strengthen itself in research methods and in certain substantive areas like stratification, political sociology, and so on, they're going to remain at the cut below excellence. Of course, the San Diego faculty didn't want to hear this—or if they heard it, they didn't want it to happen. So there was, in fact, a reform that took place over years, of diversifying that faculty, even though there was resistance in the faculty to bringing in types that they didn't especially agree with from the standpoint of their world view, and their views on methodology, and their views on where the field should be going, and so on. There was then a conflict on the campus about this, but in the end, San Diego improved its department enormously by diversification.

That's the kind of role you can play. Sometimes the situation was, in a way, beyond your control. The review at Santa Cruz comes to mind. It was a hopelessly conflicted department, left, right, and center. People weren't talking to each other, and the graduate students were choosing sides. It was all a complete mess. What can you recommend? Stop fighting? You can't. We made a standard group of recommendations about the way in which the department could strengthen itself substantively. We made a few comments on its internal divisions, but it was a tough one. In fact, it was a very interesting review. We saw the self-review, which covered everything over. It was just a polite, self-congratulatory review.

25-00:34:29

Rubens:

Is that written by the chair?

25-00:34:31

Smelser:

Yes, but the department was all complicit. In a way, they were trying to hoodwink any other reviewers or the administration. We went down there and we began interviewing faculty members. The dike broke. They all began singing their songs about their grievances, about other faculty members. Some were punitive towards graduate students. On and on. We got the whole, whole picture of the department there. I was a bit frustrated because it was, in a way, very hard to make recommendations for it.

25-00:35:08

Rubens:

Was this exacerbated by the college system as well?

25-00:35:12

Smelser:

No. Santa Cruz culture is an extremely divisive culture generally. One of the big splits was between a very sizeable far-left group in the faculty, and a kind of centrist group. That was the main source of contention, so it was politicized in the same way that the Berkeley department was politicized, but it was small and hopelessly divided. The quality of the faculty members wasn't as high as it should have been.

25-00:35:49

Rubens:

Another reason why you weren't chancellor there. I know it didn't appeal to you.

25-00:35:55

Smelser:

I did tell David Gardner the place was ungovernable. That was my reason for not accepting.

25-00:35:59

Rubens:

I never asked about external reviews while you were chair.

25-00:36:08

Smelser:

Of our department, you mean? None of them coincided with years of my chairmanship. We had a few. A couple, as I remember, in which I was interviewed. They turned out to be somewhat inconsequential. They were basically fairly favorable, with cosmetic changes to graduate program and things of that sort. No, I wasn't on the tough receiving end of any of these.

I gave you two dramatic cases of two UC campuses. All the departments I went to had problems. Iowa had drifted into an applied mode, teaching a lot of criminology and a lot of applied courses—I think probably to boost its number of majors. It had also fallen into a line of specialization that was kind of unproductive. We made much the same comments about Iowa as we did about San Diego. If it were really to move itself forward, it had to do this. I didn't follow up on it. I didn't see what the consequences were of this. I was at Minnesota, too. That occurred to me. I was on a Minnesota committee.

25-00:37:29

McIntosh:

Other than a department being diversified, were there other qualities that you thought every good sociology department should have?

25-00:37:37

Smelser:

There's a joke about these reviews. I shared this joke with my committee. As we sat down to meet, I said to my own committee, "I've got the report written. It's in my back pocket. It says, you need to change here and here and here, and the main thing you've got to do is to get more FTEs." These reports tended to get stylized. The successful department chair will do his best to push his aims through the review committee. There's always a kind of interesting context. You're proud of your own field. These were all sociologists. We were, in that sense, kinsmen with the department members, and we were all proud of our field. You don't want to damage your own field or have administrators be punitive to your own field. That's one of the motives you have. There's a kind of tribal quality about membership in a professional association. We didn't want to simply echo what the chairman wanted, which was usually more resources. That's the standard chairman's orientation to things. In some cases, it's a justified request, because the department was undermanned. It was too small. We did, in fact, on some occasions, call for expansion of the department's resources. And that's also the diversification—entails expansion. We didn't always tie it to diversification. When I get to Harvard and Yale, we talk about yet another intellectual variant of this.

25-00:39:17

Rubens:

Just one more question about these reviews. Did you know if your textbook was used? Did you encounter people who wanted to talk about it or who did—

25-00:39:26

Smelser:

About my work?

25-00:39:27

Rubens:

Well, the textbook particularly. Who was using the textbook?

25-00:39:33

Smelser:

No. My own personal status or standing as a sociologist didn't seem to enter into these things.

25-00:39:45

Rubens:

You had written *The Changing Academic Market*, about the whole process of hiring and expanding.

25-00:39:52

Smelser:

That's right. There may have been a mention of it, but it was very incidental, my own work that might be relevant.

25-00:40:01

Rubens:

I think Harvard comes first. Well, Harvard and Yale are both the same year. '88, you started Harvard, through the nineties, and Yale, '88 as well.

25-00:40:11

McIntosh:

Yes. How were these experiences different than the standard one shot review?

25-00:40:15
Smelser:

Here was a problem. In both Harvard and Yale, the departments were really beleaguered. Harvard had experienced a period of exodus of senior faculty to other institutions in sociology and an inability to hire replacements. It was accompanied by a lot of internal division in the department, plus the fact that the level of citizenship in the department was very low. Most people were involved in research centers or other departments or other schools, and their citizenship in the department was marginal. They sort of gathered there to fight. These were the general symptoms. One day, a couple years before that, a Harvard committee formed. Henry Rosovsky, the dean of the college, the most powerful person in the university, came out to Berkeley. He and I were friends from way back. We were in the Harvard Society of Fellows together. He was my best friend in the Harvard Society of Fellows. He came to Berkeley the same time I did. During his stay here, which was about five years, we remained closest of friends. I was heartbroken when he decided to go to Harvard because we were so close, but we maintained a relationship, and still do, though it's much diminished now.

He came to me. He made a point of coming to my house. He was in a fit about the sociology department. He said he just didn't know what to do. He said he was just on the verge of declaring the department abolished. I think he was maybe overplaying it, but he was clearly at loose ends. Or he thought of putting it in a receivership. He had very radical ideas—because he had been driven to kind of the end of his rope, I think, by the circumstances of the department at the time. So I had this very honest conversation with Henry, and I said, "Really, I don't think you should do anything that radical. It won't help you. It will make Harvard look bad. It certainly won't help the field of sociology. What's to be gained by this very dramatic axing?" There had been a few axings around the country. Syracuse, Washington University at St. Louis had killed their sociology departments, only subsequently to reintroduce them, either under the same name or under some different program. But nonetheless, it was a field, not like geography, which had been dying department after department after department around the country, but sociology had a couple of cases in which the administration—Washington University at St. Louis had become a totally kind of destructive Marxist department through hiring and was a completely negative influence in the university politics. Syracuse, I don't know the story, but that was one of the other universities that experienced that.

So I said to Henry, "Don't get the department reviewed the way these one-shot ones are, but why don't you set up a committee, which is, in effect, a receivership, but don't call it that. Call it something like external advisory committee to the dean. That committee can operate over a period of time to try to improve the appointments." Henry bought it. For this brilliant suggestion, I got the invitation to chair it, which often happens when you come up with an idea. They say, okay, you do it. They put on Robert Merton and Bill Sewell, two really senior statesmen in the field, and Buzz Zelditch

from Stanford. He happened to have been a fellow graduate student with me. He was over on the hard sciences side with that Stanford group, the original Stanford group, but I wanted someone who was more on the quantitative side, because one of the criticisms of the Harvard department was that it had gone soft. It was now getting into ideological debates. It was now going into, roughly speaking, comparative studies and the analysis of ideas and stuff, and there was no real solid core of disciplinary sociology there.

25-00:44:52

Rubens:

When you had not taken the offer to replace Parsons, who did? Was there someone who represented that?

25-00:45:02

Smelser:

No. You can say that people like Daniel Bell would overlap in some interests and so on, but there wasn't a single theorist. They didn't appoint a single theorist after that.

25-00:45:14

Rubens:

Did you appoint this committee for which you were now chair?

25-00:45:24

Smelser:

Yes. I recommended them to Henry and he bought all the recommendations. They basically seized the power of appointment from the department and gave it to this committee. We were to come up with a new range of appointments to strengthen the department. This also involved diversification. Increasing the representation in things like stratification and organization and demography, just to get it more mainstream, to balance it out from the kind of isolated intellectual commentary. Someone once called the Harvard faculty a Jewish Bloomsbury, just commenting on the state of the world and so on. Intellectuals, rather than social scientists, in a word.

25-00:46:24

Rubens:

Were you also charged with affirmative action?

25-00:46:28

Smelser:

No, but we were very much aware of the affirmative action dimension. We recommended both women and minority candidates, as I recall.

25-00:46:38

McIntosh:

Did you have the power to fire as well?

25-00:46:39

Smelser:

No. No, they didn't get into the business of getting rid of anybody. That was too much for any university to do, to fire tenured faculty. I guess if you disbanded the department, you could have done that, but it just raised that whole realm of academic freedom, of tenure. They avoided that whole nasty range of issues by just saying, we're going to authorize three or four major appointments in this department, and you should be responsible. Much of our work was, in fact, searching, identifying, and recommending to Rosovsky the appointments. He accepted every one of our recommendations as to who to go

after. Their batting average was not high, but they did appoint, over the course of years, three or four people, and changed the character of the department. I think I was on that committee for four years or five years. I'm not sure. We would meet with Henry. I insisted on keep meeting with the department. I knew there would be really bad blood. The department accepted this external committee. They couldn't resist the dean, so they didn't say, disband it, disband it, we'll do our own business. They accepted it, and in fact they accepted our recommendations. But I made a point of wanting to meet with the faculty, with the assistant professors, sometimes with graduate students.

25-00:48:10

Rubens:

As a group or individually?

25-00:48:12

Smelser:

Both. Those were political moves on my part. Just keep people incorporated, even though you were doing things that they rightfully thought should be in their power. That's the most important thing a department could do, is appoint new members. I got a call from the *New York Times*. The *Times* is always very interested in Harvard and all the big Eastern universities. The reporter was very direct. He said, "Are you trying to make a positivistic hive out of this nest up there?" Really putting it to me. "Are you going to quantify this department?" He was really giving me the hard-line questioning. He was quoting other people who said we were just turning it into kind of a sociological, quantitative workshop and so on. I had fun with this reporter in evading those kinds of accusations.

25-00:49:24

McIntosh:

Did you feel any conflict between your allegiance to the Berkeley department and, for instance, in seeking and hiring new promising sociologists?

25-00:49:35

Smelser:

No, no. Absolutely no conflict of interest. There was also the possible overtone that I had been courted by Harvard three times, and I turned them down every time. In our original interview, Henry Rosovsky eased that issue by saying, "Well, Neil, we know there's no problem here with you. You've turned us down several times, and I'm very sorry that you didn't join us." He just eased that situation. No, I would have to say that I identified with Harvard's problem wholly. I didn't say, well, gee, if Harvard gets this person, we couldn't get him. If that person is on the market and we think they might come to Harvard, ah, maybe we could pull a little move here. It never occurred to me to do anything like that.

25-00:50:19

Rubens:

Or your own students? Did you have some students who—

25-00:50:22

Smelser:

No. They wanted senior people. We may have considered a couple of them, but they were not among the ones that we came to recommend.

25-00:50:39
McIntosh:

Who were some of the names that you came to recommend?

25-00:50:44
Smelser:

They appointed Aage Sorensen from Wisconsin. They went after Bob Hauser from Wisconsin, who declined. They went after David Featherman, who was at Wisconsin. He declined. They went after Nancy Tuma from Stanford. She declined because they couldn't offer her husband a job. He was a teacher in the local state college. He hadn't really published anything. He taught English. There was no place for him. It was kind of an interesting thing. A little joke connected with this, because he was also a minister. Her husband was also a minister. Harvard went so far when they were courting her to try to look around for a possible congregational ministership for him in the Boston area, just because Harvard couldn't take him on as a faculty member. There was no way, because he didn't have a scholarly record or anything of that sort. One of my committee members said, "This seems like a case of publish or perish." So she didn't come. There was a chap from North Carolina who did come. His name will come to me in a moment. So there was a batting average of maybe a third, which is about what you might expect in cases like this, but it did in fact make a mark on the department. Later, they hired Theda Skocpol, in a very disputed tenure case. I turned out to be on the external advisory committee for that appointment. It was a tough case for Derek Bok and Henry, because she was threatening to sue them for sex discrimination. It was a very bloody case. In the end, she did get appointed, but mostly in political science. That was just another subsequent and separate episode.

At a certain point, I said to Henry, "I think our work is over." This was after several years. "There have been several appointments. I think we've made the dent that you wanted us to make on the department." We met maybe every six months. It was an active committee. In a way, it was easy, because I was working with such agreeable colleagues on my own committee. We had the greatest mutual respect for one another. We didn't have any fights at all in terms of what our priorities might be about recommending people. The department, in a way, was forced to take our recommendations, but they didn't fight them. I never interviewed Dan Bell or others who were there, but I think they might have been a little bit relieved that we behaved very responsibly. We didn't go wild or far out or make any totally radical suggestions. We just thought it should be strengthened, and we put our best efforts to it.

Let me start on the Yale one in the time we have left on this tape. Yale was another department which was under heavy fire. Yale was under heavy fire financially at that time. There were cuts. It was one of the down points in Yale's fundraising history. They decided, instead of making cuts across the board, or limiting salary raises or something, they were going to go for departments. A dean of humanities, Frank Turner was especially aggressive in this regard. He was a historian. Another provost and the president got in their

minds they're going to cut selected departments. They made several recommendations to slice back engineering, which was a weak department at Yale, to cut out forestry, to cut out sociology, and maybe to cut out philosophy and statistics. These were the targeted departments.

It so happened that I had been an advisor to Yale sociology for years. I first turned down an invitation to join their faculty in the early sixties, and Kingman Brewster took a liking to me, so he would call me up. Sometimes see me when I was on the Yale campus. They tried to hire me twice. They had a poor department, as Ivy League departments go. It was way below the level of their other departments. Of course, the culture at Yale doesn't place the social sciences very high, with the possible exception of psychology and political science. Sociology was, in a way, a kind of scapegoated department in the administration's eyes. They picked it out on this basis. It so happened that the year before they decided to make this move, I was appointed to a five-year visiting committee. Not to do with the department. This was just a Yale visiting committee that was responsible for taking a look at psychology, anthropology, and sociology, keeping ongoing tabs on these departments and making recommendations. It wasn't a receivership committee. It was a well-institutionalized visiting committee arrangement at Yale. It happened to all departments. There was nothing unusual about it.

My second year coincided with this move to get rid of sociology. Immediately, that recommendation came to us. I was cast in a very interesting role because I was the only sociologist on that—two of us. Me and a woman named Pepper Schwartz from Washington were the two sociologists. We played a real role. I opposed the killing of the department and proposed an arrangement something like Harvard's, the external committee to try to see it through. In the end, my suggestion worked. Not that I was on it. I was on this overseeing committee, or visiting committee, but I recommended the formation of another committee to superintend its improvement. As it turned out, the one man that we had brought to Harvard, the guy from North Carolina whose name I can't remember, chaired the external committee and I sat on it for a while. The improvement committee.

It was very, very, very delicate. The hostility to the department on the part of the administration was great. It turned out to be the completely wrong move on the part of the Yale administration because it triggered a faculty revolt. It's extremely hard for institutions to pick out units and say, you're going to go. It always excites the next question: who's going to be the next to go? So this faculty solidarity comes together, even though I'm sure the scientists and a lot of the humanists didn't like sociology at all, or philosophy, or whoever it was. They banded together. I joked. I said, "You wanted to get rid of five departments. Instead, you got rid of three administrators." Turner left, went to William and Mary. Kagan resigned. President Benno Schmidt resigned a few years later. Not all directly because of this faculty revolt. Turner, yes, he just

left. Kagan, who was his strong ally, sort of left as well. Benno, who was in other kinds of trouble, left a couple of years later.

The sociology department responded beautifully. They made several really strong appointments in the subsequent years. It's really raised its ranking in the nation. It was somewhere always hanging around twelve, fifteen. Maybe it shouldn't have been that high, I thought, because they really just hadn't done very well by way of academic appointments and maintaining their quality. They were beleaguered within the institution. Being hounded all the time.

25-00:59:32

McIntosh: When was Jeffrey Alexander appointed?

25-00:59:35

Smelser: He was appointed later. They got the chair from UCLA, a Hungarian sociologist, Ivan Zelenyi. He later, then, as chair, hired Alexander. Alexander's appointment was a couple of years after my work with Yale was over. While I wrote a very glowing letter of recommendation for Alexander—he was my student—I didn't have any direct role. The advisory committee at Yale didn't have any direct role over appointments. It was a little less intrusive than the Harvard one.

25-01:00:16

McIntosh: Alexander seems like a strong appointment. Who were some of the other people over the next few years?

25-01:00:23

Smelser: A sociologist by the name of Roger Gould, who unfortunately died of cancer a few years after his appointment. An extremely strong appointment. Zelenyi from UCLA, who came into chair it. He was a first-class sociologist. They formed this core, and Alexander started this Center for Cultural Studies there, which is probably the leading one in the country. Insofar as I've followed their appointment policies, those are the names that come to mind. They just moved into a different league.

[Begin Audio File 26]

26-00:00:06

McIntosh: I just wanted to follow up with one question about the Harvard case, which is, I'm curious about how such a formerly prestigious department in a very prestigious institution drifts so far away from a sort of solid core. We know what the symptoms were, but I guess I'm wondering what you think the causes of a drift like that can be.

26-00:00:36

Smelser: It's very hard to be absolutely precise, because I don't know the full answer to your question. Harvard had an especially individualistic culture among faculty members. I'm talking about departments through the institution as a whole. They are full of prima donnas. It drove Henry Rosovsky crazy. I used to be his

therapist on this matter. Sociology was no exception, even when it was in the social relations department. There was a lot of division in that unit. They said, well, it should stay in the social relations department, 1970, when the move came to form a separate sociology department once again. Their motive for forming a separate sociology department was that there were too few sociologists in this one, big department. They felt they were just starved for numbers. In the end, even after they split, it remained a relatively small department, as first-rate departments should go. In Harvard's own pecking order, the social relations department was not high. As a matter of fact, it was thought to be an "easy" major. There was this general view about it as not being held up to being really at a Harvard standard. There's a lot of mythology there and a lot of snootiness on the part of other faculty members and so on.

It was always the case that Harvard appointed stars. Their appointment system has almost always been really tough on assistant professors, not many of whom ever got promoted. They would go to people who were well-established in the field and bring them in. Already having some muscle, you might say. I think this always would contribute to some internal prima donna-ish division. A sense of your own privilege, your own place, and so on and so forth. Also, probably to a diminished sense of collective responsibility for the department. These were people with national reputations. Commentators in the world and so on. David Riesman was an example of this type of appointment. Parsons did a very good job, for many, many years, holding that department together, pretending it was unified in a way. When Parsons left, there was a lot of squabbling. They would try, after, to replace him with me, but they ended up not getting anybody. A couple of their good people joined institutes, taking refuge in them and participating only casually in the departmental—I hate to use a clichéd term, but there was just a decline of community in the department. That was more evident than all the fighting. It was just a malaise.

26-00:03:52

Rubens:

In the end, leadership does play a role, doesn't it? There is no one to command citizenship, engender respect or loyalty.

26-00:04:05

Smelser:

They didn't have anybody who wanted to chair it. It was this kind of floating thing. Henry was right to be discouraged, because it wasn't really behaving.

26-00:04:17

McIntosh:

That concept of citizenship is interesting. I never thought about that in terms of—

26-00:04:22

Smelser:

That's what occurs to me. We had enough of that in our own department here. Maybe I was sensitized to it. It was more drift than conflict there, though. There were conflicts, but more drift than conflict.

26-00:04:42

Rubens: Sort of summing up this decade of service, you wanted to talk about the absence of recompense.

26-00:04:48

Smelser: Yes. I'll have to tell you that one of the remarkable things about this period was that, of all these ranges of service that I've described in these last couple of interviews, I got paid for none of them.

26-00:05:10

Rubens: They had to cover plane tickets and things.

26-00:05:12

Smelser: They always cover your expenses. There is a philosophy, you're never out of pocket. But on the other hand, you don't profit from it in any way either. The entire National Academy assignment was without recompense. The Academy has an absolutely strict policy of not recompensing their members. I wasn't a member yet, but that policy carried over to those of us who were on their committees and still not members of the National Academy. I didn't get anything for the service on these external review committees. It's thought to be part of your professional duties. It's a funny story. For the Santa Cruz external review committee, they offered a modest honorarium, a few hundred dollars, to members who weren't in the UC system, but those in the UC system were supposed to get nothing, on grounds that this was part of their commitment to the University of California. The joke here is that they accidentally mailed me an honorarium for it. Then they wanted to get it back and I said, "Are you sure?" They just gave up and let me keep it. It's very interesting. I never raised the question with any of them, either, about some kind of payment for this, because if you just think in terms of the time involved, any one of these assignments was very, very big over time. I just sort of felt it was either pointless or unethical to raise questions about this. Of course, every time you read a manuscript for a publisher, you get some money for it. Sometimes if you give a speech at another institution, they give you an honorarium for it. But I sort of liked the idea of not getting paid for these assignments.

I must tell you one story that brought this highly into my consciousness. In about 1990, I suppose it was, I was written a letter by the dean of the law school in a major Eastern university, asking me to review a person for tenure. I had done this a lot. This is part of your duties. Sometimes anybody who's wanting to promote a local faculty member to tenure or to full professorship, they will get a series of external reviews so they can defend the recommendation to the administration. I wrote many letters on behalf of colleagues around the country who were coming up, assessing their work. Mostly you would give a green light to the move to promote them, but in some cases, you raised reservations about their scholarship. None of this was paid. It was all assumed to be part of your collegial responsibility to the academic world or to your own professional discipline within it.

The Columbia letter said if you write us this letter, we'll pay you \$300 for the letter of recommendation. I don't know why, but that caused great uneasiness in me, to be offered money to review a person for promotion. I think it was irrational on my part, but the thought occurred to me, is there some way in which they're paying for a positive recommendation here? I was kind of uneasy. Not to the point of refusing the honorarium. That's their business, and it was small enough. It didn't make any difference one way or the other to my life, so that was an incidental aspect. But the principle bothered me, and it's led me to wonder if we ought not to give better definition to exactly what is service, and on what occasions should it be compensated for, and how much. This has gotten exacerbated, of course, with the increasing involvement of faculty with corporations. This issue of for-service compensation sometimes gets very big money. When you get into the world of patents and joint firms being formed by faculty members and business firms, it just diminishes or dwarfs all these other questions of minor payments for professional service.

We haven't thought it through. It's all grown up completely historically and by accident. I served as editor of the *American Sociological Review*, free. I got all my expenses taken care of and I got a secretary and associate editors and a copy-editor and so on, but nothing for me. Then, a few years later, they decided to give an honorarium to the editor. No reason one way or the other. It's a sufficient commitment of time that you might think of making it rewarding monetarily. There's a certain amount of prestige involved in it as well. We don't have any true answers to this. I guess it must be a little bit of the monkish tradition in me. I sort of like the idea that you serve without recompense. It kind of clears your mind of any expectations.

26-00:10:56

Rubens:

Service on academic senate committees does figure, in a small way, into tenure or salary upgrade, doesn't it? That's very minor.

26-00:11:12

Smelser:

Service to the campus. That is one of the criteria that are listed. If you want me to rank the considerations that go into the granting of advancement or tenure, I would say it's down the line. The highest is publication record. The second is national recognition in various forms for publication record in terms of prizes, further grants, et cetera. Then comes teaching, and then comes the two types of service. If you have an extraordinary record in one of those latter areas, then it will be called to note. By and large, I would have been a total fool if I'd interpreted these service assignments I took as contributing to my advancement. I was already University Professor. I had a kind of luxury here. I certainly didn't need the money in these cases. It somehow or other made me feel more comfortable, and I wanted to record this sentiment in these interviews.

26-00:12:22

McIntosh:

Would it be best described as a sense of duty to the discipline?

26-00:12:27

Smelser:

Yes. I maintained that sense of duty about missing classes. It was absolutely a sacred feeling within me, that I should never, ever, ever miss a class. In fact, during my entire career, I missed one class, from an episode of crab enchilada food poisoning that nearly killed me. I missed one lecture. Now, occasionally I would miss a lecture in traveling. I would always fill it up with a colleague. I'd convince a colleague to come and talk on a given matter. I never turned that over to a TA. When I did give my TAs in a research methods course an opportunity to address the class on their work and the methodological aspects of it, I was always there. It was just part of a mix on the education I participated in. That was another of the sacred commitment I had about not slighting my teaching at all.

26-00:15:19

McIntosh:

Back in the late eighties and early nineties, when a lot of these committees that you're serving on are taking place, how much time are you devoting to your own research?

26-00:15:30

Smelser:

I can respond to that by saying that when I was in England, in 1977, '79, I had gathered an absolutely enormous amount of material on my topic, which was the evolution of British primary education for the working classes in the nineteenth century.

26-00:15:46

McIntosh:

Can we just take a step back and revisit how you settled on that topic?

26-00:15:50

Smelser:

Yes. In the mid-seventies, I had this relationship with Erik Erikson going on adult development. It ultimately manifested itself in a publication of a co-edited volume with him and a contribution on my part to it. I was also involved, incidentally, with the group at the Russell Sage Foundation, headed by Matilda White Riley, on age stratification and age in a life course. It became one of my interests, and I was much influenced by Erikson, and much influenced by the very interesting work that was going on by these Russell Sage people, and Glen Elder at North Carolina, on the unfolding of the life course. Of course, that was the theme of the work I did with Erikson. Could the adult years be characterized in terms of distinctive phases, or was it just a period of kind of continuous ad-hoc adaptation to situations? We gathered together these groups of scholars, which differed on this issue, but nonetheless carried on a dialogue.

Since my work on comparative methods, which was a real scholarly piece of work, I kind of felt that I was ready for another really major historical piece of scholarship. It just made sense to me. I felt that I had really proven to myself I could do it with my dissertation. I loved, as a historian—only a historian can love Victorian England and its complexities. I decided I would do something historical, but related to the framework of the life course. After all, the coming

in of formal education for the working classes and the reform of child labor marked a serious reshuffling of the life course for young people. I was going to try to bring the life course perspective to these changes in these formal institutions. That was the way I was carrying on a dialogue with myself about this topic, so I decided, that's what I'm going to do. I'll go back to Victorian England. I know its general history. I steeped myself in working-class history during my dissertation, but it wasn't the same as education history, and I wouldn't be repeating myself. I'd scarcely be repeating myself at all. That's how I drifted toward it, this general orientation to doing a major research project, my love of the historical research, my feeling that I wanted to push myself into another really major scholarly effort.

All my free time, when I was director of the Education Abroad Program, was spent in the British Museum, going over the blue books, the Parliamentary reports, and the secondary literature on working-class education. I discovered, in doing this research, the original framework I wanted to bring to bear on it, of alteration of the life course, just wasn't working. It just didn't fit the preoccupations of the time and the dynamics of the change of the educational system. True, they were accomplishing those alternations in the life course of young men and young women, and I wasn't totally off track. But history was talking back to me and saying, that's not all that was going on. Other things were going on. In particular, it came to me that I simply couldn't address this topic without talking about the religious warfare and about the class system that was going on in England during the nineteenth century. That just stuck out, completely stuck out, in the literature and in the primary material on the educational system, that, in a way, my subject matter turned its back on me and told me I was doing the wrong thing.

I myself underwent this change. I said I have to change, because history is ordering me to change. I just put on these different lenses. Even though I was dealing with changes in fortunes of a young group in the population, I was going to miss the dynamics of what was going on and why it did and did not develop, and why it developed in the way it did. I simply re-oriented my thinking on it and decided during that two-year period when I got into the primary materials that I was going to write basically a different book than I had envisioned in my planning phases. Okay, I had this vast amount of material available. My academic work wasn't quite completed. I did, in 1981, decide I wanted to spend some time in the Bodleian Library in Oxford. I went there for a month. I got put up in Magdalen College, Oxford College, in a room called the Old Fellows Room. I had my meals in the senior common room, which I had, as an undergraduate, just looked up at in the dining hall, and went into the Bodleian Library and did a really intensive month of research. Family joined me over there for Christmas and we had a holiday in London over Christmas in 1981.

What did that library have that you thought that you needed to go see?

26-00:21:49

Smelser:

It just had everything I wanted. I wanted to go back to Oxford. It had full Parliamentary papers. I especially was interested in Wales at that time. I could have gone to London. I could have gone back to the British Library, but I knew the Bodleian had everything. I just went back. The fact that I had been an undergraduate at Oxford, they let me right in. I didn't have to go through the whole application for membership and so on. It was a good month. I froze myself. It was December. But went ahead, so I was moving on this.

I began moving in the formulation of things. I began doing some writing. The thing inched forward for years. I just have to tell you that you don't write these things between committee meetings. This is a kind of project that requires a whole lot more sustained effort and digesting and drafting and outlining and going back and checking and doing new lines of research. All this is very time-consuming, and it's something on which you need to concentrate. You don't do it between phone calls, you don't do it between meetings. However, at the same time, I was beginning to kick myself around because I wasn't making progress on this project. It was kind of a major conflict in my life. In fact, I kind of got, I would have to say, a little bit depressed about this whole thing, because I thought, maybe I won't finish this work after all this investment. Maybe it just won't happen. There was an element, I have to say, of loss that I felt, and frustration.

26-00:23:32

Rubens:

But you weren't turning down any of these efforts.

26-00:23:34

Smelser:

No, and that probably contributed to it. I was living a life that was, in some sense, a little contradictory for me at that time. I kept going on to these other commitments, and not really regretting any of them, as I've indicated. But nonetheless, there was this serious gnawing feeling that I wasn't going to make it. I got a little pressure from one of the funding agencies that had supported me. That was the National Institute of Education. I got a federal grant to work on this. They began pressing me for draft material so they could sign off on the project. Well, I was able to do that. I basically wrote the chapter on Welsh and Scottish education during that period, and that sufficed to show progress on my part, so they signed off the grant. Closed it. Closed its books. But I was still behind. I still wasn't doing it. After I finished with my senate duties, a sabbatical was looming. I had built up sabbatical years, which had ripened into two thirds of my salary for nine years. I said, I'm going to take that sabbatical and I'm going to leave town. An incidental part of this was a family consideration. Our youngest daughter was going away to college in 1989. The nest was emptying. We decided to empty it ourselves and move away for a year.

I knew the Russell Sage Foundation very well because I had had this previous association with them and their educating project. I was a reader for them. I kept a relationship. I also knew the new director of it, Eric Wanner, pretty

well. He had been my editorial guidance in the Erikson book because he was at Harvard University Press at the time, so there was a linkage. I applied for a year's fellowship at the Russell Sage Foundation. It's kind of like the Center. They had the group of scholars, fifteen, working mostly in sociology and economics, and many of them on practical, applied problems, but I applied to do this historical study. They took me and they brought my salary up to its full level to come there for the year. I also applied, just to cover my bases, for a fellowship at the Woodrow Wilson School in Washington, which had a fellowship program that was roughly parallel of bringing scholars in. It was a little more applied, a little bit more governmental in its orientation, but nonetheless I could do free scholarship. I applied to the Woodrow Wilson Foundation. I got that as well. That was awarded. Then I was thinking about being in New York, being in Washington, D.C., so I was talking to various people around, attracted to both places. Gardner Lindzey, my lifelong mentor, said, "You don't even have to think about this five minutes. Go to New York. It's a much more interesting place to be." We were a little apprehensive, Sharin in particular, about the possible dangers of New York. Very high crime era in New York at the time. Possibilities for a lonely life. A lot of noise and movement. So a lot of reservations, none of which came true. We loved it and never got into any trouble or any danger. It was a beautiful year from the standpoint of our just being there and enjoying that city.

I really decided to take this seriously. I could have kept up some of these external involvements, but I resigned from the labs. Other things had discontinued. My work for the National Academy had finished. My work on the academic senate had finished. All these service areas that I've talked to you about were all finished, with the exception of the labs, so I told the labs, I'm not going to make four trips out to California this year. I'm not going to do it. Thank you very much. They said, fine, we'll do that. Then I knew, when I went to New York, that I was going to get asked to talk at a lot of places. That region is full of educational institutions that would love to have you come and give a forum or a colloquium. I was right. I began to get invitations two weeks after I got there. I developed a party line on these invitations. I said, "Please, I've come here to work. I will not say no, but if you'd like to come back and talk to me in April, I'll see where I am, and I might be able to come." That would be near the end of a year. They almost all accepted this as a perfectly reasonable basis for not responding to their invitations. Only a couple of them came back in April. It was basically a decline, so they forgot about me by next April. Though I did, in fact, go to Bard College and I went to Princeton as I finished up my work. I went to NYU and City University for talks, regular professional talks, a couple of them on my current work.

It was a solo operation from the standpoint of my work. None of the other fellows at Russell Sage Foundation were at all interested in doing any historical scholarship at all. You gave a presentation on your work at the foundation at Wednesday afternoon seminars. I gave a couple of presentations and I excited some interest, but it was just mainly myself. That year was

absolutely perfect. We lived one block away from the foundation. I had a beautiful room in this Upper East Side of New York, 65th Street. They gave you secretarial help, all you needed. I had a fulltime secretary in Berkeley anyway. They gave me a new secretary there, a research assistant if I wanted one. The foundation had developed full linkages with libraries around the country. Most of the books I could get from Hunter Library, which was up the street, or from another New York Public Library. They would make requests from libraries around the country, if I had these rare things that I wanted to use. Then I went to the NYU library from time to time, when I wanted to get into, say, papers. The NYU had the full Parliamentary papers that I had to go back to from time to time. It was an absolutely ideal arrangement.

26-00:30:20
Rubens:

You were now using a computer, is that right?

26-00:30:22
Smelser:

Yes. One of the things that happened to me is that I had gone on to the computer before going there, but all I did on it, basically, was type. Present my lectures. Print them out. That was all I knew. I was basically illiterate. There were two techies on their staff. All you needed to do if you got into trouble was yell. Someone would come, tell you how to do something. Furthermore, I had to communicate with my secretary by computer. None of this dictation stuff.

26-00:30:55
Rubens:

You carried, I imagine, suitcases of notes?

26-00:30:59
Smelser:

Yes. All my research notes were along with me. I knew where the gaps were by then, what I needed to fill in with respect to each of the topics that I was going to be writing on, and was able to track it down adequately. I was able to complete the drafting by April of this magnificent year. I said to Sharin, "This is going to be a nine to five work day. None of this working upstairs coming down. It will be nine to five. I won't work in the evenings. We'll take the best advantage we can of all that New York has to offer. But I'm going to be nine to five in the job." That's the way I organized it, is went there and worked all day, five days a week. When I happened to be going on the plane somewhere, I'd take work and so on. It was a change in lifestyle.

26-00:31:59
Rubens:

You weren't getting up at six.

26-00:32:00
Smelser:

No, no, no. I had long since given up that kind of insane dedication. We'd go and take a little walk in Central Park in the morning. I'd go in by 8:15 and go to work. It's absolutely amazing the amount of progress I was able to make under those circumstances of having the infrastructure there to do the research. My motivation was very strong. I never got hung up. I had no blocks about moving ahead on the research. The writing came freely, and the

computer was a huge help. I could do it. It was much faster. I don't know how I ever did it before the computer. So the thing was finished in April. I did some additional work, going back and checking a lot of the stuff in the New York libraries in the month of May, and then I decided to think about publishing.

The Russell Sage Foundation has a policy, which has sort of crumbled, that you publish with them. They, of course, wanted to have full rights to publication. They had relaxed this at the protest of some fellows who wanted to publish more commercially over the years. It was an arrangement that was beginning to crumble around the edges. I said, "I would like to publish this jointly with you and some other press. A university press." It had to be. It was a scholarly piece of work. I knew they had precedence for that. They said okay. I sent it to Oxford Press and UC Press. They both wanted it, and none of them wanted any changes. I decided to go with my favorite press. UC Press put it in a special series of books of exceptional scholarly interest. It was published pretty quickly. It was published in 1991. I really, really felt wonderful when I finished that book. Just euphoria. It compensated for all of those doubts. I really felt it was intellectually in the same category as my doctoral dissertation, from the standpoint of the quality of scholarship and the quality of ideas that I got into it.

It did not, definitely, have as much impact as my dissertation. I wonder why. It was very different, and my theoretical framework changed quite a lot in interpreting the material. It brought into play group conflict. It had to. That's what the history of that education was all about, was deadly group conflict for three quarters of a century. It was not quite so mechanical in terms of the application of a fixed model, even though I did talk about the structural changes in the language of that model in certain points in the book. The topic itself was of interest in my accounting to myself. There is an industry in England about writing about the history of education, and this is one of the most overwritten subjects in British history, is their educational woes they had in the nineteenth-century working class. They've got a whole variety of lines of interpretation. There's a Marxist theory of what was going on. It just spreads out over the whole range of intellectual preoccupations. I of course took my place within that. It was an eclectic book that didn't categorically reject other explanations, but tried to incorporate them in some way in my interpretations. In the United States, that topic is of almost zero interest to sociologists. American scholars of education mostly study America. I think I landed in the part of the market that didn't read it very much. From those who did read it, I got very favorable responses, but it didn't generate—maybe it's the stage of my own career. Who knows what it was. I was satisfied with the book intellectually, and so I didn't sit around moping that people weren't raving about it more or it didn't get more widely reviewed or used than it apparently did. But nonetheless, it was a great landmark in my scholarly history.

26-00:36:59

McIntosh:

It's a deeply interdisciplinary work, which is sort of typical of your scholarship, I feel like. You've talked about how it fits in with the historiography of British education. What other literatures did you see this book fitting into and responding to?

26-00:37:22

Smelser:

In the early part of the book, I named what I saw as the main schools of thought about it, which were all in the history of British writers. No Americans, basically, had written about this subject before. In answer to your question, I would have to say that I was really talking to the historians, as I had been talking with them in my thesis. I had one chapter that came as close as any did to common ground with my dissertation, but even that didn't, and that was the family economy of working classes, and how it, in some sense, undermined efforts to bring education to them, because the working classes themselves wanted and had to have their children at work. They did not particularly welcome education, and education was not regarded at that period as an avenue of upward mobility. It was the idea to make the working classes respectable, not to advance them in the class hierarchy. I had to address the motivation of the working class themselves, who were constrained by low salaries and income, and relied on the work of their children and did not really see—correctly, did not perceive—the social capital that would be gained by getting an education. It was highly stratified. Even the teachers, all the teachers for the working-class schools were recruited from the working classes. It was so absolutely segregated. It kind of blows the mind.

This whole idea I brought at the time about self improvement and Samuel Smiles and the importance of education and getting ahead—he was interested mostly in the respectability of the working classes. That's what the non-working classes wanted, was a respectable working class of people who stayed off the doles, who worked, but not necessarily those who advanced upwardly. It was one subcategory of people who advanced, and that was those kids—when they needed railroad clerks in the railroad explosion in Britain in my period, they hired a lot of clerks. This is a cut above working class. This is more white collar. A significant number of them went into teaching in their own kind of schools, which was, in fact, a kind of social mobility. They got out of the working classes, in white collar jobs. Those were the two exceptions to any kind of systematic mobility that one either expected or emphasized in society. This contrasts a lot with at least the ideology in the United States that education is a way of getting ahead, getting rich, moving up the status hierarchy, and so on. That ideology in Britain only came in much later. It was extremely layered and stratified and so on.

I was talking to the economic historians. It was a dialogue with economic historians as to what was really mattering in the world at the time. I gave a lot more role to the family than they would have. I was talking to political scientists. It was one of the running sores of British politics during that whole

century. Parliament didn't know what to do. Forever lost. Forever fighting. Religious groups were forever leaning on Parliament to do this or that, contradictory things. Parliament was more or less paralyzed, and inched forward. That's why I named this book *Social Paralysis and Social Change*. Here you get the development of a modern institution, only by the greatest and continuous birth pangs that lasted fifty years, because they were all engaged in defending their own turf and what education should be about, and fighting over the youth of the country. They weren't caring about the development of the youth. They cared about their own secular and religious commitments. They were fighting out those battles over how the kids should be educated. So there was a political science dimension to it. Naturally, I had an audience of interested historians. I went around several times during my tenure as director of the Center and lectured on it at different institutions. I went to Oxford and Cambridge and Redding, and then talked on it at some of the campuses where my students were studying. I got a lot of feedback from English colleagues on this topic as well.

26-00:42:09

McIntosh:

It also struck me that so much of a North Atlantic scholarship on the twentieth century and in the twentieth century was about the creation of a public sphere that religion is sort of segregated from. You see that in philosophy. You see it in social thought. You see it in history as well. And the concept of the primordial that you use, and the sacred, really seems like such an important part of the story, in showing that that framework is not as neat when you actually look at the historical record.

26-00:42:52

Smelser:

When you get dimensions of social identity that are primordial—that is to say, indistinguishable from oneself and one's heritages and one's hopes for the future—that's when people really fight, because things are much more at stake than a few shillings an hour. The class conflict and economic conflict were very deep in that period, but here was a case where the holy principles were at work. I didn't have this idea of the primordial quality of class, which was an original formulation, because none of the anthropologists who write about primordality ever talk about class. They talk about blood, kinship, region or community, ancestry, religion, and citizenship as the primordial points of identity. I said, well, look, Britain seemed to have a period in its history in which the class played an equally important role in terms of the way they wanted to organize their life, who they kept company with, who they built their institutions for, and who they didn't build them for, all breaking down along these primordial lines.

26-00:44:11

McIntosh:

Class consciousness almost takes on a sacred quality.

26-00:44:16

Smelser:

Class consciousness became a crescendo during the period, with high points in the periods of deep labor conflict, like in the 1830s period and so on. By the

time I got to my period of 1870, which was the big breakthrough in the establishment of nonsectarian public education for which the government paid, the government schools came in then without being tied to religious organizations. Class consciousness played a very big role in that. The workers and the unions were at a significant point in British politics that they were not in at the beginning of the century. It became class competition. The workers began to make noise about social mobility and make noise about better incorporation of the working classes into the system and so on. So there was a gesture on the part of the Gladstonian government when it brought in the public education in response to the increasing power of the trade unions and the nascent forces that were to form the Labor Party. It was a secular increase in class consciousness during that period.

26-00:45:29
McIntosh:

A secular increase, but it also seemed like religion was something that just never disappeared, no matter how much of a secular framework was kind of built upon it. It didn't seem to go anywhere.

26-00:45:43
Smelser:

Well, the point is, it was competition, not religion versus secular. It was competition among the religious sects for a piece of the pie. Once the government got into subsidizing schools, the pressure was for them to be evenhanded and not favor the Church of England. They would incorporate these other sects, ultimately Catholics and Jews, into the educational system, so long as they were religious. The big bugbear was secular. It would never support a secular school. It always had to be some kind of a religion, so it became a competitive relationship among the sects, with the Church of England beating a sluggish retreat during the entire century, and these other groups wanting to get their fingers into the system so that their own children could be educated on the same basis as the children of other religions. It never went away. England has a requirement for religious education still on the books. It's national. When our kids went to St. Michaels school, which was a previous Church of England school that was now, for all intents and purposes, secular, except that the local vicar came and locked it up every weekend, in a ritual way, they had RE, religious education, which was pretty watered-down. I have to say it was kind of like moral ethics and so on.

There was a funny incident with my young daughter that happened in France when we were there. We were sitting in a park and watching these French guys playing boules, playing petanque. The kids were running around the little park there that we were sitting in and playing around. My daughter, Sarah, who was then maybe six, went up to a brick wall surrounding the garden, threw herself against the wall, face-forward against the wall. She says, "I'm Jesus backwards." That was from her RE, seeing the crucifixion backwards. Which I thought was something. But anyway, that thread, as you can see, how history dictated my choosing a religious dimension, even though I was not initially inclined to do so.

26-00:48:27

Rubens:

As I understand it, as you're abroad in the late seventies, doing the research, you realize that the life course was not going to be the organizing principle. Then you spend this incredible decade of writing a text, of doing three major studies on the state of the behavioral sciences, and then also all that work through the German Theory Association of looking at theories of modernity, modernization. Is it possible to point to your evolution of thought as a result of all that work, of some highlights of what lines of inquiry, certain thought, theoretical frames, or particular people that may have influenced how you ultimately finished the book?

26-00:49:18

Smelser:

I did mention, through Erikson, the continuing influence of psychoanalysis. I was doing bits and pieces of writing in the area of psychoanalysis for over thirty years. It later came out in a book in the late nineties. That, of course, was a direction that I was sent that turned out to be the wrong direction for this particular study that I was choosing. I suppose that in keeping with other things that were happening in the field, I think my thinking was moving away from systematic theory, more toward, you might say, a more eclectic approach to—certainly I've always been interdisciplinary, but at the same time, my explicit concern with theory was a huge, major part of my world view in the fifteen years during my doctoral training and afterwards. Of course, the field of sociology more or less turned its back on theory during the sixties and seventies, with all the attack on general theorizing and its spilling over into political opposition to functionalism and so on. We more or less turned the serious, big-time theoretical thinking over to the Europeans, who still dominate that general theory. This country, in sociology, is kind of in the doldrums, I would think. What comes closest to it is some interdisciplinary work or thinking up the larger implications of a study, but explicit concern with theory has gone downhill. I have not given that up, but I think my own thinking has kind of slightly paralleled that larger intellectual movement within my own field.

26-00:51:19

Rubens:

Despite coming to terms with a kind of reckoning with German theory.

26-00:51:23

Smelser:

Yes, I kept the interest in theory alive. It wasn't an all-or-nothing thing. I'm just thinking of the larger trend in my own thinking.

26-00:51:31

Rubens:

And that's showing up in your book.

26-00:51:33

Smelser:

Yes, not as much. It never disappears. I consider the recent work I did, both the terrorism work and the odyssey work—they are theoretical works, among other things. It isn't gone. It's just not as in tune with the dominant style of analysis that my sociological colleagues have.

26-00:51:57

McIntosh:

This historical aspect of this specific project, too, sort of disqualifies it from being a primarily theoretical work. In your conclusion, I remember you using your historical work to revise functionalist and Marxist theories of education. To me, that's a particularly unique contribution that this work offers.

26-00:52:30

Smelser:

I would have to say that I was always interested in making my own view of how the world changed during that period as general as possible. At the same time, there is this historical contingency involved. The other thing about that book that was appreciated by some who commented on it, and I certainly made it a conspicuous piece, was the fact I made it into a comparative work, even though it was on Britain. In other words, there was a chapter in there on religious conflict in New York. I had made special cases out of the Scottish and the Welsh and Irish experiences, all of which traced primarily to religion. Not entirely. In Scotland, class also played a big role. Religion and foreign domination were the big story in Ireland. While they were all going through the establishment of primary education at the same time, they took very different paths. I tried to say, why did they come out so differently, even in this one little island? I took a note from my own work on comparative studies to do what you call within case variation as a mode of comparative analysis.

26-00:53:48

McIntosh:

There seems to be a comparative aspect, if only alluded to, in terms of conclusions about nation-building, and the differences between the role of education in the United States and the role of education in Great Britain as well.

26-00:54:07

Smelser:

They had very different preoccupations. The British commentators on American education were perplexed. They envied it because it seemed to be so easy to establish. They'd look across the Atlantic. How did they get away with this? We're having all these fights and struggles and paralysis and so on. Also, they couldn't figure out how you mix social classes in the classroom. Said, we don't understand. They didn't like it. They thought it was wrong. Also, they couldn't understand it. That the community-based American school was community-based, rather than class-based, whereas in England it was geographically located, but it was very strictly class-stratified. Then they couldn't figure out why we let boys and girls in the same classroom. I didn't talk about the gender aspects too much in that book. I did talk about some, and the role of female teachers and various other aspects, but it was not the most conspicuous. It was kind of taken for granted by them. It was not a note of conflict. It was just what they did. They just segregated the sexes and they segregated the teachers by that principle. It was very sexist in that they taught cooking and sewing and things of that sort as useful for working-class wives and mothers. I like that comparative aspect that I built into it.

26-00:55:42

Rubens:

And your point, Jess, about nation-building is?

26-00:55:46

McIntosh:

Just that he reassess the role of education. There are different theories about what education does in terms of nation-building.

26-00:55:57

Smelser:

The English were extremely conscious of this aspect, and they were very conscious of other nations passing them up. They were forever looking to see what Holland had done, what Prussia had done, what the French were doing, and what the Americans were doing. England came out last in terms of its own development of a proper educational system. They were forever pointing to their continuing poverty, to their continuing crime, to their continuing conflict in the society. In a way, they rued that education wasn't building a very good nation.

26-00:56:31

McIntosh:

Now, I have to just ask one more question. Taking this work into consideration, but also your other historical projects as well, how do these projects make you reassess the ideas, the very sort of prevalent ideas, of—we talked about Habermas and Rawls last time—the value of the concept of a public sphere that is sort of insulated from these primordial aspects. Is that something that projects like this have caused you to reassess?

26-00:57:20

Smelser:

Not really. The dominant commentary on contemporary society, over time, and not limited to the present time, but it goes back really to mid-nineteenth century, is that they're falling apart and they're losing community. This is absolute one-note kind of *gemeinschaft-gesellschaft* sort of discussion. I've always been somewhat an enemy of this dichotomy, and I've always looked around for functional—I wouldn't say equivalents, but alternatives to institutions that are in fact evolving, sometimes in radical ways and sometimes, in a way, losing their original force. Actually, that turns out to be a big theme in my most recent book on usable social science, is that I have a general chapter there called "The Strange Fate of *Gemeinschaft*." So I analyzed the ups and downs of interest and trust and networks and bases of social integration over time as a kind of history of knowledge with respect to this.

I never joined the doomsayers. I get constantly impatient with the communitarians and that Tocquevillian tradition that says it's all falling apart. That the social order is falling apart and there are all kinds of negative consequences, and we've got to do something about restoring community. I refuse to get hysterical on this subject, the way a great deal of the literature is. I'm always looking for inventive ways of reinventing social linkages that go on with increasing specialization and differentiation of society. I take kind of a middle of the road there. I don't celebrate progress in the way that a lot of

economists and others do, but at the same time I don't have this Cassandra view of civilization falling apart and there's no basis for its falling together, and people in it losing their sense of self or bearings or whatever. I don't buy that. I'm constantly looking towards this middle range of conversation between these two. I don't deny the contrary tendencies to a higher specialization and primordial and primary links in the society, but I'm very much interested in social inventiveness that goes on in, you might say, assuring the continuity of both these aspects of social organization.

26-00:59:56

McIntosh:

That seems to be something that you do focus on in the *Social Paralysis* book, which is maintaining that complexity. If you do maintain the fact that gemeinschaft-gesellschaft can sort of work together and interact, it opens up sort of new evidence as to how people are maneuvering.

26-01:00:19

Smelser:

Yes, absolutely. I think I have a view of mankind as somewhat more inventive than many of my colleagues.

26-01:00:28

Rubens:

Do we have time for one last? I'm just wondering, in the course of this study, did you kind of reassess your theory of collective behavior?

26-01:00:43

Smelser:

Not formally. No. I did analyze a lot of social movements in this book, but I didn't explicitly address the implications for social movement theory.

Interview #14 July 08, 2011

[Begin Audio File 27]

27-00:00:00

Rubens: Hi, Neil.

27-00:00:07

Smelser: Hi, morning.

27-00:00:08

Rubens: This is our fourteenth interview. We're moving along very well. I think these interviews are very substantive, instructive, and filled with good stories.

27-00:00:15

Smelser: I'm feeling at home.

27-00:00:16

Rubens: Good, I have kind of an off the wall question. In an interview with another narrator, I don't know why, but I asked, "Did you ever have any kind of fetishes or superstitions or—" And he opened up a whole dimension of his life.

27-00:01:07

Smelser: Well, I'm not loaded with them but I do have a lucky number, which is four. And I do have a favorite color, which is blue.

27-00:01:15

Rubens: Goes way back. I don't know the significance of it.

27-00:01:21

Rubens: Using a certain pen or—

27-00:01:24

Smelser: Oh, little ones. Doodling on coffee cups. Nothing much.

27-00:01:27

Rubens: All right. So we're here today to begin talking about the *Handbook of Sociology*. And would you tell me how that came about?

27-00:01:38

Smelser: Yes. This was initiated by Sage Publications. I had had a growing relationship with Sage and there was a special—a man there by the name of Mitch Allen who was interested in what I was doing and they conceived of the idea of a handbook of sociology. There was one in, I believe, 1962. Came out when I was first here at Berkeley. Edited by Robert E. L. Farris. But there hadn't been one since. And these are kind of a standard format in which the editor or editors really basically take responsibility for organizing the field by subtypes and recruiting the best representative authors they can to write essays of an assigned length on this.

They approached me just because I was known as a general sociologist but they also asked me, the Sage Publication asked me, to choose a co-editor whose area of work was kind of more on a micro side of organizations and we would have a division of labor and therefore I'd have a more comprehensive symbolism in the co-editors' bios and we'd also take joint responsibility. I named the co-editor. His name was Ronald Burt. He was one of the four people we hired in sociology here in 1976. It was written up in the book with Robin Content. We didn't identify him but he was one of the persons. And he came here. He was a student of formal organizations and he was very mathematically inclined and also interested in—he was methodologically inclined and on a quantitative side of—so he was a good complement to me and we got on together.

27-00:03:46
Rubens:

You had known him? You liked him?

27-00:03:48
Smelser:

Yes. And he described me as his professional role model to other people, so we had a congenial relationship. In '86 we began to do the preliminary organizing work of trying to identify people. We were extremely successful in recruiting the authors we wanted for the different topics. Topics on mass media, topics on social movements, topics on stratification. It was an organization of the field which had to be more or less conventional or else it wouldn't reflect the field.

Well, a very interesting thing happened. Burt worked with me closely and well in the preliminary stages.

27-00:04:31
Rubens:

Where were you? Out here writing it?

27-00:04:33
Smelser:

Here, here. And had he gone to Columbia, I think he had by then. He had gone to Columbia but he was out here and we collaborated actually in this house to get the thing organized and setup and then we did all the requesting and so on. At a given moment, when it came time for me and Ron to work out an introduction, which was conventionally what co-authors do, co-editors do for a handbook, he suddenly stopped working. He couldn't write a piece of his own. He didn't write a piece of his own. He wasn't following up on his authors to get them to write. He more or less stopped. He just left. Left the project. He didn't give me very good reasons. It was, "I'm either too busy," or blocks about this and so on. It was a kind of curious episode and it created a crisis, of course, because I didn't want to do all the work and—

27-00:05:50
Rubens:

Well, you were so involved in so many other things.

27-00:05:53

Smelser:

Yes. But my problem was that I didn't want to do all the work and still have him as a co-author, a co-editor. So I immediately went to the Sage people and I said, "We've got a crisis here that this book is not going to go forward unless I do it by myself." So we more or less simply decided that he shouldn't be part of it. And I didn't have to confront him. The publisher did. So suddenly I became the sole editor of this.

27-00:06:27

Rubens:

And just parenthetically. So by then he was at Columbia. You didn't have to see him regularly?

27-00:06:32

Smelser:

No, no. And we saw each other since. It was a bad episode. It didn't affect our relationship. I mean, we—

27-00:06:39

Rubens:

He just never accounted for it?

27-00:06:41.3

Smelser:

It was just basically a kind of disappearance, a block, as though he—it's kind of like any author who often signs a contract and doesn't follow through. So I took it on my own and from that point on I did the whole thing and I was listed as the single editor. We had some benign language in the preface. I thanked him for his contribution and used a euphemistic phrasing as to why he withdrew on account of other commitments or something like that. But nonetheless, I was it.

27-00:07:17

Rubens:

You had to follow up with the people that he had?

27-00:07:20

Smelser:

Well, typically an editor of a volume of this sort receives the drafted manuscript and has the responsibility of giving him really detailed feedback. That's what an editor has to do if it's going to be his or her product. Look for continuities and overlaps and repeats and substantive problems and raise intellectual issues about every chapter and so on. That's really what you do and that's what I did. And so it was a heavy burden on me to carry through this. I knew how to do it. I had a lot of experiences editing and I didn't feel any—that I was out of my depth or that there was any—and I wrote a separate chapter in the book on social structure, theories of social structure that was included early in the book in the theoretical section in addition to my own preface. It was more than a preface. It was an introduction to the whole volume and commentary on the field of sociology in that introduction. So I had really basically two chapters in this book in addition to being editor. The book was given big attention by *Contemporary Sociology*, which is the book review journal of the American Sociological Association. There was a lead or series—

27-00:08:46

Rubens:

I didn't know there was a separate journal for reviews.

27-00:08:50

Smelser:

For book review and editorial. The regular *American Sociological Review* used to be the only one. At the time I edited it, I had a book review editor who was my colleague—David Matza, my assistant. But they split the reviews from the substance of the journal, oh, maybe ten years after I was editor. And so *Contemporary Sociology* is a journal of book reviews. And so they set aside—they had maybe six reviewers review the handbook. Each of them wrote review essays on the book. And then I wrote a rejoinder—a response to reviews in the thing. And in a way I got really enlightened by the reviews and I actually made a point of mentioning this in the response to reviews. I would say 70 percent of the material in the reviews was complaints that a certain topic wasn't heavily enough emphasized. And in almost every case it was the topic that the reviewer himself would have known and was a pet or a research area of the reviewers themselves. And it struck me so profoundly that in my response to the reviews I sort of, in a very polite and civil way, talked about parochialism in the field of sociology and how—

27-00:10:45

Rubens:

That's certainly Alexander's point in his introduction to his book with Marx et al., where he says that the field has become so fragmented and so specialized.

27-00:10:53

Smelser:

Well, this was a good reason for choosing me as editor of the handbook, because I had done research in a lot of different areas and I was always interested in the borders. Borders of the field. Bringing in a synthetic style and I also—I had commented on the fragmentation of the field in the introduction as one of the threads of intellectual history and, of course, that wasn't that particularly profound. It was right in front of your eyes and still going on, of course.

27-00:11:29

Rubens:

Was the complaint also about the people you chose to review? That there were other people you could have chosen?

27-00:11:35

Smelser:

No. There was no questioning of my intellectual taste. There was a questioning about my—some questioning about my organizing of the chapters and I didn't give enough play to certain aspects of stratification. So these are all worthy observations but the cumulative effect was that people can be pleased about general and comprehensive work. It was quite widely used.

27-00:12:05

Rubens:

Was it?

27-00:12:05

Smelser:

Yes. And reviews were not—from the standpoint of substance were not negative. Just except that you didn't represent this emphasis enough or that emphasis enough or so on. So I think you would get that in anything you did.

27-00:12:22

Rubens:

When you say widely used, in college introductory courses or—

27-00:12:27

Smelser:

It was used in graduate courses. Graduate courses, as well as upper division courses. Many colleges and universities have upper division general sociology courses. It wouldn't really have been appropriate for freshmen because it's just too technical and it was—freshmen and sophomore courses either take one of two forms. One is they use a text and there are dozens and dozens of texts.

27-00:13:03

Rubens:

Yes. Including yours.

27-00:13:04

Smelser:

Including mine. And they would use that book, in the upper division general introductory courses as well as graduate courses. Another windfall of not having Burt involved is I got all the royalties. They doubled my royalties. But he was basically chagrined at having to leave and he raised absolutely no questions about partial royalties for having started the thing. Some smaller minded person might do that.

27-00:13:51

Rubens:

Did you later on update it?

27-00:13:59

Smelser:

No. There was never a second edition. However, I would like to, even though it's not on our list, talk about a second book of the same sort, in 1993 with UNESCO. This is a book called *Sociology*.

27-00:14:20

Rubens:

Oh yes, we must.

27-00:14:23

Smelser:

It was put out by UNESCO in a series of disciplinary books. And I was chosen by the UNESCO people. There was a committee to write the following kind of text. It would be in contrast to the *Handbook of Sociology*. That was predominantly American. And I got a few criticisms for that, that it was not international enough. This was a book that they wanted to have written in the following way. It was a curious kind of text. I was going to be the author but I was going to commission nineteen or twenty chapters to it from an international body of scholars, only a couple of whom would be from the United States. And they would send me their drafts but I would, in agreement with them, be the author. I would rewrite everything to give continuity to the whole thing. It's an odd kind of arrangement and I don't know why I entered

it because it's very likely to set up inflamed relationship between the editor or the authors, which was I, and the contributors who want—It's their stuff. And I, in fact—

27-00:15:42

Rubens: They're paid for this?

27-00:15:44

Smelser: I'm the one who got paid. No. I don't remember all the contractual things but I was the only listed author and they were listed in this book as associates but never titled. Well, I rewrote what they sent, but it's very odd. It goes against certain ego requirements of the contributors, I thought, because I got the credit. But they all agreed, which was strange to me.

The book was—I don't know what its fate was. I was constrained by the politics of UNESCO to get somebody from every continent in the world, so I had to go far afield and I had great trouble locating an African scholar, for example, just because the field is so less developed and contributed very little to international sociology. But I did. I located the right kinds of scholars in the right kind of places.

27-00:17:21

Rubens: You're using your extensive networks.

27-00:17:22

Smelser: Networking and—yes. Well, I was very active in the International Sociological Association and I actually interviewed a lot of my colleagues in international sociology in search for authors. So I used them as well for this.

27-00:17:37

Rubens: And then we'll pick this up later, but you had been to Asia three times so you had some—

27-00:17:41

Smelser: Yes, that's right. I had some contacts and I think I exploited them. But mostly I followed my own knowledge of the international literature and I followed the advice I got from representatives to the executive committee on the ISA, which I was a member at the time. It was my ISA contacts, and I went to Paris and worked with the people at UNESCO.

27-00:18:05

Rubens: So when are you working on this?

27-00:18:08

Smelser: 1992. Just when I came back from New York. It was in that period that I did it. And it came out in '93, as I remember.

27-00:18:35

Rubens: So how long did it take you to write?

27-00:18:38

Smelser: Quite a while because I—

27-00:18:42

Rubens: You have to send out, bring in.

27-00:18:44

Smelser: I had to go back and forth. I had to get their approval. They came back. It was a very big diplomatic exercise on my part because I wanted to keep peace with the authors while at the same time doing radical rewriting of a good deal of this stuff. The chapter that I remember, I mentioned the difficulty with finding African scholars and Asian to a lesser degree. But this African scholar I chose from one of the universities, it might have been in Kenya, to write an article on welfare systems. Well, he wrote a little essay on the welfare system in his own country, which wasn't really appropriate for this. So I had to do a lot of original work to flesh that out into a proper article on welfare systems. And others I recruited—one of the guys I recruited was Michael Schudson from the California—from the San Diego campus to write on cultural—some aspects of cultural sociology.

27-00:20:02

Rubens: Media was his specialty?

27-00:20:04

Smelser: He was into the media. He and I later co-chaired the commission on general education in the twentieth century that came out in 2006. We'll talk about that when we get back to my post-retirement years. But Schudson wrote an article that I couldn't improve on at all, so I more or less used it but claimed authorship. But, oh, Michael is a nice man so he didn't have a—he wrote this one on nationalism, cultural aspects of nationalism. He knew more about it than I did and he's an eloquent writer. So I did very, very little additional work on that. But nonetheless, I don't think I should have done it. It had such a crazy charter.

27-00:20:50

Rubens: How is it that UNESCO came to you?

27-00:20:57

Smelser: The name will come to me. He's a Middle Eastern scholar administrator who was hired. He was on the UNESCO staff and he was simply heading up the enterprise to get this list of seven or eight books in the social sciences published. And I suppose they did their regular homework and decided that I would be a good author. I had a very good working relationship with him and he comments on it—comments on me very warmly in his own series. He's the series editor so he wrote a brief and he was extremely flattering in how nice it was to work with me and so on. But it had all UNESCO politics involved in the selection. We couldn't ignore certain parts of the world. We couldn't do this. In some sense it was a political document in that respect, as well. But I went along with it and did it.

- 27-00:22:03
Rubens: This isn't a high royalty piece, is it?
- 27-00:22:06
Smelser: No. I do not remember. I may have gotten nothing. It does not stand out in my mind as being a source of significant income. Or maybe none.
- 27-00:22:21
Rubens: Right. It was a public service really.
- 27-00:22:23
Smelser: Yes, it was. And it was in keeping with my own ability. I had written my own text.
- 27-00:22:30
Rubens: Yes, and the handbook.
- 27-00:22:32
Smelser: I'd edited the handbook. So I was in the neighborhood of being able to do something like this in the international—I gave a couple of talks at UNESCO. As I visited Paris, they asked me also to give talks on general topics in sociology at UNESCO, as well. Well, it was an interesting little twist and I—
- 27-00:22:57
Rubens: Yes, well and a big undertaking it sounds like, too.
- 27-00:23:00
Smelser: Well, yes, I'd have to say so.
- 27-00:23:04
Rubens: Were there essays that you want to point to that were particularly outstanding that—I know you rewrote the whole thing but were there people that you really did think were first rate?
- 27-00:23:13
Smelser: Well, I mentioned Schudson's contribution. There was a woman by the name of Valerie Moghadam, who was from Finland but she's of Middle Eastern—either Arabic origins but she's taught at the United Nations University in Helsinki. I got her to write the one on feminism. And there was another delegate issue because she was very ardent and I wished to give it a little bit more analytic flavor. So I had a back and forth with her but that was also agreeable. I guess it constituted a big test of my diplomatic abilities to deal differently with all the contributors.
- 27-00:23:54
Rubens: Yes. You had been identified as a diplomat very early in your life.
- 27-00:23:58
Smelser: That's right. And so this was an exercise in diplomacy because I had no idea what kind of egos I was going to be stepping on or what kind of expectations they had, even though UNESCO was clear with them that they were not going

to be listed as authors. So they stepped into it knowing that that was not going to be the case. But the rewriting I did was different in every case. So I had nineteen separate dialogues going on with the different authors.

27-00:24:27

Rubens:

Forgive me if this is sort of crass the way I'm saying it, but what do you get out of it, in a sense? You have your hands into the international field, into the sociology and finding out what's going on in the field. Your last textbook comes out in '95 so I would imagine there's always a little bit on your mind about what you can utilize and—

27-00:24:55

Smelser:

Yes. Of course, I didn't copy things out of my text but it was, of course, background work that I had done and I was equipped—

27-00:25:02

Rubens:

And similarly I would think some of the information that you learn and the way things are discussed could be used in the text.

27-00:25:12

Smelser:

Over time I came to fancy myself as an international sociologist.

27-00:25:15

Rubens:

Okay, sure. You still had that affiliation at international studies at Berkeley.

27-00:25:20

Smelser:

I was in the Institute of International Studies. I went to every sociological congress of the ISA from 1960 onward. I missed the Delhi conference and recently I have not gone to them all. But I had a very big network. As we described earlier, I was very active in forming their research group in economy and society and got to know a lot of people in these different countries and it became part of my identity. So I was comfortable doing an international text even though it had these kind of weird dimensions to it.

27-00:26:04

Rubens:

So shall we move on then to your return from New York.

27-00:26:15

Smelser:

At Russell Sage I had finished this book on British education, and was feeling really wonderful about it. I had just been to Madrid where I had been nominated to be president of the ISA but lost in that election that I described. So here I was coming back. I was exactly sixty years old coming back. I had a finite time until my retirement. The Stanford Center was not on the horizon yet. In fact, in 1989, I was interviewed to be director of the center.

27-00:26:57

Rubens:

Okay. You had been on several committees and on the board for the Center but we haven't talked about any of that.

27-00:26:59

Smelser:

I'd been on the board of trustees for quite a long time and I'd been on a committee on special projects. I was a very close colleague with Gardner Lindzey, who was the director. And I was interviewed in New York by the search committee and I learned that I came in number two to Philip Converse, a political scientist from University of Michigan. He was director for five years immediately preceding when I was chosen. I have to tell you that at that time—you see, I was coming back from this time in New York. I was at a point in my career I wasn't quite sure where I was going in this last decade of my active career. I assumed I was going to stay on in sociology until the end here.

27-00:27:49

Rubens:

And that's demarcated? At what age are you supposed to retire?

27-00:27:56

Smelser:

No, no. The mandatory retirement had ended. It actually ended for academics in 1993. So the uncapping took place before my retirement time. I was anticipating, thinking seven years maybe until retirement. Then they uncapped and then my vision got to be retiring at seventy. But on the other hand, here was a finite period of time. Where was my career going to go at this point? And I had just been through a close election and not elected president of International Sociological Association and in a way I was sort of torn. Do I want to spend these last years of my—I remember thinking about this—last years of my service to the university doing exactly the same thing I did before? There was a bit of an idea of playing out of my career at that time. It's what I guess would happen at age sixty.

27-00:29:11

Rubens:

With a little depression or simply questioning?

27-00:29:14

Smelser:

No. Well, a little disorientation, I would say. I wasn't depressed. I was buoyed up by my book and I felt really liberated because that had hung over me for so long. I've described that. So here was an idea of what do I do. Well, the three years before I went to the president's office were in the department mainly. My first year they said, "Come teach your theory course again." This was the graduate theory course. I'd become the journeyman. I taught it not exclusively because some other faculty members did and Ann Swidler took it over from time to time. She taught it when I wasn't in town. For example, when I was on the Education Abroad Program I was away for two years. When I was at Russell Sage Foundation I was away for a year. They had to get someone to teach the course. They used other colleagues. But anytime I was around, I was the favorite. I was the journeyman.

27-00:30:15

Rubens:

And with the graduate course, did you co-teach that ever or was that the undergraduate course?

27-00:30:20

Smelser:

Well, we talked about that I co-taught it with Kingsley Davis when I was very young. I taught it with Philip Selznick a few years later.

27-00:30:29

Rubens:

We're talking about the graduate course.

27-00:30:31

Smelser:

The graduate course. Then with Burawoy. Taught it with Arthur Stinchcombe for two years. They joined the theory and methods together experimentally and I taught it with Art for two years. And then I taught it with Burawoy. Unsuccessfully.

27-00:30:50

Rubens:

So they want you to do it. You come back and—

27-00:30:51

Smelser:

So I come back. They want me to—and I taught it alone this time and I updated the—because I hadn't really taught it more than once in the last five years. I updated the readings of the theorists. I covered some classical theorists and I covered right up to the present time. I introduced Bourdieu and Habermas into the course. I knew their work and it was, I think, very important—and I included some feminist literature. And I just wanted to bring it up to date on more contemporary developments in the field. I came back to teach it. The class had an interesting composition of a few vigorous graduate students who were—a required course always excited ambivalence on the part of students because they have to take it. And this course was regarded as kind of a hatchet course and a lot of mythology about it. If they didn't do well in it they were going to be thrown out of graduate school. That kind of graduate student lore developed about it. And this class, which was about twenty-five people, I guess, had a small group of, say, five with one, I'd say, leader. Very ardent feminists. And somewhat chip-on-the-shoulder type person. I'll actually relate what happened with her in a minute.

But what I decided I would do in this course was to—and the meetings of the course was twice a week for two hours. It was pretty heavy. So I would present my own representation of the theorist of the week or theorists of the two weeks and then open it up for discussion and then I would ask two students to supplement my presentations with presentations of their own to add, to incorporate them. And every student had to participate during the course of the semester, so I made it a little more collective. Well, this group, this active group, said, "We don't want you to present first." The idea was that you're going to poison our minds and you're going to effect our presentations. So there was this little kind of mini-rebellion that went on in class. So I said, "Fine, let's do it that way." Wasn't a very big rebellion in my mind. We did all the same things. And then I felt comfortable enough because there wasn't much repetition with what the students had presented.

27-00:34:02

Rubens: Did they sign up for who they wanted to?

27-00:34:04

Smelser:

Yes, they did. But I insisted that they all participate, and I was able to get volunteers and spread them out according, in large part, to their own wishes. But I had to juggle some because they would cluster their choices and wouldn't choose some theorist. Some of the stuff was really hard. I assigned a lot of Parsons. And this was kind of a very funny story on my part. We got to Habermas and it is absolutely turgid stuff that I was assigning and very, very tough. Four or five of the students, including two representatives of this leading rebel group came to me and said, "We'd like you to present Habermas first." And from then on—so it changed back to the form in which I had begun. Because these students were totally lost and they did—really felt the need for some orientation to Habermas's work and where he was coming from and what he was saying and what the import was, which was what my presentation was going to be about.

So it was kind of a very funny rebellion that they took back right in the middle on grounds that they really preferred it the other way in terms of a learning experience. Now, this woman who was the ringleader, later when I went to the Center three years later, when she got up to the preparation for PhD, she asked me to be her PhD director of the dissertation. It was quite curious because she was quite inimical at moments before and I think she sort of—I sort of had the idea that she thought I was the wrong kind of person because of her own ideological commitments. I wasn't an anti-feminist but I tended to take a more remote or distant view of things. Talked about dilemmas in the literature, so on and so forth. I just tended to distance myself from anything that I taught and tried to take an overview of it. Well, she wanted to write a thesis. She had a program and it was a very strong feminist program.

27-00:36:41

Rubens: Had she been a good student?

27-00:36:44

Smelser:

Smart. Smart as anything. No question about that. She was very smart. She was Israeli. So I took her on. But that didn't seem to do the trick because she came to the Center for consultations when I became director, to show me drafted material and get my feedback. But she got the idea that I was somehow or other in opposition to what she was trying to do and that I had—and was trying to influence her to do things she didn't want to do. It was somewhat—

27-00:37:19

Rubens: And yet she had chosen you?

27-00:37:20

Smelser:

She'd chosen me but she started fighting.

27-00:37:21

Rubens: What was the topic? Do you remember?

27-00:37:25

Smelser:

Yes. The topic was to write a history of how major sociological theorists had treated or rather ignored emotion in their theories. I thought it was a very good topic. It was consonant with my own interests and I thought it was intellectually a valuable topic. I was in the middle of thinking about ambivalence and so on. So I was happy enough to take her on and she chose me. And I didn't turn many students down. I don't think I ever turned students down who asked me to chair their dissertation committee. But we waged war for a while. And one time she came down there a year or two after she was into her dissertation. She was fussing at me, assigning viewpoints to me that I didn't have. So I finally just sat down with her at a table one lunch we had together and said, "Look, Mickey, I think you've got it just wrong. I think you misunderstand what I try to do when I'm a doctoral dissertation advisor." And then I went through a philosophy of respecting the independence of the people who are doing their work, of being helpful and part of being helpful was being critical. I wasn't waging war on her. That I was in fact trying to engage, get these issues and trying to help what she wanted to say. And this one lunch had a very big impact on her. She turned completely around. It's often the case. Jeff Alexander and I experienced something like that. He thought I was some kind of enemy early on because he came out of SDS in Harvard and he thought I was representing some kind of establishment. And I was a Parsons person. He didn't like Parsons. So he turned completely around and became very dedicated midway through graduate school and she sort of did, too. And it became embarrassing for me how positive she became toward me. It was this swing back and forth. And I now still keep up a relationship with her. I just—

27-00:39:36

Rubens: What's her name?

27-00:39:37

Smelser: Mickey Kashtan.

27-00:39:39

Rubens: And where did she end up?

27-00:39:41

Smelser:

She ended up not in an academic role. It's almost a kind of one person social work. She's very interested in Israeli-Palestinian reconciliation. She lives here in Oakland. I tried to get the essence of it and couldn't completely but it's pretty much in the good works area. She helps people out. She's not exactly a therapist but she didn't continue in the academic world. I think she had a deep level of alienation from that. But it was a very interesting turnaround.

27-00:40:22

Rubens: But were you satisfied with the dissertation? Did it become a good dissertation?

27-00:40:26

Smelser: Oh, yes. Yes. I didn't have trouble approving it at all. Actually, I had influence on it. She said there was a continuous rebellious streak in her and it didn't always completely go away. But nonetheless—but she asked me to come—made a point of coming up from Palo Alto to put the hood on her when she graduated. So it became very positive, embarrassingly so in some respects because she wanted to continue to involve me in her life, which I really didn't want to do. But nonetheless, it was a kind of an interesting story.

27-00:41:06

Rubens: Well, you're using this as an occasion also to lay out your philosophy about being a dissertation director.

27-00:41:10

Smelser: Well, that was my philosophy. That was my philosophy about students. I never dictated a single topic to a student to do something that was one of my side interests that I wasn't attending to or I wanted a pet thesis that I wanted a student to work on and so on. I always took a receptive—initially receptive view as to what these students' interests were and what they wanted to do. I had, in a way, a natural claim on students because I taught this first year theory course so much that I was the first person they came into contact with. Many students have told me that I was a student friendly faculty member in that I was not punitive and didn't call people down and had a receptive role as the teacher. And I was well known in the field. I was not going to hurt their career chances by being the chair of their dissertation. But I would then be a very active supervisor in that I would take this topic that they were interested in and I'd begin talking intensively and at length about where they might want to go with it, what they'd take it: Was it conceived very well in the first place, shouldn't they redefine it in certain ways? And as chair I would make a point of having more frequent conferences with students than if I were just another member or an outside member, though I did take an active role in the history department and other departments, such as welfare, psychology, when I was an outside member. But I was probably more active than most outside members, as well. That was the way I operated. I would exert as much influence as possible—never insist that they go down a certain line but always suggest that they were going to open up a few more avenues of insight if they followed this line of thinking and I would give them references to look at and so on. But at the same time, always trying to give the feeling—it sounds like I'm manipulating but I wasn't—give the feeling that they were on something legitimate, that I was respecting their work and that I wasn't trying to duplicate myself in them. Which is the dread of all graduate students, being dominated.

27-00:43:51

Rubens:

But at the same time often graduate students come knowing they want to work with somebody or they want to work in a certain field.—

27-00:44:10

Smelser:

Well, some students came—would have come to me anyway because—maybe a couple came because they knew I was here. They certainly knew my interests. But I had this kind of recruiting kind of ground on top of that, of teaching the theory course and being on the orals examination. Because we required theory as one of the fields and they almost all would choose me because I had taught the theory course.

27-00:44:38

Rubens:

How many graduate students would you have at any one time? Were there periods of uneven—

27-00:44:44

Smelser:

It was kind of steady. In the department I'd be working with three or four at any given time and then I'd be a second member on another couple of committees. I was maybe on seven to ten committees continuously after the first year or two that I was here.

27-00:45:01

Rubens:

That's a lot.

27-00:45:03

Smelser:

I began to accumulate. Yes, that was a big part of my life.

27-00:45:06

Rubens:

While you were at Russell Sage were you in communication, for instance—

27-00:45:10

Smelser:

Oh, yes. I kept my—and when I was at the Center I had a number of students on whose committee I was whose work carried on. I didn't take any new students.

27-00:45:22

Rubens:

Okay, after '94.

27-00:45:24

Smelser:

Because I was away and I wasn't teaching. But the last one didn't get his dissertation finished until nearly 2000 so the hangers on were there. I did one thesis when I was in Europe of a person I never laid eyes on during all my correspondence. This was a guy who had been working with Bendix and he was writing a dissertation and he ran afoul of Bendix. Bendix thought he was going in the wrong direction and it became kind of conflictual. They decided to part ways. And this guy then contacted me, not on the original topic, but on another topic. It was on the history of the institutionalization of science at Cambridge University. So I decided I would do it. I did the whole thing by

correspondence. He wrote a thesis that was satisfactory and it all got approved without—

27-00:46:27

Rubens:

Was there any reason to communicate with Bendix about the handoff?

27-00:46:31

Smelser:

Oh, they had parted. Bendix was happy to be out of it. And so it didn't create a problem with Bendix for me. We weren't competing for this guy. There would have to be if it were a kind of voluntary thing and if Bendix were still interested in having him. That would have been a touchy situation. But I never had that with any colleague and this was a case where he and Bendix decided on a parting of the ways and he was looking around for somebody else.

I enjoyed teaching the theory course for the last time. I know I didn't make up my mind it was going to be the last time but it was a—in retrospect turned out to be a memorable experience because it was my last theory teaching course. I think maybe I should go on to the second year where I served as chairman. This was a desperation chairmanship on the part of the administration.

27-00:47:40

Rubens:

Once again.

27-00:47:42

Smelser:

Well, they didn't have any real candidates at that—either someone was going to be away or they didn't want to do it and I was back and so the dean came to me and asked if I would chair the department.

27-00:47:58

Rubens:

Who was the dean at the time? The dean of letters and science?

27-00:48:02

Smelser:

Was it Mendelsohn? I think it was Gerry Mendelsohn. I'm not sure. But I said, "Okay, I'll do it. But I'll do it on one condition. That you give me six weeks off in the spring to go to Bellagio."

27-00:48:22

Rubens:

Oh, okay. That's coming up.

27-00:48:24

Smelser:

I had been admitted. I'd been chosen to go to Bellagio. I was not going to give that up for anything. So I asked Vicky Bonnell to be vice chair and I said, "You're going to have to take the chair over for six weeks in May." The dean, said yes. His back was against the wall. He needed somebody.

27-00:48:48

Rubens:

So you began this in September of '91?

- 27-00:48:50
Smelser: That is correct. But I had been admitted for the spring of '92; they admit you many months in advance. So I—
- 27-00:49:02
Rubens: Well, we'll get to that in a minute. So are there issues that particularly were outstanding for you during this second chairmanship?
- 27-00:49:08
Smelser: Yes. I'll mention two, both having to do with personnel, which are at the center of every chairman's life. I think you spend 95 percent of your time dealing with personnel and that's too much. It's the center of your work. The university had passed a new rule in connection with the anticipated uncapping that required faculty members, tenured faculty members, to be reviewed every five years, whether they wanted to or not. It's called post-tenure review. Previously a person could decide not to be reviewed for advancement.
- 27-00:50:01
Rubens: And the point of the review is you go up another step?
- 27-00:50:05
Smelser: Review and recommendation, right. These usually take place on schedule because of the steps of the ranks, associate and full. Associate professor was two years. Each step was two to three years and normally you get reviewed at the end of these. But if you don't want to be reviewed, this was the old arrangement, you didn't have to be. You also get reviewed when you get an outside offer because the chair usually presents a counteroffer to recommend to the dean and—
- 27-00:50:42
Rubens: Is that just automatic? I thought you asked for that review. Well, you have to tell them—
- 27-00:50:47
Smelser: Well, when you got the job offer from outside, if you are so interested, if you don't want to just take it right away or if you're interested in either improving your local situation or wanting a choice, you go to your own chairman and say, "I've got this offer or these offers," and then the chair will review your—
- 27-00:51:07
Rubens: Now, there are occasions when the chair is happy to see somebody go or the department is—
- 27-00:51:31
Smelser: The chair himself could say, "No, I'm not even going to bother reviewing you." As I say, there's some atrocity stories of a guy coming in with a big offer from someplace and the chair says, "I wish you the very best." That's always an apocryphal sort of thing. But usually, if they want to keep you they will review you and recommend an improvement in your situation. And that happened to me several times in my career.

So a person could go for years and years and years without being reviewed but the university passed, sometimes in the eighties, the senate went right along with it, saying that no matter what, a person should be reviewed every five years. My predecessor had not lived up to this regulation. He just let it go. There were some people in the department who hadn't been reviewed for five years but didn't bother. I looked it all over. I found one colleague who'd been twenty-three years without a review and another had been nearly twenty. And so I decided I would take the leadership. I don't think I should name these people. But anyway, readers may know.

The one had been pretty much in the category of what you would call deadwood for nearly two decades. A full professor but he hadn't been reviewed for that long a time. A second one was joint with another department and hadn't been reviewed for—and wasn't quite so central because his main identity had gone over to that other department. But I felt since he was still formally affiliated with sociology I should get a review for him, as well.

27-00:53:27

Rubens: So are you trying to clean house?

27-00:53:28

Smelser: I wasn't trying to get rid of anybody but I thought that it was not right to just ignore that university requirement so I followed through on it. The one, the first, with the twenty-three year lag, I called up and said I was going to do this and I would like him to come in so we could review—we could talk and he could tell me about his activity, tell me about this. He wouldn't come in. He'd refuse to come in to talk.

27-00:54:01

Rubens: What about the bio-bib? Isn't that required?

27-00:54:07

Smelser: Well, he had done that and there was nothing on it. So I call him in and I kept trying and he wouldn't answer phone calls, he wouldn't answer mail.

27-00:54:21

Rubens: Was he teaching?

27-00:54:22

Smelser: Oh, yes. He followed the minimal. You couldn't fault him for neglect of teaching or things like that but he hadn't done any research for more than two decades. So I said, I wrote a letter saying, "Dear X, if you don't come see me I'm going to write a review anyway, on my own." Then he came in. It was rather painful because it was evident that he didn't—he made noises about what he was planning to do and so on. But it was evident that he was fallow. So I decided as chair—I guess I must have developed some institutional self-confidence at this point. I wrote a letter recommending a demotion to the

dean. “We should move him down a step or two because he’s been so unproductive.”

27-00:55:20

Rubens:

Now, with tenure you can’t recommend that he be let go, is that right?

27-00:55:23

Smelser:

No, but my recommendation was a bombshell because you never do that. You freeze someone. That’s the university’s policy. If they’re not productive and they don’t ask for—he had been frozen at a given step for that long period of time. And so I said, “No, we ought to, in effect, demote this person.” Well, I should have known better. They did not accept this and I got a letter back asking me to have a conversation with him, telling him to shape up. It was a really ridiculous response to my letter but it was basically a way of ignoring the issue that I had raised. I reviewed the second person, as well, but as I say, he was a little more marginal to the department and I didn’t recommend this. But nonetheless I thought I should report this to you because it’s so rare.

27-00:56:18

Rubens:

Sure. And did it cause any to-do in the department?

27-00:56:24

Smelser:

They didn’t know. The chairman does not communicate to the rest of the department except on unusual circumstances. Charlie Glock, for example, when I had this bunch of offers from outside, went to the department and got their assurance. He got a hundred percent vote from the department they wanted to keep me here and to do everything they could. He used that as an argument to pass upstairs to get me promoted or advanced as much as he could. But normally the chair deals with faculties one on one and I didn’t discuss this with anybody. So it was between me and the dean. But I decided to take the new regulation seriously on this matter.

27-00:57:20

Rubens:

Did this fellow have graduate students?

27-00:57:22

Smelser:

No, he didn’t. He had gotten a reputation of being a somewhat punitive supervisor and even those who might have logically chosen him because of the topic—graduate student grapevine was such that he wasn’t much chosen. He sat on orals examinations and he was somewhat punitive. Students know. They don’t want to get into a relationship like that.

[Begin Audio File 28]

28-00:00:24

Rubens: Regarding dissertation students, you had many. Comparatively—

28-00:00:25

Smelser: For most of my career, I probably had the most students. I'll tell you what probably determines this distribution. I'd say that the factors that go into it are first the eminence of the faculty member. That students tend to be drawn toward those who are more famous or eminent in their field, so you get people drawn to Bendix, you get people drawn to Selznick. When Lipset was here he had a lot. These are people who are leaders in their field and obviously drawing cards for—

28-00:01:02

Rubens: Later on, Nancy Choderow?

28-00:01:04

Smelser: I will get to that. Choderow was one. Arlie Hochschild was one. I don't want to name them all but there were people with a lot of dissertations. That's one factor, is the eminence of the person. Then you have the factor of my deadwood person who establishes or gets a reputation in the graduate student underground or grapevine of not being good to work with. That they are not supportive or they're punitive and they don't follow through, whatever. So that will determine in a way. Or you get the reputation of being student friendly, whatever. That'll have something to do with it. Beginning in the late sixties or seventies, another factor came in. When we began to get more women and minority faculty members and students. You sometimes got the effect of a woman graduate student preferring to have a woman minority supervisor as well. This wasn't a major factor but it was one that came into play. And all these things have to be taken into account when you think of the differential distribution of faculty. Of course, the subject matter of the student is a determinant. If you're going to choose in political sociology you normally wouldn't go to a social psychologist to supervise your dissertation, even if you might like the person and respect the person but just not in the area of your interest.

28-00:02:35

Rubens: Now, does the chair work with the graduate advisor? Do you have a little—oh, I don't know—discussions to sort of encourage a faculty member to take someone if they're matched?

28-00:02:49

Smelser: No, it's almost all student initiated and negotiated between student and faculty member. You get a little credit for being an advisor. It goes into your bio bib. It goes into your—whatever reports are given in the department on teaching and sometimes—at times, when I was being reviewed, they approached graduate students and asked for letters of evaluation along with your course evaluations and so on. Mostly to build up a case rather than as a part of a

critical review. So it is a kind of free for all. What I think is not the case, say in the physical sciences, where a faculty member will recruit people into his lab and that lab becomes a mechanism for assigning a thesis topic. Much more structured than it is in the social sciences and the humanities.

28-00:03:49

Rubens:

Do you have a philosophy about tenure? Sounds like there wasn't much deadwood in the department but—

28-00:04:00

Smelser:

No. Well, I've gotten myself recently into that literature on the tenure dispute. It's pretty big in the current literature on higher education and the economists have joined in pointing out how irrational tenure is from the standpoint of efficiency because you keep inefficient people on and there's a lot of discussion of tenure being only a matter of academic freedom, not a job claim. There's a certain amount of literature on—perplexed literature on whether or not the increased hiring of non-tenure track people and part-time people, which is very widespread, especially in community colleges and state colleges, whether that's undermining tenure or not. So there's a big, big dispute on it. I happen to believe that tenure is a valuable institution and ought not to be eliminated. I'm not in favor of these wholesale recommendations about doing it in. However, I'm quite sympathetic with the post tenure review movements or movement. That it ought not to be just the case that you get promoted and then you're there for life. I think this kind of continues. It's very powerful. Even though you don't go down my line of demoting people and reducing their salary, which institutions—maybe there ought to be some mechanism. That's, I suppose, unrealistic to recommend that because it won't happen. But my view is that there ought to be more continuous assessment because it does have an impact on the faculty member. So I guess that's my view.

28-00:05:43

Rubens:

I meant to ask earlier, regarding the diminished share of public money that came to the university at the end of the 80s, and then end of the 90s, that deans and chairs were raising money. Did that affect you?

28-00:06:09

Smelser:

During the times I was chair, that wasn't a dimension of my life. Maybe it was because I was so obviously temporary and demanding to be temporary that that didn't come up.

28-00:06:26

Rubens:

Well, any other issues to discuss while you were chair?

28-00:06:30

Smelser:

Yes. We had a Hispanic scholar that was taken on around the early eighties and he was a favorite of the Hispanic minority of graduate students who put on a big movement for him. We carried out a search. We were looking for a Hispanic studies person at that time and we had a national competition for the

place and developed—the department split badly on the issue. We had two candidates. This was the one who was sort of the darling of the Hispanic caucus and the other one, that I favored and was openly favoring, was a woman, Hispanic also, but obviously, in my estimation, of much superior academic promise. And I fought for her but she was not the favorite of the Hispanics because when she came for being interviewed she sort of didn't buy into their particular viewpoint at the time about Hispanic studies or whatever the issues were. So this chap was appointed. The department was split on his appointment. I opposed it. By that time, a lot of the conservatives in the department had left. The department was left-wing in its orientation and went along with his appointment.

This was Tomas Almaguer. Now, we had a couple of in-department reviews of him before he came up for tenure. He was not being productive. He was not getting his dissertation into publication and once again I was for giving a negative mid-career review, we called it, halfway to the tenure decision point. Once again a big fight came up and I was publicly not very popular among the Hispanic caucus for my intellectual reservations about this guy. And then when it came to tenure there was another big fight and he was—the department was split right down the middle. There was an issue of plagiarism that came up, possible plagiarism that didn't get highly publicized but was known about. He had one book on the history of trade unionism and Hispanics in trade unionism in California. That was it. And I had a crisis of conscience because it was being considered by UC Press to communicate whether or not this plagiarism issue should be in their mind.

28-00:09:32

Rubens:

So that's a pretty serious offense.

28-00:09:35

Smelser:

Well, yes. But he still got a lot of support in the department—

28-00:09:40

Rubens:

No, I'm asking about whether you communicated to UC Press or not.

28-00:09:42

Smelser:

In the end I decided to. That just in the interests of their own—I was close to all the UC press people. And I did so not out of vindictiveness to him in particular but just because I felt UC Press should know this and they should go about their business.

28-00:10:01

Rubens:

You weren't the only one who knew? I mean, there—

28-00:10:04

Smelser:

No, no, no. It was known in the department. Actually, it was called to the department's attention by a Native American scholar who was in the department who noticed that a—he noticed that something was copied out of another source. I think it was a founded charge but he—anyway, the

administration didn't promote him, even though the department was split completely down the middle.

Well, that set up the expectation there ought to be a Hispanic slot, right. And this was when we were hiring a new person, my year of chairmanship, in the area of race relations. We had this complete genius who applied for a position, along with a lot of other people. But this was a man from Harvard who was in the Society of Fellows. He has a PhD from Chicago and a PhD from the University of Paris. He was obviously a whiz kid and the department was unanimous except for one person in wanting to hire him. And I was very favorable.

But the Hispanic caucus in the department thought it had to be a Hispanic. This was a Frenchman and he wasn't anything like a Hispanic and he wasn't identified with any particular movement. He was sympathetic to civil rights and kind of a continental European left-winger. But nonetheless, we were all really, really for him. And the department voted to hire him and the Hispanic caucus went crazy in opposition to this because they demanded a Hispanic scholar.

28-00:11:57

Rubens:

They wanted a second—

28-00:11:58

Smelser:

It was a slot. No, not second. Almaguer was gone by this point. This was a replacement, in effect, for Almaguer and they wanted a Hispanic slot that would be guaranteed filled perpetually by a Hispanic.

28-00:12:09

Rubens:

I'm sorry I'm stuck here. So Almaguer was not recommended for tenure?

28-00:12:14

Smelser:

No. The administration turned him away. He went to another institution. So this was an opening. The Hispanic caucus mobilized a lot of liberal thinking graduate students and they declared a one week strike. It wasn't very well organized and I didn't see it as a thing that was going to endure but it put me on the spot. In the meantime, it became clear that Wacquant, his name was, the Harvard candidate —Loïc was his first name. Had not submitted his application by the deadline —the published deadline in our advertisement for the position. And that came out that he was a little late. As chairman of the department, I was very much interested in getting him appointed and the administration said to us, "You can't hire this guy. He applied late." So I happened to know that in the past many departments around the campus had hired people well after the deadline, sometimes even when they didn't even apply. The search was very informal. And furthermore, I searched through the university regulations and found no rule about deadlines and ruling people out. The administration, in my estimation, had caved because there was a strike.

The person I was most in relationship was John Heilbron, who was the vice chancellor at that time. Tien was chancellor. But I didn't deal with Tien at all. I dealt with Heilbron. And so I said, "Okay, show me the rule." I became kind of a faculty rebel. It wasn't my self-image. But I decided to stick my ground. I thought they were being arbitrary and so what they did was they enacted a rule and said, "You can't hire him." So they stuck to their ground, said, "You cannot hire this man."

28-00:14:53

Rubens:

And this you think really was a response to the student protest? They just caved.

28-00:14:56

Smelser:

Yes. Think it was a political response. That was my reading at the time and they basically didn't have a defense against the practice that had been going on so they enacted a new rule about deadlines.

28-00:15:14

Rubens:

Okay, so there's the rule.

28-00:15:15

Smelser:

But ex-post—

28-00:15:16

Rubens:

Had you been in the administration, what would you have done?

28-00:15:29

Smelser:

Well, I would have said, "What's it all about? You shouldn't be compromising on political grounds. If you take the department at its word, they recommended this appointment." It hasn't broken a rule. I said, "You hadn't broken a rule because there's no rule." That was my stance. And so I played it and I never backed down but we lost. We lost that. So then my duty became convincing Wacquant to apply next year because he was really wounded. He came to the campus. He gave a talk. The Hispanics yelled at him, you know. And we couldn't hire him.

28-00:16:06

Rubens:

So as chair you could designate, "We're not filling this position this year and we're going to—"

28-00:16:12

Smelser:

No, the administration said we're not filling this position this year because you can't hire that guy and the hunting season was over. So we couldn't fill it. So my idea was, "Well, let's appoint him next year." We got a rule and we'll make sure that he gets his application in on time. So I had to deal with Wacquant during the course of that year when I was chair and even afterwards, telling him not to reject us for what happened to him because he was somewhat embittered. But he badly wanted to come to Berkeley. He resonated with Berkeley. So I spent the rest of my chairmanship and some

informal time afterwards working informally with Wacquant and getting him to reapply the next year and we hired him.

28-00:16:57

Rubens: And it was a good hire?

28-00:16:58

Smelser: Well, I don't want to comment. I'd say my reactions to him as a departmental citizen have been ambivalent. He's a famous man. There are no questions about his quality and his capacity. He's turned out to be questionable as a citizen of the department. That's all I'll say on that. But that was—I didn't bargain for this particular role and I didn't particularly relish fighting the administration. It wasn't pleasant for me. Because it wasn't in my blood. I believed in a cooperative and reasonable, always, relationship with the deans and other administrators. I was a good citizen of the university and here I was fighting like a rebel.

28-00:17:45

Rubens: Well, this seemed to be a principled honorable fight—the majority of your department supported you.

28-00:17:49

Smelser: Once I discovered that they apparently were caving in politically I got mad and said, "We're paying a price we shouldn't pay for what we've done."

Well, anyway, I'd say that those were the high points of my chairmanship. I did all the routine things naturally that a chair does. And then I insisted on my time in Bellagio, which I took.

28-00:18:52

Rubens: In this same year that you're chair, you're asked to be on a commission to evaluate athletics at Berkeley. You published in your report in *Reflections*. Why don't you say how that assignment came about.

28-00:19:37

Smelser: Okay. Well, I was already—I was taking it from my point of view. I had not played an active role in athletic policy in any sense of the term.

28-00:20:37

Smelser: Well, I do not know the insides of it. I hadn't gotten myself into the athletic world. I would have to say I was a fan, a football and basketball fan. As a matter of fact, I had been twice the honorary coach at a game. They had this institution—brought a faculty member in. You were with a team during its last day of practice. You had meals with the team. You were on a bench during the game and you were kind of—it was a nice thing for faculty relations. And I had been chosen to go—I had been taking my older son to the big game with Stanford in the early sixties and I then got an invitation to go to one of the basketball games when Campanelli was here. So I went. So I was a friend, I followed the sport and I was—but nothing in the administration of the

program and nothing in the context of that athletics always involved in. So I was kind of a neutral figure in a way. I believe they very badly wanted to have a faculty member head it up. The fact that I was a university professor, the fact that I had this history of notable committees behind me. I had the UC Press, education and under lower division education studies behind me. I was a seasoned leader of important committees.

They called this a blue ribbon commission. They really made it big and they asked me to do it. I was in close relationship with—John Cummins was very much involved and I'd always had a close relationship with Cummins. He was chief of staff for Chancellor Tien, and probably was very instrumental in influencing Tien to ask me, just because he respected me a lot and we maintained a good longstanding friendship. Played golf together. But then it was a committee that had to be something more than just a faculty committee. About a third of the committee was faculty, about a third was either administration—there was a student and there were two or three administrators on it. There weren't any coaches but there were some alums. Some old blues, they were represent—

28-00:23:13

Rubens:

Obviously there was going to be a big financial impact. So it was officers from the Berkeley Foundation as well.

28-00:23:20

Smelser:

Yes. Was there a regent alum? I'm not sure. Anyway, it was a highly diverse committee with at least three major constituencies on it and it was not—and the faculty members were sort of chosen. They were not enemies. There was an articulated anti-athletic sentiment in the faculty that's always there. We had people who were not in that camp. I didn't choose them.

28-00:23:50

Rubens:

Who's appointing the faculty? Tien who did the whole thing. The administration did the whole thing. I had no control over the membership of the committee. But we had—

28-00:24:01

Rubens:

Cummins must have made the recommendations though.

28-00:24:04

Smelser:

Yes. We had Aaron Wildavsky. He was a political scientist who was very friendly to athletics. He even had student athletes to his home and went to their practices and so on. He was an outstanding scholar. Really one of the leading scholars in the faculty. And Jack Citrin I believe was on it.

28-00:24:28

Rubens:

That's right.

28-00:24:29

Smelser:

He's political science and he was also the NCAA representative. Also very favorable, a fan of sports. So I always joked about the political science jocks. They were real big fans, Cal fans, Bear rooters. And so they were more represented on the committee than somebody who would be critical of spending too much money on athletics as ruining students or athletics as a source of academic compromising. So it was a committee that was I think either neutral or fairly favorably disposed toward athletic programs, although not loaded. I wouldn't call it stacked. It was not stacked but you're not going to get fundamental criticisms of athletics, of which I had some myself.

28-00:25:28

Rubens:

Well, the task is really to deal with deficits and to—

28-00:25:33

Smelser:

Well, it had to do with the general mission for athletics on the campus. It had to do with financial aspects. It had to do with new facilities and it had to do with outstanding issues of which the gender issue was the most important.

28-00:25:51

Rubens:

Yes. The kind of fracturing of it, that there was a women's division and a men's division.

28-00:25:55

Smelser:

Yes. And the national movement at the time was toward merger of women's with men's. And, of course, Title IX of the federal legislation called for expenditures and fair treatment, equal treatment of men and women athletes. That was very much on my mind and I myself assumed from the very beginning that we were going to recommend merger of the programs with a general AD, athletic director. Women's athletes had to be some degree separate because women and men play different sports in many respects, in many cases.

28-00:26:45

Rubens:

And Lou Lilly, who was the head of the women's athletic department, sat on the committee.

28-00:26:51

Smelser:

Yes. And we interviewed some of the coaches, the women's coaches, and I got delegations coming to me, which I will discuss in a minute. But anyway, this was the general picture and the commission was well treated. It was well staffed by the administration. Tien wanted it to be an important report. He was, of course, a kind of jock. He was a very big athletic fan. He went to all the games. He would always end his speeches with "Go Bears," and he was all gung-ho. So we attended to the general mission of athletics, intercollegiate athletics mostly, although we did consider recreational athletics. The way I put it in the report, and I was the advocate of this position—I, in a way, didn't impose it on the committee but it was satisfactory with the committee. Took the following line. That UC seemed to be playing an ambivalent game toward

athletics. They seemed to be—wanted badly to be in a major national conference, PAC 10, and showed no inclination of wanting to not be in it. But at the same time, it seemed to settle for permanent mediocrity, being seventh or eighth in major sports. And we recommended that the university make up its mind and go one way or the other.

28-00:28:22

Rubens:

Well, that's your recommendations. Was it the fact that there weren't facilities, the coaches weren't paid enough, there was fracture? All of that ate away at their excellence?

28-00:28:36

Smelser:

Well, there are jurisdictional disputes within the different sports. They're competitive with one another for budgets and so on. And, of course, we weren't as aggressive in recruiting as increasing numbers of PAC 10 institutions. Obviously USC and UCLA but also Stanford were becoming more active, northwestern schools. Washington was. We hadn't been to the Rose Bowl since 1958. So we'd been floating along with kind of an ambivalent or mixed neither here nor there role. So I felt the campus ought to decide it would go one way or the other and we gave five models beginning with the University of Chicago. Said, "No, we don't want it." And another end was Ohio State's and Mississippi's and Notre Dame's, the really almost professional schools. And so we basically asked for some kind of consistency. We didn't say, "Let's go big time gung-ho." But, of course, Tien sort of took that end of the range of possibilities and took our report as being favorable and endorsing certain recommendations. Endorsing going big time. The language of the report didn't say we should go big time but nonetheless laid itself open to that interpretation.

28-00:30:09

Rubens:

You have a nice piece in there that talks about using and then critiquing C.P. Snow's sense of two cultures being in conflict—

28-00:30:20

Smelser:

Oh, yes. That was my introduction to saying we had an unfortunate way of thinking about the opposition between academics and athletics and I used Snow as kind of an analogue. Gave a little academic reference to the report.

28-00:30:36

Rubens:

But saying it was an oversimplified dichotomy.

28-00:30:39

Smelser:

Oversimple view and it doesn't fit anymore and it gets frayed and we went on to say the distinction between athletic and academic is also not neat and we gave all the reasons of why.

28-00:30:52

Rubens:

But you're saying there's something you can look at that's common between the two, that the university is about excellence over all.

28-00:30:57

Smelser:

Yes. We stressed the continuities. And that was all my prose and my initiation. So that was our kind of plate—what we took on and it was satisfactory to the whole commission.

28-00:31:12

Rubens:

How often did you meet?

28-00:31:15

Smelser:

About every week or ten days. I asked to have some time off because I had been appointed this Phi Beta Kappa speaker, which meant two periods of two weeks away each, during which I lectured at four universities. I took a point of investigating their athletic programs when I went to these different universities because I was on the commission.

Well, we had a lot of recommendations which we saw as aggravated relations between the different sports programs, between intramural sports and intercollegiate sports. The physical education department had some anomalies in it. We had some issues of administrative reporting, too, recommendations. Athletics is an extremely turf conscious body of units that were forever complaining and wanting more share and it was a competitive—

28-00:32:16

Rubens:

With the alumni being real factors.

28-00:32:20

Smelser:

I'll say something about the alums in a second. But we had these administrative things. And then we had the issue of the merger, men's and women's. And we did a lot of interviewing on this. And I assumed it was a foregone conclusion. There were only six institutions in the country that had not merged and they had just dragged their feet. They were mostly southern and Berkeley. We hadn't merged. So I thought, "Well, this is a piece of cake. We'll just recommend we merge." First of all, we'll be in conformity with the law and secondly because it's only just that we do so. I got a storm of opposition from the women. From the women's coaches and women's athletic director.

28-00:33:12

Rubens:

And then there was a women's analog to the Bear Backers, I think called the Bear Boosters too. I forget what they were called.

28-00:33:20

Smelser:

Yes. They did not want that and they had a specific reason for not wanting it. They were fearful that it would mean a downplaying of women's athletics because the AD was going to be a man. They at least now had a co-athletic director who had her own empire, right. And their fear of merging both departments under a male, who they assumed it would be forever and ever a male athletic director, was going to downgrade them. And they were really, really tough. I didn't say uncivil but they were very forceful. I got

independent delegations of women, people in the women's athletic department and coaches came to me to work on me pretty aggressively on this. Now, that didn't prevail but we had some language in there about making certain that there was—or safeguards against informal downgrading of women. So that language was kind of a reflection of the pressure got. And, of course, we got a lot of pressure from the independent sports like tennis. There wasn't a feminist issue. We didn't get it on general feminist grounds. It was just jurisdictional political basis for opposition I thought. Whenever I would meet a coach on a social occasion, women's coach, I was not forgiven. Some thought I was an enemy really for this.

28-00:35:16

Rubens:

And, of course, now we have Sandy Barbour, who's the—

28-00:35:19

Smelser:

Well, we got a woman AD, now. No one was predicting it at the time. I had a suspicion they might be right, that there would be only male ADs but it turned out to be one of those areas in which they were able to appoint a very good athletic director.

It was a tough committee to chair for the reasons I mentioned. I got the alums on the committee and other alums who would represent themselves to me individually. Were very, very strong. Almost religious in their—and the way I put it is every alum knows with full certainty exactly what went wrong and when. “Should never have fired this coach.” Something like this. They're very strong about it. They would claim that it's been downhill since then. And tremendous enthusiasm and love for the sport. But tremendous energy and passion and commitment.

28-00:36:32

Rubens:

How about money? Many of these athletic supporters, alums, are actually giving a lot of money to the university.

28-00:36:41

Smelser:

Well, I didn't get into the status as money givers. I assume some of them had and maybe others were just Bear Backers. They just went to all the games and maybe gave some money. I didn't identify them in that category. But nonetheless, it was an extremely difficult contingency, groups coming. And then we were thinking about reducing the number of tennis courts in Strawberry by one for other purposes, a tennis group comes in and starts working on you. So it was as political a committee as I've ever been on and chairing it, of course, you were in a symbolic position to receive these visitations and these targets. Well, we worked our way through it. The committee itself kept a good culture. I developed nice relationships with almost everybody on it, including the alums on the committee, even though my own views were never—I was playing the role of the chair, actually. Really feeling the necessity to get a good strong report out.

And we also made recommendations about what new facilities to concentrate on. We did call for a new gymnasium which was not built but led to the reconversion of the Haas gym for about the same price we thought a new gym would cost. Took almost that much to refashion the old one. Improved the gym a great deal and so it was the right thing to do. We had recommendations about the dangers of the playing field in the stadium, which was artificial turf that was associated with injuries and we recommended changing this. We recommended use of different facilities. We also recommended what has sort of come to pass, the endowing of sports. We said the minor sports really ought not to be continuously sucking these losses out of the budget and we should get those enthusiasts to endow the sports. If you endow a sport, then it's going to be able to stand on its own. And we had a strong recommendation about endowing sports. As it turned out, baseball ended up being that example, where they recently killed it and said—basically the administration said—I thought it was a great ploy—“If you want to continue baseball you've got to pay for it.” So they got nine million dollars to continue baseball and here they are in the finals this year. And so that was another one. I took the thing to Tien. Tien, of course, embraced it. It fit into his own enthusiasm.

28-00:39:33

Rubens:

Did you actually write it?

28-00:39:34

Smelser:

I wrote it. I wrote the whole thing. Yes. I didn't delegate any part of it. I didn't get any flak from my committee for it. In fact, I got a very—we had a very happy last party in this house.

28-00:40:39

Rubens:

So Lou Lilly wasn't there because she wasn't on the committee. .

28-00:40:41

Smelser:

No, but Barbara Gross, the Dean of educational development was. It was mostly men on the committee, as I recall. I had worked with Gross on undergraduate affairs, as well. And I don't remember that the head of Bear Boosters was a thorn in my side. She doesn't stand out. She would have stood out if she'd been a thorn.

28-00:41:07

Rubens:

So I interrupted you. You said you had a nice party at the—

28-00:41:12

Smelser:

Yes, a party in this house. It was all for me and they gave me a nice present. So I had the committee and I see Carl Stoney, who is big alum, at different university meetings like the Berkeley Foundation and he—

28-00:41:29

Rubens:

So this was written before you went to Bellagio?

28-00:41:31

Smelser:

Yes.

28-00:41:32

Rubens: Wow, what a year.

28-00:41:33

Smelser:

It was a very busy year. That was a big assignment. That was as big as the undergraduate commission. I gave the report to Tien and I said to him verbally—I don't think it was ever recorded anywhere, "You ought to appoint a second committee and that should be on excesses in athletics," because I sort of knew in my heart how he was going to take this. He was going to take it as an occasion to get more aggressive in recruiting, to go for bigger and better standings, to go for participation in bowls. In fact, that's the way the policy went afterwards. And didn't manifest itself until a few years later, but nonetheless Cal has gotten more big time. In the recent hullabaloo about spending too much money on sports that blew up in the senate the last year, my report was often quoted for being a starting point of downhill. The enemies of athletics thought this report was not—It was the wrong one. But anyway, I was sensitive enough to tell Tien verbally, "You should report a second—"because I said, "There's no reason to believe that if we begin to take intercollegiate sports more seriously that we're not going to be subject to precisely the same temptations that intercollegiate athletics is in, in every other institution that pushes it: evading the rules, recruitment violations, questionable practices, payments, all these things which are a black eye to the intercollegiate athletics. We can't make believe just because we're Berkeley that we would not be tempted and that's absolutely true. But Tien didn't

This suggested—I had that ambivalence about intercollegiate athletics myself, that it's been a sore spot in a lot of university life and led to a lot of wrong practices. Unethical, illegitimate.

28-00:43:55

Rubens:

Now, were you recommending higher salaries to coaches?

28-00:44:00

Smelser:

No. That was not part of our—we didn't get into this. We didn't get into support of athletics. We did talk about the student athletic learning center. which we recommended be improved.

28-00:44:23

Rubens:

Citrin was very big on that.

28-00:44:24

Smelser:

Citrin was very strong on that. And I wasn't against it. I thought it was fine. But I myself knew—we had Jason Kidd in one of the courses in sociology. One of my colleagues had him in a course on—

28-00:44:43

Rubens:

The star basketball player.

- 28-00:44:44
Smelser: That's right. He's now going to be All American pro. He was on the world championship team this year at Dallas. And Kidd was enrolled in this course. It was in Far Eastern history, some kind of mistake that got him into there. But he was having real performance problems and actually the faculty member who was teaching the course was conscious about this and not—didn't give him artificial grades but didn't want him to flunk, right, so there was—
- 28-00:45:15
Rubens: That's always a little problem.
- 28-00:45:17
Smelser: Oh, academic performance is a problem—payment of athletes, and recruitment violations.
- 28-00:45:21
Rubens: I didn't understand. One of the recommendations was that the athletic rep to the NCAA reports directly to the chancellor. Why would that be?
- 28-00:45:35
Smelser: I felt that that recommendation would give us a better relationship with the NCAA. That—
- 28-00:45:48
Rubens: Heyman had kind of sullied it.
- 28-00:45:50
Smelser: Heyman had been anti-athletic. He and Derek Bok were practically run out of the association because they both were fans of downplaying athletics and they were in such a tiny minority. Heyman said he wasn't quite sure he would have done that if he knew what was going to happen to him. But he was a known athlete. He was a basketball player at Dartmouth.
- 28-00:46:17
Rubens: So you thought that would elevate—
- 28-00:46:20
Smelser: We wanted to get a better integration between the NCAA and the campus.
- 28-00:46:26
Rubens: Neil, I can't remember. So who does the athletic director report to?
- 28-00:46:31
Smelser: They may have taken it on. They may have taken this recommendation on. He was just there. He would communicate with the chancellor but we thought there ought to be a reporting relationship in the—and I don't know whether they took that on or not.
- 28-00:46:44
Rubens: But the AD, who does that person report to?

28-00:46:50

Smelser: The chancellor.

28-00:46:54

Rubens: Wow. What a demanding role that was.

28-00:47:00

Smelser: Well, I had gotten comfortable in this kind of leadership role. And, of course, I was chairing all kinds of bodies off the campus. The government taskforces and so on. This was the end of the eighties, after all, and that was my time of leadership and service. These things were becoming kind of like second nature to me and it was a challenge. I quite enjoyed it. As I said, I went around. One of the places I lectured as Phi Beta Kappa lecturer was at the University of Florida in Gainesville. That lectureship, by the way, is something that—

28-00:47:49

Rubens: We need to talk about how that comes about.

28-00:47:50

Smelser: Shall I do it now or should I—

28-00:47:52

Rubens: No, I don't think so. I think we're almost finished for today. Let's finish up the athletic and then we'll—

28-00:47:59

Smelser: Okay.

28-00:48:00

Rubens: You're saying when you went to Florida, though?

28-00:48:02

Smelser: Well, when I went to Florida, I went around. I went to the athletic facilities to see what they were like. And Florida, of course, had just recently built a new multi-purpose gym. It was absolutely heavenly. Full of potted plants and stands that were movable and everything. It was just state of the art. I can't believe how much it cost. But it was just absolutely beautiful. So I wandered around it and then went into the offices of the various athletic—some coaches and talked to them about their program. Now, the University of Florida is pretty big time in the Southern Conference and it takes athletics much more seriously than frankly anybody in the PAC 10 takes it. So I went and talked to these coaches and I got this very interesting feedback from these coaches, all of whom were—took my—I told them who I was and what committee I was on, so on and so forth. They all began to gripe to me about their situation of not having enough money. Here I was in the middle of this grand palace. I said to myself, "This athletic enterprise has no bottom." Even these well off and completely beautifully endowed institutions are crying poor mouth about their needing more. It's kind of in the nature of the case that they are. And that's one of the problems of intercollegiate athletics. None of them make

money and they all feel strapped even though there's a very big business. So there we are.

28-00:49:37

Rubens: So just a final conclusion. A lot of these recommendations were—

28-00:49:44

Smelser: Taken on. Yes. It was a successful report in that regard. It led to-internal administrative changes. Dave Maggard left about the same time. There was either an acting or—he went to Florida. He went to Miami, Maggard, as AD there. There may have been a couple of acting ADs which—I think there might have been another AD in there, before Sandy Barbour. I didn't follow it all that carefully after the report.

28-00:50:16

Rubens: Were you brought in to advise on hiring her?

28-00:50:20

Smelser: No. People from other universities would come visit me, athletic departments. The report got some attention outside the campus and they—a lot of little delegations from different places came and asked me about the circumstances and so on. So that was a little feedback from the report, was that I became temporarily an academic athletic spokesman because of the report.

28-00:50:46

Rubens: It's 11:40. I think we—

28-00:50:50

Smelser: Well, we can stop now.

28-00:50:50

Rubens: I think we want to do the Phi Beta Kappa lectures and the—

28-00:50:54

Smelser: That's not too long.

28-00:50:55

Rubens: Then Bellagio and then we can do the intellectual work of the decade, and then the office of the president. And then your last year of teaching.

28-00:51:17

Smelser: That'll be brief. I taught at San Diego and Irvine. So, I'll add a little bit to that about my duties as a university professor teaching on other campuses. I'll say some general observations about it.

28-00:51:32

Rubens: Great. So I think we'll end for today. Thank you.

[End of Audio File 28]

Interview #15 July 12, 2011

[Begin Audio File 29]

29-00:00:00

Rubens: Hi, Neil.

29-00:00:01

Smelser: Lisa, hello.

29-00:00:03

Rubens: We are on number fifteen and it is the 12th of July 2011. And I thought we'd begin today with talking about your month stay at Bellagio and then the book that comes out of that.

29-00:00:16

Smelser: Yes. I mentioned that I had applied for Bellagio in 1991 and got admitted and then I was asked to be the chair of the sociology department and I said, "Okay, but only if you let me go to Bellagio for a five-week stay." And I wanted to go there in the best months of the year, which are May and June. So I said, "Okay," and the dean bought it. He had to. He badly needed a chairman, so he agreed readily to my going away. I mentioned last time, too, that I was working on some themes in the history of American sociology, a book which has never yet been written. Bits and pieces of it. I may go back to it. I don't know.

What I did in that connection is—I knew how good Bellagio was. I had been there twice for conferences but Sharin hadn't come. These were only for four days. They were on topics that were organized in connection with my collaboration with Cardozo and Makler and Martinelli in the International Sociological Association. So I knew Bellagio. I knew what a magical place it was and I had this dream of going there, living right in the villa and doing work. But I was going to do it on a theme or a couple of ideas on the history of American sociology.

So what I did was I packed dozens of journals and books into boxes and mailed them to Italy so they'd be right there when I arrived. The Italian mail service did not deliver them. None of them arrived. When I was there, there were no books and nobody seemed to know where they were. And here I was. It was the month at hand and I had no way—really no way to proceed. And I tried negotiating. I got an official at the Bellagio Villa to try to intervene with the Italian mail service. No success. He sort of got a hint that if I paid a certain amount of money they would accelerate their investigation but I didn't want to do that, either. And they never arrived until after I left Bellagio.

29-00:02:42

Rubens: So you did get them back eventually?

29-00:02:45

Smelser:

Here. Yes, they came back here but I never saw them when I was in Bellagio. This created a problem for me. I had signed up with Sage Publications, however, for a kind of—not indefinite future but some time in the future to write a book in a strange little series that they had organized called Survival Skills for Scholars. I think that was the name of it.

29-00:03:09

Rubens:

Why are you calling it strange? Just odd?

29-00:03:11

Smelser:

Well, it's sort of a gimmicky thing. They had books on how to get tenure, how to chair a department. There were maybe ten in it. And I negotiated with Mitch Allen who was the head of the series for Sage to—they wanted one on committees. And I said, "Well, that seems to be—I could talk about chairing a department, I could talk about a number of the things. I could talk about the path to tenure," things like that. But I was the one who chose committees and I negotiated, I think properly, not to make it a scholarly work of going into the, say, the public administration literature or the idea of—or the education literature. I just didn't want to do a scholarly work on this. It's not very good scholarship in the sense that it's somewhat formal and official. So I wanted to—I said, "Okay, I'll write it but I want to make it biographical. I want it to be about committees and commissions that I've been on." And there were a lot of them by that time.

So I decided I will make it general. I will talk about what academic committees are like, what their distinctive characteristics are. I'll talk about strategies. But I want all my illustrations to be out of my own past. They liked this idea. They thought it would be a grabbing feature of it rather than dry summaries of literature on political committees and whatever. So I decided to do it that way. But I didn't bring any material. Of course, I didn't need to bring any.

29-00:05:06

Rubens:

So this discussion had been had earlier? The terms of the book had been set.

29-00:05:13

Rubens:

But you didn't know when you were going to get to it.

29-00:05:17

Smelser:

I wasn't anticipating writing it when I went to Bellagio. I was going to do it during the next year or two. It was all vague. I didn't have any deadlines for them because it was this open-ended series. But as an adaptive frenzy, I decided, "Okay, I'll work on this book here in Bellagio." And because I was equipped with my own memory and my own history and with many ideas I had developed over time, I said, "Okay, I'm going to spend this time—I'm going to launch that project here." I didn't say I was going to finish it because I didn't know how open-ended it was. But work circumstances there are

perfect. You're completely isolated. You don't have libraries. Bellagio doesn't have a library. They do supply computers and email and—

29-00:06:14

Rubens:

I suppose the internet was not quite as rich as it is now.

29-00:06:17

Smelser:

No, but I had email and I could keep up minimally on that. I didn't need more advanced Google searches for this book, you see. It was all coming from within. I didn't have to do the way I have done with most books. So it began to flow and it flowed very smoothly and I really surprised myself by finishing the entire thing during that five week period. Well, in the meantime, having a lot of leisure to wander into Bellagio—we did a couple little side trips into Milan. We wandered through the countryside. We explored the Lago di Como—it's right on the peninsula.

We went boating and nearly met tragedy because we ran into high winds. Not that it was going to capsize the boat but I had to row back into the wind. It nearly killed me. Thought I was going to get some kind of heart seizure because I was so exhausted. No, we had a wonderful time there. It was in the spring. The weather was just as we loved it. We went to Bergamo. And my daughter was in the meantime in an education abroad program in Florence, so she came up with her then boyfriend. Not the person she married but he had come over. They came up to visit us. We went to Florence afterwards and so it was a perfect kind of coincidence that she was in the education abroad program. That's my youngest daughter, Sarah the artist, who was at a print making studio in Florence.

29-00:08:02

Rubens:

Now, who was there and was there interaction with the other people there?

29-00:08:09

Smelser:

Oh, intense. It so happened that one of my very dearest friends, Robert Wallerstein, who is head of the—who was in the Psychoanalytic Institute when I was a fellow and I taught a course with him in San Francisco earlier on that campus—he happened to be there with his wife. The man who wrote *Catch-22*, Joseph Heller, was there with his second wife and we became immediately attached to them. Michael Young, the senior British sociologist was there. And then there was a big diversity of people. There were a couple of poets from the Philippines, a couple of scholars from Eastern Europe. It's very highly diverse. As a matter of fact, I can tell you a story about my mission. The Rockefeller Foundation had sort of decided, for better or for worse, to be very politically correct in its selection of fellows. They wanted the greatest diversity possible. Many women, minorities, completely international. But they only had fifteen spots. So it was a big job. They were overloaded with talent.

29-00:09:15

Rubens: And do you apply or do they—

29-00:09:16

Smelser: You apply. You apply in a regular way. You describe your project. You send them all the relevant materials on yourself and so on. And I heard from a spy who knew members on the selection committee that my appointment was a little controversial. There wasn't any challenging of my credentials but the idea was that I was "too establishment", meaning that I didn't fit into the—I was in these comfortable old categories. A white male senior person who had contributed in a conventional and in a professional way to his own discipline and so on. Not that I was opposed but this question was raised and I thought it was very, very interesting that there was some issue that I was in the social science establishment, meaning that with all the connotations of that. Perhaps more conservative than they'd like. I certainly wasn't representing any minority group or whatever. And I was sort of amused by it, particularly since I did get the appointment. I would not have been amused if I hadn't.

29-00:10:41

Rubens: What were your spies? You don't have to name them. But I mean—

29-00:10:44

Smelser: This was a man in the Ford Foundation who lived in New York and he knew members of the selection committee and they told him and he told me. I didn't commission him to tell me. He just revealed the materials to me. And it was a reliable report, I'm sure.

29-00:11:00

Rubens: And so how would you meet with other people? Was that informal or did they have seminars?

29-00:11:05

Smelser: Oh, no, no. They had it all scheduled. It was a formal setting and all meals were collective and for dinner you had to have ties. At one time they had a black tie tradition. It was very, very, relatively speaking, rather stiff. It wasn't—it was an open and warm environment. But we would have all meals together. But the dinner was the big one. And then, once a week, a fellow would present the results of what he was working on, or she. And so there was a collective element. And we were all crowded into one villa. Of the fellows that overlapped with us, we saw maybe four or five of them on a continuing basis afterwards for several—for years. Artists in New York. Heller we saw independently when they happened to be out here and we went to New York. We saw the Hellers. Of course, Wallerstein was a continuing relationship anyway. But it was very close. As a matter of fact, I included discussion of the Bellagio experience as an odyssey in my book and analyzed it from that standpoint. So it was collectively very— There was one man, Philip Rieff — he's the sociologist/philosopher who was on the Berkeley faculty during my very early years and then he went to Penn—he's one of the few people in the world I don't like. I just didn't like him. He was an arrogant, dismissive and

aggressive kind of academic who was a complete snob and he is not—and he's offensive in his interpersonal behavior. Not necessarily to me. He respected me but he alienated himself from practically everybody else there because of his behavior. By and large, it was, however, an extremely collective and communal and positive experience. I wanted to go back in 2002 but they didn't give me a second one.

29-00:13:23

Rubens:

Can you anticipate applying again or do you feel a little shy?

29-00:13:27

Smelser:

I don't. I think they give a lower priority to people who want to go back. I would love it, of course, but it's not a big thing in my mind.

29-00:13:37

Rubens:

And was there room for Sharin there since you're saying there were artists?

29-00:13:39

Smelser:

Oh, yes. Well, we did not live in the villa. There's one big villa which overlooks the lake. The fantastic classical grounds. The Rockefeller Foundation agreed to keep it in pristine form when they took it over. There's a villa in which many people live and then there's a second adjoining little villa that's just a little bit down the hill and we lived in that. And you walked up some fifty or a hundred steps to the big villa and you had all your meals up in the big villa.

29-00:14:14

Rubens:

Do you write in the same room that you—

29-00:14:16

Smelser:

I wrote in my own room. Sharin was there. She did a lot of photographic work and she had her own projects and enjoyed them. She wasn't at loose ends. And spouses were incorporated into the meals and fully, yes. The conferences, the one like I went to earlier for shorter periods, met in the villa but the participants lived down the hill on the lake in a separate building and they don't permit spouses there at those conferences. If they come, they have to live—they have to stay in Bellagio, in the town of Bellagio. We had a nice leisurely—we did an exploration of every ice cream shop. You had to walk about a quarter to a half a mile into the town of Bellagio. So we would go in there in the afternoons. Every afternoon we walked into the town and would treat ourselves.

29-00:15:25

Rubens:

So you pursued your work schedule. You'd get up at 6:00 and write?

29-00:15:30

Smelser:

I would get up early. Not always at 6:00 but I would get up early in the morning. I'd work more or less steadily until noon. We'd take a break in mid-day and I would work some in the afternoon but not really.

29-00:15:41

Rubens: And you got it done?

29-00:15:43

Smelser: And I got it done.

29-00:15:44

Rubens: It's not wholly unscholarly. You have citations.

29-00:15:48

Smelser: Well, this literature I was familiar with. I had to complete the formalities of whatever scholarly references I made. But I referred to literature with which I was somewhat familiar, like the small group literature in social psychology which applied very much to how committees work.

29-00:16:10

Rubens: Just to stay on the book for a minute. We've talked about your experience on so many committees. On the back of the book you list the committees.

29-00:16:18

Smelser: Some of them, yes.

29-00:16:20

Rubens: But it's organized very interestingly. You talk about the committee as a creature and then talk about its collective thinker and umpire and unifier, those that rubber stamp or are competitors in the power game. I thought that was really interesting. I wanted to ask you particularly about—you talked about it's important to know when you're being co-opted and how to strategize about that. But I don't think you talked about—I was wondering about the issue of stacking the committee.

29-00:16:51

Smelser: Well, I guess I could have illustrated it. I didn't want to write exposés. But I could have discussed this issue at some length with respect to that athletics committee because here was a committee that we knew what the chancellor wanted. He was a jock. He was very pro-athletics. He didn't try to influence us directly. Just set up the committee. However, the committee was biased in a friendly direction. No question about that. So the issue of its independence of the chancellor became very, very relevant and I was very much aware that I didn't want to simply be a voice that would say, "Go, Bears," the way Tien would have actually really very much liked it, I think. But all committees that are formed by a parent body face this issue of exactly to what degree are they going to be rebellious, what degree are they going to go along with what is thought to be the main policy thinking of the administration and endorse things. It's an issue.

29-00:18:02

Rubens: Yes, of course. And related to that, I wanted to ask you about the phenomena of caucusing separately from the committee—lining people up and trying to have a caucus, something that would—

29-00:18:19

Smelser:

Yes, well, that's an important role. Important role for the chair is to be aware and it's also a kind of an axiom in committee or group work, that you always know the outcome of a vote before you take it in the sense that you've maintained these individual ties. But, of course, caucusing can also take another turn when minorities of the committee themselves meet separately and decide what they're going to do. That's the caucusing of a minority and that's, of course, a very tough thing for a chair to deal with, when you have basically a second committee that figures out its own strategies and is going to try to push its own point of view. That happens all the time.

29-00:19:04

Rubens:

We've talked about it not so much in formal committees but in terms of what you encountered with the sociology department.

29-00:19:09

Smelser:

In the sociology department it was blocks and it's always tougher to deal with blocks than it is with—I've countered it to some degree in my work with the National Academy, in this very big work on the future, cutting edges of social science research. We have groups of people with common interests who would themselves get together. The anthropologists and some social psychologists formed a little group because they didn't think they were represented well enough among the economists and others. So you got cliques and blocks and that provides a much tougher assignment for chairs than just simply having a collectivity of people who are interested in the common outcome. So that had to be discussed.

29-00:19:58

Rubens:

Just a couple other questions. Well, it's very you. You talk about the final party after a report. But not every committee has a party afterwards. It's not necessarily a—

29-00:20:17

Smelser:

No, no. It's not every.

29-00:20:19

Rubens:

You seem to have been animated by recognizing and celebrating the work people did.

29-00:20:23

Smelser:

There is this body of literature that I cite on task groups that comes out of social psychology and they say, okay, they go through phases. There's a phase of searching and there's a phase of trying out ideas and then there's gradual work toward commitment and then at the end there's what they call expressive behavior. A lot of joking, a lot of expression of friendship and solidarity or remembering we're in a special group and we'll meet again and so on and so forth.

29-00:20:54

Rubens:

Kind of mending also whatever the discords had been?

29-00:20:56

Smelser:

They call it tension release in part. And in a way the party has that. Celebratory. We had a great party, for example, here in this house after the athletics committee finished its work. And even if you don't have a party, there is this hail-fellow-well-met sort of feelings that developed once the work comes to, combined with some sadness because we're ending. There's a kind of a death of the committee as another feature of it. As a matter of fact, some of the language in there predates my odyssey work. A finite group working together. It's not a very emotional odyssey usually because many are very dry and many are very boring. But nonetheless, it contains some of the elements of an adventure trip.

29-00:21:49

Rubens:

Did you ever really get mad at anyone in any of your committee, commission work, and did you have to contain that?

29-00:22:11

Smelser:

Yes. Oh, yes. Particularly it ties up with this block of people who get together and bring in a very forceful but not workable position and they sit on it and defend it. I typically get very angry at this kind of thing because it disrupts the leadership and the process.

29-00:22:39

Rubens:

And what would you do about it?

29-00:22:41

Smelser:

Just work with them. I was very, very careful. If a chair loses his cool, that is about the—that's a road to self-destruction and ineffective leadership. When the chair is designated, there's an expectation that they'll have a certain type of interest in what goes on and not become too partisan themselves and not blow the stack because that's divisive in the committee work. Oh, yes. And I would get angry with individuals who were too aggressive in pushing their point of view early in the committee's work, prematurely in the committee's work. Occasionally I would work with them independently, quietly, on the side and talk. Try informal influence. But I never tried to humiliate anybody in the committee. I think that's also very self-defeating behavior on the part of a chair.

The last two chapters I tried to pay attention to the literal survival skills. So those were rules of thumb that a member, a responsible member ought to observe. That was a—I wouldn't say a concession but an adaptation of that book in the direction of the series in which it appeared.

29-00:24:07

Rubens:

I think it's very useful book. What was the reception?

29-00:24:11

Smelser:

Well, the reception was very good. As a matter of fact, there were three or four occasions in which—groups in which I was implicated or associated. The

staff of those groups would make mass buying of that book mandatory for their work. The example I'll give you is the National Research Council. It is a group within the National Academies which forms dozens of committees and task forces and panels to do work. It's all committee work. The head of the staff that I worked with knew about this book. Bought, I think, fifty or seventy-five copies, brought it into the National Research Council and gave it out to every chair who—for a period of time that was happening. There were a couple of other occasions in which a dozen or two were purchased for purposes of handing it around. So the book, I get feedback on it still to some degree. And even though it's not in the normal genre of the kind of scholarship I've done, it was one of the—it was enjoyable to write.

29-00:25:26
Rubens:

And you got it done quickly. You didn't string it out.

29-00:25:30
Smelser:

It was a good feeling of accomplishment and it was published smoothly and given ample publicity so as to sell these books, the Sage Publications. And people in the academic senate took an interest in it because the academic senate is a—committees are their life blood. A couple of the chairs of the academic senate were interested in getting it distributed and so on. So I'd say, by and large, it got a quite positive reception. I never got blasted for it.

29-00:26:07
Rubens:

And it's got a continuous life. It came out in 1993.

29-00:26:10
Smelser:

I think it's still in print.

29-00:26:13
Rubens:

Do you think we've done enough on Bellagio?

29-00:26:18
Smelser:

I think we have. I think I got the spirit of Bellagio and the characteristics of that work.

29-00:26:24
Rubens:

Oh, I just wanted to ask you what was the connection with Heller? What was the glue that made you two really—?

29-00:26:35
Smelser:

He was an interesting man, kind of a cynic about life. He had certain views about academics. As a matter of fact, our initial meeting was—and his wife was very interesting and my wife more or less immediately started getting on with her so that obviously helped. But Heller developed a picture of me as being a—it wasn't exact—totally flattering picture, although he made it flattery. I was the kind of guy who could get anything I wanted in the academic world. Grants, subsidies, invitations to interesting parts of the world and he would tease me a lot.

29-00:27:16

Rubens: So he did some homework? He knew—

29-00:27:18

Smelser:

Oh, no. It just came up where we'd been in conversation. And so he developed this stereotyped picture of me as a big operator. I would joke with him and sometimes go along, sometimes tell him it was crazy. But it was kind of a good humored relationship. Then when my daughter's boyfriend came up with her to visit us at—he had come over to visit her in Florence and they came up to have a meal at the villa. Turned out that his life was more or less transformed. The boyfriend's life was more or less transformed by reading *Catch-22*. He just loved it and he began—when Heller was there, he just jumped on him and sort of began asking him about this and that. Heller couldn't even remember all of the things that the boyfriend had brought up. That was another very cementing kind of thing. And it just became one of these situations in which you fall together. The four of us. It was really not just me and Heller. It was his wife and Sharin always collectively. We'd sit together at lunches and we would take walks together. And he was, of course, a very interesting kind of guy. Interesting in many respects but one of them was that his blockbuster occurred early in life and he never wrote anything like that again. He wrote a lot of books. But none of them—You say Heller, you say *Catch-22*, right. And that created a little bit of a lifetime problem for him because he never really matched that tremendous fame and fortune.

29-00:28:58

Rubens: Did he talk about that?

29-00:29:02

Smelser: Not too directly. I sensed it.

29-00:29:06

Rubens: Well, so shall we move to the Phi Beta Kappa lectureship?

29-00:29:10

Smelser: Oh, yes. We can spend a little bit of time on that.

29-00:29:13

Rubens: That was '91, '92.

29-00:29:14

Smelser:

And this was right in the middle of my academic athletics committee and my chairmanship. What it was is an arrangement that the Phi Beta Kappa makes with scholars, known scholars, most of whom I think are themselves past members of Phi Beta Kappa or became Phi Beta Kappa when they were in college. They define it as follows. You pay visits to eight institutions and these are institutions to which you would not otherwise go. I wouldn't call them out of the way but they're not the major center—major academic centers that a Berkeley faculty member, for example, would typically go to like Harvard, Princeton, Chicago, Michigan, North Carolina. The elites. But they

are places where you wouldn't go and you're supposed to go and give lectures.

29-00:30:22

Rubens: Do they pick them or do you pick them?

29-00:30:24

Smelser: No, no. It's negotiated. I said, "I'd like to go to the South and I'd like to go to New England." Well, they agreed with that. It's fine. There are plenty of institutions in both those parts of the country that fit the category. And what you do is you go for a half of a week to each institution and you have a negotiated program with each institution. You may appear in front of certain classes. I appeared in front of some economics classes because of my work in economic sociology, some psychology, some meetings with faculty. Mostly meetings with students. If they had a Phi Beta Kappa chapter, I would give a presentation to that. And then I would give one big public lecture. And they could be the same one in different places because it was different places. The institutions I went to on the first trip were Hampden-Sydney College, one of the two remaining male colleges in the United States, in Virginia, not too far outside of Richmond. And I went to Randolph-Macon, which is a women's school in Lynchburg, Virginia. Those were very out of the way. I then went to the University of Kentucky and the University of Florida and gave sessions there. Two week trip for these four institutions. And then another two week trip for four other institutions. I went to Holy Cross, I went to University of Maine, I went to University of New Hampshire. And on the way I went to University of Oklahoma.

29-00:32:16

Rubens: So all of these are—they're not top tier schools.

29-00:32:18

Smelser: They're not top tier schools and that's part of the picture. They wanted to get eminent scholars to come to these places. I had one lecture that I had developed that I'd given to the American Psychoanalytic Association in the 1980s and I did it because I was involved—the Institute for Government Studies, which was doing work on California as a state, as an economy, as a type of polity and on California culture. And I joined in a lot of the discussions on that completely voluntarily. So I fancied up a lecture to give to the psychoanalysts called the "Myth of the Good Life in California". And it had to do with an analysis of a lot of the cultural symbols of California, the dream, the California dream, the utopia and the life of ease, the life of plenty and so on, on one side, and then the darker side of the myth of California, which includes people going crazy and social disorganization and anomie and lots of suicide and divorce and Hollywood pathologies and I even included the Donner Party myth in it. And I did it under this rubric of ambivalence that later was to become so important in my thinking and writing on psychoanalytic matters. And so I said, "Well, basically California is a dream, mostly positive, but you never have positive dreams of instant success and

wealth and happiness without paying a certain price of punishment.” In other words, part of the American myth—an extreme version of the American myth of success and happiness but at the same time I picked out all of the corresponding negative factors and I wove them into a—

29-00:34:35

Rubens:

I’d love to read that. Where is that?

29-00:34:38

Smelser:

Oh, it appeared in a very remote place. First of all, I didn’t publish it for several years. Then somebody discovered it and asked me if he could publish it in the *Humboldt Journal of Sociology*. It was at Humboldt State University. A little obscure journal there. I published it there. Then I put it into my book on *The Social Edges of Psychoanalysis*, as one of the essays in there.

29-00:35:08

Rubens:

Oh, I have that book. I’ll read it.

29-00:35:08

Smelser:

Well, I decided, “Wouldn’t this be a good lecture to give as I went around to the different parts of the country.” So it was my major lecture. Every time I mentioned I wanted to give it, they said, “Fine, beautiful.” And, of course, if you go around the country and give it, you excite all kinds of anti-California sentiments. Audiences read it mostly as being California bashing, which I didn’t intend it but they liked it. It had enough of that in it so that they would selectively draw it out. There were a lot of funny incidents when I gave this lecture. In Maine, for example, somebody in the audience goes, “Is there a distinctive culture, myth, about Maine?” Well, I didn’t know a myth about it, so I said general things. And the place where it was most avidly received and most treated as California bashing was in Florida.

29-00:36:23

Rubens:

Economic competition –the fruit industry?

29-00:36:24

Smelser:

Competitive leisure state and so on. And somebody in the audience asked, “Is there a myth about Florida?” I was, by that time, getting a little irritated because everyone was trying to get me to say more and more negative things about California. So, on the spot, I said, “I think the myth about Florida is death,” because the original myth was its fountain of youth. That’s what Florida was supposed to be, right.

29-00:36:55

Rubens:

Yes. Ponce de León.

29-00:36:56

Smelser:

Ponce de León’s fountain of youth, right. So I said, “That’s something, isn’t it.” And that became part of a big discussion.

29-00:37:03

Rubens: The Seminole Indians forced into the everglades.

29-00:37:05

Smelser: And then I said, “Well, look, what do people do? They go to Florida to die.” Retire and die. They didn’t like that. But I sort of did it on the spot, largely irritated, out of irritation, I suppose.

29-00:37:21

Rubens: California looms so large in the national imagination that it—

29-00:37:26

Smelser: International imagination. When I was director of the Education Abroad Program, all of my students were from California. They’d go off to these English institutions and meet these myths about suntan and beautiful women and the good life that all the—they got a lot of exposure to that. In fact, that was one of the sources of input into my own thinking, was how my own students all got treated as Californians, and in a way the expectation of the British students were that they live up to it. Be the kind of carefree, open, happy types that Californians are supposed to be. And so the California phenomena are international phenomena.

I’ll say something about the Stanford Center. In my first year at the Center, at the end of my first year, I was in fact over giving the Humboldt lectures in—giving the Simmel lectures in Humboldt University in the spring at the time the—about the time the Center year was over, although I left before it ended and my associate director took over the graduation stuff. During my absence, one fellow committed suicide.

29-00:39:23

Rubens: Must have been tough.

29-00:39:25

Smelser: A very big tragedy and, of course, my Associate Director, Bob Scott, carried the biggest burden of it. But when I came back, the other fellows were still concerned, still disturbed. It’s a huge event when something like that happens in a collective community. And among the other things, a delegation of the fellows came to me and they said, “You’ve got to warn incoming fellows about the darker side of the life of the Center: That there are adjustment problems; there are people feeling that they aren’t getting their work done, that they’re guilty of having the honor and not living up to it, that kind of thing.”

29-00:40:04

Rubens: Hierarchical or disproportionate relationships to each other intellectually.

29-00:40:08

Smelser: Yes. The interpersonal side of it, too. Invidious comparisons will be developed. But I did not think it was right to treat—I thought it was infantilizing fellows to tell them, “You’re going to be unhappy part of the

time.” No way to do it. I thought, “This is no way for a director to do it.” So I rejected this understandable pressure from some of the fellows who were themselves sort of shocked by—

29-00:40:39

Rubens: They’re trying to wrestle with what happened.

29-00:40:40

Smelser: Yes, they were wrestling with their own grieving or to the degree that it was—it wasn’t really deep grieving in the usual sense there but those reactions happen when some event like—it was a grisly suicide. So what I did was I said, “Okay, I’m not going to do that but I will do something.” So at each orientation meeting I was the first speaker of the year and I gave my California speech. “Look, there is a light side, there is a—“ But I didn’t apply it to their experience.

29-00:41:15

Rubens: There’s no question that there’s an arc. That even in a class that you teach there’s a kind of honeymoon period and then sometimes an elation but there’s always at some point some kind of disillusionment or conflict that emerges. Maybe not amongst the whole group but a few that just get upset and then there’s usually a resolution.

29-00:41:48

Smelser: There’s a real testing often that takes place. In classes, it’s very true what they’re saying. And I used to be very much aware of the evolution. It wasn’t always typical and wasn’t always the same but there’s kind of an evolution of a class—what I used to call a classroom culture. Even a big lecture class. My theory classes would give evidence of this kind of initial enthusiasm and then the letdown and an irritable session and then a kind of a pulling together. You sensed it in questioning, you sensed it in students talking to you or talking to each other that you got privy to. And I developed a kind of a theory about the year abroad when I was directing, that that had a rhythm. That you had this fantastic—I call it the U shape. That is there is fantastic euphoria upon arriving. And they all did. They all came to London and everyone was in heaven, all right, that they’re having this magnificent year abroad. And that would last for a while. And then it would begin to get tough. Winter would begin to set in. Foggy and rainy English days and so on. That’s why Thanksgiving dinner was such an important feature. It was just at the beginning of that—at the downing. And then there was this flat period and then it was a combination of euphoria and panic that it was ending. That was the U. And it seemed to have a lot of validity to it as a dynamic.

29-00:43:19

Rubens: I think so. But I decided, especially in an institute that I ran that was very intense for a week, that I wasn’t going to say this. I wasn’t going to say, “Now, inevitably there will be a point where you’re disaffected.” I thought—

29-00:43:35

Smelser:

Don't say that. That's making the odyssey too stiff, too formal. I made that one point in the odyssey book at the beginning. That rational reflection is an enemy of the odyssey experience. It kills the romance and a lot of these things have to remain unconscious in order to be—so if you suddenly bring everything back up to “this is what's happening now” to the group, you see, that's self-defeating. I think we've inhibited independent discovery at that point.

29-00:44:15

Rubens:

But you yourself need to know that this is a pattern that will happen and be prepared.

29-00:44:22

Smelser:

It's helpful to anyone in a leadership position. You're absolutely right. But it is kind of demeaning to groups to tell them what's going on in their lives. And that's one reason why I was so reluctant to lecture among the adult fellows at the Center that you're going to feel weak and unhappy. We did have a psychiatrist on premises, not that he was practicing but he was available for people to talk to. I think many people did.

29-00:44:56

Rubens:

Even independent of the suicide? It was a practice?

29-00:44:58

Smelser:

Yes. He was a permanent—He was a psychoanalyst. There's a guy who was in the institute with him who was on the staff of the Center. And he was there. He said to talk about any problems that may come to mind or illness. “I'll help you out finding doctors.” It was a general availability but he also was able to—and we arranged it that he would deal with people who wanted to come talk to him but he would never tell me about it. Unless it had something to do with the welfare of the Center and so that was a good arrangement and it was an interesting feature of that group.

29-00:45:35

Rubens:

Well, of course, we'll get to in-depth about being director of the Center [Center for Advanced Study in the Behavioral Sciences]

29-00:45:37

Smelser:

Yes, we're already being ahead.

29-00:45:39

Rubens:

Yes, we were talking about your Phi Beta lecture, and it's application at the Center—that your California culture essay/talk as a way of at least modeling that there's a bright side and a dark side to life.

29-00:45:51

Smelser:

It was a successful part of my visits. Sharin came along to all of them. She was on every one of these trips except the trip to Oklahoma. She joined me in New England and we went around to the—at the University of New

Hampshire, I was to give a presentation to the Phi Beta Kappa chapter. This presentation coincided in time with an appearance of Jesse Jackson on the campus and my sense was nobody's coming to hear me. And that was more or less true. Even Sharin went to hear Jesse Jackson. She was pretty tired of this California speech by that time anyway. But I had a captive audience of Phi Beta Kappa members and their parents. It coincided with their initiation. So those people did not go off to him and so I had a reasonable audience.

29-00:46:46

Rubens:

Did you meet him?

29-00:46:47

Smelser:

No, no. He was just appearing in another building but it happened to be at the same time.

29-00:46:52

Rubens:

Is there anything to say about being in the South? Had you been in the South before, other than in Florida?

29-00:46:57

Smelser:

Well, it was very interesting. I had gone through the South. I had gone to Florida a couple of times. I'd gone to Atlanta a couple of times. I'd gone to New Orleans a couple of times. I had driven across the South when I was going on my way to college, going by car to Alabama. I never lived there for long periods of time. I got an offer from North Carolina. All these were tangential. This was a very interesting moment actually and I made a lot of discoveries. I made the mistake, for example, of using the phrase "after the war", referring to World War II. And they have only one war in mind. And, of course, being in Virginia, in particular, was very revealing in this regard because it's a lot of domination of Confederate symbols and statuary and myth and so on. And politeness. Sharin got so sick of being called ma'am by these students. Hampden-Sydney was a—

29-00:48:06

Rubens:

It was an African American school?

29-00:48:09

Smelser:

No, Hampton College was black, a traditional black university. This is just the opposite. Wealthy Southern white. And it's a brother college to Randolph-Macon. They honor each other's courses and so on. Neither of them is co-ed but they're about fifty or sixty miles apart and they sometimes take classes and so on. Kind of this formal Southern quality had hung on and it was—We were in Farmville, the town in which Hampton-Sydney was located, just after the beginning of the Gulf War, '91. There was a real surge of patriotism throughout the South. Flags at McDonald's. The South has a much closer link to military tradition than other parts of the country and patriotism really was generated by this. And this school being filled with more or less conservative white males was gung-ho for the war. And they had a little teeny protest of a couple of kids, couple of students, who stood outside holding an anti-war sign

and they were really ugly to those two for even doing that. And so this to me was a very instructive episode. And, of course, anyplace you go you begin to pick up peculiarities. Kentucky, Florida. Florida's an ambitious big time university. Not nationally tops but it's aspiring to it. And I think I discussed making it a point of going to the athletic facilities—Florida is strong in football, Kentucky in basketball—combining the talks with my athletic committee work.

29-00:50:26

Rubens:

Right. That was a big year for you. Chair of the committee and then these lectures.

29-00:50:30

Smelser:

I was very much engaged in the chairmanship, as we learned a couple of interviews ago, because of the conflicts that occurred and I had to be responsible for those. But at the same time, I managed to have these different activities going on at the same time. Probably therapeutic for me.

29-00:50:50

Rubens:

I was going to say, and probably intellectually stimulating.

29-00:50:53

Smelser:

Very much so.

29-00:50:53

Rubens:

So do you think we've done that part, the lectureships?

29-00:51:00

Smelser:

Yes, we have.

29-00:51:10

Rubens:

Well, why don't we talk your last year of teaching. I think we have ten minutes on this tape.

29-00:51:16

Smelser:

Okay, I can do that very briefly. I came back. I taught the theory course, I remember, and then I taught something minor when I was chairman because I was given further relief. I think maybe I taught one course that year. Not a major one. '92, '93, I decided I was going to go back to my university professoring and my normal teaching load was two courses because I was permanently half-time with the Institute of International Studies. Had been that for the last twenty years of my career. So I decided to arrange to go to two campuses. Irvine to teach a graduate course in theory to the newly formed sociology and anthropology department. Irvine became departmentalized after a committee I was on recommended it. Abandoning that school's group experiment that they started out with. Anyway, that's neither here nor there. But I went there to teach a theory course. Then I went to do a similar course at San Diego where I had taught before. So this was a rational thing for me to do from the standpoint of logistics because I would fly down to Irvine one morning and rent a car and I would give—devote that day to the Irvine class,

including office hours. And then in the afternoon, and here I was coached to by the students, I would get into the car before the heavy freeway traffic began and drive down to Oceanside or Carlsberg on the way to San Diego, sleep overnight, then go the next morning and teach my course in San Diego in the morning and fly back that afternoon. So this took place over two days. I'd go in the early morning and come back late the following day. Perfectly satisfactory. I decided to do it commuting rather than to uproot Sharin. Our kids were already in college away. Joe at Oberlin, Sarah at UC Santa Cruz.

But in all the cases of teaching in the other campuses of the university, I went there on a day by day basis rather than—In total I taught two courses in Santa Cruz, one at Davis, once at UCSF with Wallerstein. And then twice at San Diego and once at Irvine.

29-00:54:07

Rubens:

Had you ever done two at the same time before?

29-00:54:10

Smelser:

No.

29-00:54:13

Rubens:

And these were semester long classes?

29-00:54:17

Smelser:

Quarters. They're both on the quarter system. I taught here, they were semesters. They were quarters. Though they counted as a course, however, so I, in fact, did a little less teaching. There was nothing especially notable about those particular classes. I got involved with the students, the usual classroom—always a graduate student in these. I taught a course in comparative methods at Davis. They were always voluntarily arranged between me and a chairman or something of the sort. It was sort of mandatory that university professors should teach on other campuses but it was nothing scheduled and it was meant to be by negotiation. It was meant to be a matter of conscience. That was explained to me by President Hitch when I was appointed to that position. And not all university professors took it that seriously, teaching on other campuses, but I said I would. Some of them were arranged by prior friendships. For example, my first teaching at Santa Cruz was organized by my student, Herman Blake, who was the head of Oakes College. He had me come down and teach a course with him on black social movements.

29-00:55:45

Rubens:

He's African American.

29-00:55:46

Smelser:

Yes, he is African American. He later went to be president of Tougaloo University, a black university in the South. And then he went to the University of Indiana where he was a vice provost. I kept in touch with him. Wonderful man.

So I taught with him. There was a very interesting episode in that one. This was not long after the black power movement and Herman Blake himself co-authored a biography with Huey Newton called *Revolutionary Suicide*. When I went down a couple of years later to teach with Herman, we were having lunch. And he said, “Neil, I don’t want you to—to force anything on you.” He was very polite, very circumspect about it. And he said, “But Huey Newton would like to come to the class that you’re teaching,” particularly when we’re reading that book because it had just come out and we were having the class read it for one part of the assigned reading. But he didn’t want to force anything.

29-00:57:02

Rubens: Or make it uncomfortable, I guess?

29-00:57:04

Smelser: He was thinking of me. And he said, “Huey just wants to come in and listen in to hear what students have to say and what you have to say.” And so I said, “Of course. Why not? Let’s do it.” I didn’t feel at all uneasy about it, though I had followed Newton’s career and it was a very turbulent thing. Newton showed up one day. He brought in a bodyguard, huge man, and three women.

29-00:57:38

Rubens: And three women?

29-00:57:39

Smelser: Three chicks, you might say.

29-00:57:41

Rubens: And Huey’s a bit diminutive. He was not a big guy? He was a small guy.

29-00:57:48

Smelser: I didn’t get a definite feeling about whether he was—But the interesting thing, and I have to report this, is that his demeanor in that class was a combination of deference to me and an effort to impress me that he was intellectually savvy. In other words, that he’d read this and that. And it was artificial. You could tell that he was forcing it. That he, in the presence of this professor that Herman had no doubt built up, because I was Herman’s mentor, you see, he fell into a very—combination of passivity and exhibitionism, if you will. And I was actually kind of embarrassed.

29-00:58:36

Rubens: For him?

29-00:58:37

Smelser: Yes. I thought this is, in a certain sense, pathetic that this guy couldn’t be himself. But he was so adapting to this social situation. I think we must be near—

29-00:58:48

Rubens:

I'm cutting you off a little. I want to ask you one more thing about it. But one of the things you talk about in your book on effective committee service is about not being a phony. You say you can only do what you can do and you can't make yourself be something that you're not. And so—

29-00:59:09

Smelser:

Here was a case where I got embarrassed by someone else being that. But this was my absolutely constant advice to students going out for job interviews. I said, "Do not try to figure out this guy's position and that guy's position and try to please anybody or do anything or figure out what your exact strategies are going to be. Just do it. Just do it. You're not going to fool anybody and you may alienate if you try to be something you aren't."

29-00:59:37

Rubens:

Sure, Good. Okay. So we'll take a break.

[Begin Audio File 30]

30-00:00:05

Smelser:

The course I taught with Herman Blake was called Black Social Movements in the Twentieth Century and it began with the Back to Africa movement of Garvey and then we covered more—not exactly movements but we went through a period of—we had sections on race riots after World War I and during and after World War II. We were interested in Martin Luther King, in the civil rights movement. We were interested in several varieties of the black power movement. It had several different versions. And, of course, it was quite reasonable that we—that Newton's book would play a role since this was his account of the revolutionary activities of the Black Panthers and—

30-00:00:55

Rubens:

And he wrote it with Blake?

30-00:00:58

Smelser:

Yes. It was a turbulent co-authorship. Blake was kind of his academic conscience, if you will, or his substantive conscience and they did it together. And Herman also had better contacts with publishers than Huey did. But subsequently they had a big falling out over royalties. And Huey, of course, was killed somewhat later. But before that time, he and Herman had a fight and a big falling out. It's just sad but I guess not totally unexpected.

30-00:01:42

Rubens:

So when you came to discussing the book in the class, did you feel there was a kind of free discussion? Or because Huey was there did it limit what—

30-00:01:55

Smelser:

I didn't feel inhibited. I—

30-00:01:58

Rubens:

Well, not you but even the students.

30-00:01:59

Smelser:

The class was transformed by Huey's presence because a lot of them were minorities. Here is this big, big figure coming in and he came with this fanfare. And also he himself sort of dominated the conversation in the ways that I described. So in a way it was a totally unusual class from any of the others because we didn't have any of the guests for one thing and it was conducted in a more conventional way with—

30-00:02:31

Rubens:

How big a class about?

30-00:02:33

Smelser:

I'd say it was less than twenty.

30-00:02:36

Rubens:

So it was intense.

30-00:02:39

Smelser:

We were around one table. It was a big seminar table. So it was a rather positive experience on my part with this unusual twist when Huey showed up. I went down another time at Santa Cruz and taught a more traditional course on social movements and that was just a regular course in Stevenson College, I believe, by invitation of the sociology people.

30-00:03:06

Rubens:

You mentioned that you had been part of a committee that advised Irvine. Let's discuss that briefly.

30-00:03:40

Smelser:

Irvine was set up in 1962 on an experimental basis explicitly to break from the rigid mold of departmentalization and the dominant academic modes of departments.

30-00:04:01

Rubens:

This is slightly before Santa Cruz, isn't it, or about the same time?

30-00:04:02

Smelser:

Same time. It was the big surge right after the master plan. That included turning San Diego into a general campus, Irvine and Santa Cruz. Those were the three around 1961, '62 that swung into action.

Well, in the social sciences in particular, James March, a very eminent political scientist, psychologist—he's an interdisciplinary figure who spent most of his career at Stanford—went to be the organizer of the social science school. Didn't have departments at Irvine. And March was kind of a maverick anyway. I know him quite well. Said, "This departmental idea is an enchainment of academic life. It gets people into slots. It creates all kinds of new bases for—it isn't intellectually viable. He said, "The big thing is to get bright people to come here and let them form groups according to their

interests and these groups are going to be fluid, not rigid.” Not departments, right. It’s a very imaginative idea and—

30-00:05:30

Rubens:

Had he gone to Harvard, or been connected to the Department of Social Relations?

30-00:05:34

Smelser:

No. He was a student of Herbert Simon at Carnegie Mellon but he was a very imaginative free ranging sort of character. He had a lot of insight into the malfunctions or dysfunctions of departmental life and he wanted to do this grand experiment. And because it was a new institution he could do it. And he got the blessing of the university because they were doing—they had a school of social ecology which was interdisciplinary. And the whole school was organized on a very experimental basis, as was Santa Cruz, though on a different structural basis.

Well, anyway, they institutionalized this and March was the head of it. And for a couple of decades, this was a format for the school. Then it began to grow in terms of size of undergraduates—the student body grew rapidly. It was one of the most rapidly growing campuses. They began to experience demand on the part of students for majors. Economics major, political science major, psychology. This couldn’t happen under the group thing because people were all interdisciplinary and so on. But the administration began to feel this pressure and the groups began drifting toward more conventional lines. In other words, there was a group on public policy which was basically political science. There was a group on economics and there was a group on cognitive psychology, which was not the whole of psychology but nonetheless made up more or less exclusively of—so there was a drift going on. But they weren’t called departments yet. There was an anthropology group, there was a sociology group. And then there were linguistic groups and interdisciplinary groups and so on. So you had an administrative pressure, or student based pressure, really.

You also had—a bit of a pathology developed over the issue of tenure because these groups were given the assignment of recommending their own members up for advancement and tenure. In that sense, they were little departments. But they had to have some mechanism so they used the groups. Well, on a few occasions, people who wouldn’t get tenure in one group, they’d go to another group hoping they might—it was kind of a strange thing and kind of undisciplined sort of arrangement in the new circumstances.

Then after March left, he went to Stanford after maybe five years, but left the experiment behind him. The new dean, the dean in the late eighties, because that is when it was, was getting more and more frustrating about the administrative difficulties involved in these schools and the school of social science in particular was a headache. So the new dean convened the

committee, a national—very eminent national committee. And Henry Rosovsky was the head of it from Harvard. He was just ex-dean at that time, I think. Robert Adams, who was a very big figure in the National Academy of Sciences that I'd worked with on these NRC reports, was on it. It had me. It had Kenneth Arrow, I think, from Stanford. A very eminent economist and Nobel Prizewinner. And I was the sociology person. It was about a five or six person committee and we met with the dean on our own with leaders in the school, with individual faculty members.

We met several times. And I've always been a kind of skeptic about the academic department based on disciplinary lines because I'm very much aware of the complications, the striving, the rigidities in that departmental form. In fact, I talked about that just yesterday to our foreign academic leaders who are gathered here for a conference at the Center for the Study of Higher Education. So I was very ambivalent about just simply saying let's departmentalize. Let's recognize de facto what's happening and what's needed. I had a heavy heart when we wrote this report saying, "Let's departmentalize this school along conventional lines." So they did. They just simply converted the policy school into a political science department. Psychology emerged out of the others. Anthropology and sociology became a joint department out of that reform. And this committee sort of had its way. They didn't know what to do. I say reluctantly, or I reluctantly thought, "Well, this is just going to go down that route," because that was happening in San Diego. Santa Cruz was becoming more departmentalized and the colleges were weakening as a basis of organization. I said, "Do we give into this total lockstep kind of organization?" So I was very ambivalent but actually helped draft the report that came out for them.

They built a new social science building about the same time and they put our committee member's names in the cornerstone of the building.

30-00:11:18
Rubens:

And Irvine has become quite a leading light in the system.

30-00:11:25
Smelser:

Of the academic fortunes of those three new ones, San Diego, Irvine and Santa Cruz, from the standpoint of advancing toward academic excellence in the national competitive race for university recognition, San Diego was clearly the winner. It was a miracle story. Rapidly passed.

30-00:11:51
Rubens:

Right. Had a med school associated with it, too, didn't it?

30-00:11:54
Smelser:

They had a medical school. Irvine also had a medical school. Still does for that region. And Santa Cruz never did. And Santa Cruz, I wouldn't call it a laggard, but it was less—it's less well-known and regarded as less eminent

nationally than the other two. Irvine is in the middle. Irvine has had a very aggressive building in the past fifteen years and it's up there.

30-00:12:27

Rubens: In terms of theory I thought that it was pretty important. Mass media as well.

30-00:12:30

Smelser: Very strong. And I know the social sciences have improved. They've improved radically, including in my own field.

30-00:12:47

Rubens: So perhaps we're ready to talk about you being asked to join UC's Office of the President.

30-00:12:54

Smelser: Yes. I'll talk about that. I guess I should first talk about Jack Peltason, who became president of UC. At the University of Illinois he was head of American Council on Education. He was a very leading national administrator and he was chancellor at Irvine for many, many years. And I got to know him first at a scholarly meeting on higher education in Rome, to which I was invited in one of those periods when I was overseas. I can't even exactly the date it. It might have been when I was director of the Education Abroad Program. But I met Peltason for the first time and we hit it off at this meeting, especially at the informal dinners during and after the conference. We became figures in each other's mind the way that happens on the basis of even a fairly brief interaction. You sort of feel you click with somebody. And I had that feeling about Peltason.

30-00:13:59

Rubens: How old was he about compared to you?

30-00:14:00

Smelser: He was a tiny bit older. He's still alive but he's ailing now. But we were in the same rough intellectual cohort—went very different routes. He was a political scientist and wrote texts and some research but he got into the administrative line very quickly at Illinois and continued to stay in it. I saw him a good deal down at Irvine on that committee and when I was on the board of regents he and I continued our—so there was a mutual respect. He teased me consistently. Every time we would re-meet in a room or at a conference or at a regents meeting or something, he said, "Well, here comes Mr. Report," meaning—he was referring to the lower division report on education and the one on athletics. "So he's Mr. Report. This guy writes all the reports for the entire university." He's just going on and on like that. I got a little tired of it but it was all in good humor. It wasn't any—and sort of basically kind of flattering.

But one day, totally out of the blue, when I was teaching at the Southern campuses, he called me up. I guess it must have been spring of 1993 because it was a few months lead time. And he said to me—he had been president for

only one year. He had nosed out Atkinson for the presidency. But he was senior by that time. He was old enough that it was generally anticipated that he was not going to remain in the presidency very long. That was my general understanding of the aura of his appointment and I think he himself didn't want to have a long term as president. But he had been in one year. He called me up and asked me in a very general way, "Would you come and be my advisor on long-term change and development?" without much specification of anything else. It wasn't a surprise to me that he would have thought of me because of our past but I knew that he had a respect. We had mutual respect and so I didn't see it as a completely out of the blue or unanticipated invitation, though I certainly didn't anticipate. I didn't initiate anything and the call did come out of the blue.

So I went over, chatted and agreed to do it for one year. He never really got very specific about the portfolio. What would I be doing? What statewide, systemwide groups would I be involved with? What would my role be? He did insist that I would work very closely with Walter Massey, who was his number two person in the administration. Massey had been—he's black. He had had a very distinguished career at Nuclear Labs in Chicago and then as head of the National Science Foundation and came out here to join the highest ranks of the systemwide administration, even though as a university administrator he was relatively green because he'd been in other roles. A talented man, obviously accomplished, and then made his way around Washington very successfully. And I don't know the circumstances of why he was tempted to become the vice president, but he was there at the time. And because he was the vice president and provost and I was an academic primarily, he was going to be the person that I would work with on a day by day basis, although I would see Peltson on an as needed or as wanted basis. But they never defined me other than a floating person, which is very rare for a systemwide office. They're normally pretty organized and siloed and it's one of the problems of a systemwide organization, it becomes encumbered with sub-organizations within it that have their own turf and their own interests. That's what happens.

So I thought about this. I guess I wasn't too conflicted about it. I thought it would be a very interesting—I indicated to you when I came back from Russell Sage, that here I was sixty years old, sort of looking around. And this seemed a really interesting way to spend a year. To go over there to UCOP. I gave up my teaching entirely. My budget moved over to systemwide. I said, "Okay, I'll come. I'd like to bring my long-term secretary, Chris with me."

30-00:19:37

Rubens:

So you were given an office and she was too?

30-00:19:39

Smelser:

She was given an office and I was given a beautiful office. It was in the Kaiser building at that time. It hadn't moved over to 11th Street. I had a magnificent

view of Lake Merritt and Chris was there. I continued a lot of my independent work that were ongoing projects of different sorts. They did need me to do some traveling. I think I went once or twice to Europe when I was over there. But nonetheless, it was defined as full-time.

But I would have these assignments, or sometimes I would even initiate what I wanted to do. There was an early event that I listed there in the notes of a conference that Walter Massey wanted me to organize on long-term planning. This was within a month after I arrived I think that he initiated this project. And it was a system wide project and it involved leading faculty and the administrators from all the different campuses, including a few chancellors, who came to it. It was held down here in the Doubletree Hotel and I was the organizer of it. I wrote the agenda, I wrote the outcomes and I chaired most of the meetings.

30-00:21:08

Rubens:

Did you invite the people who came?

30-00:21:10

Smelser:

No, the systemwide office issued them all but I was the—

30-00:21:14

Rubens:

Well, I didn't mean literally but did you identify who—

30-00:21:16

Smelser:

Some, some. It was negotiated. I didn't know enough about the whole system to be able to dictate the entire membership. And some of the members were more or less mandatorily dictated. If a chancellor wanted to come, he certainly came. But there were also spokesmen from the senate and outstanding faculty members. People who had taken leadership roles on the different campuses came. And it was a big free for all and I actually also wrote that up, and it became a kind of a university document that has since no doubt been forgotten but nonetheless it was an interesting piece. And Walter Massey was so appreciative. He thought the conference was a complete success, gave me a big bottle of champagne after having organized it. So it was a kind of a baptism for me because it occurred early in my stay.

30-00:22:09

Rubens:

Where was the university financially at this point?

30-00:22:13

Smelser:

You're asking about the background for my appointment, the question why would Peltason need somebody in this area? Well, this was 1993. It was right in the onset and plummeting of the early nineties recession. It was the only time that the university faculty took a cut. They restored it the next year. It was a five percent cut and there were tuition increases. The budget was badly slashed. It was a prelude to 2009-10, up to the present, and not as deep as that. And then the economy rebounded about 1995-96 and you had a series of absolutely plush years in the last half of the nineties. But this was in the tough

times, right. And furthermore, they were laboring under long-term issues of how they were going to continue to make the master plan viable because they saw this flood of students coming in. So a tidal wave II—tidal wave is what Clark Kerr called it—fed by increasing population, fed by immigrant groups, fed by the fact that we were not expanding the number of campuses except for the Merced campus. David Gardner had asked for three new campuses a few years earlier in the late 1980s when he was there. They'd barely financed the Merced campus and so the system hadn't grown but the student numbers had been continuously going up and up and up. So the question of could the university still live up to the master plan was a huge issue in terms of—and continue the diversification because affirmative action was still in full swing in the early nineties. Didn't get the serious constitutional and voter challenge until 1997-98. So it was high on the agenda.

And planning was a problem and I undertook a number of projects on my own and partly with Massey and Massey's encouragement to explore various long-term issues. For example, there was this document that some personnel in the systemwide worked up called the Thirteen Commandments, meaning thirteen ways to save money and make the operation more efficient. And it included some older ideas and some newer ideas and among the older ideas was a return to year round instruction and making the system all semesters. All kinds of ideas that have since also arisen in the current times but they were ways of making the university more effective in its operations. Revising the whole notion of summer school. And so I wrote a working document on this particular set of issues, a critique—critical.

30-00:25:33

Rubens:

Those set of issues meaning the thirteen or—

30-00:25:36

Smelser:

The thirteen, some of them, selectively. I wrote one on year round operations in particular and I did some analysis of past attempts to do that in the university as well as efforts in other institutions. I came up with a lot of skeptical critique of the idea of exactly how effective and workable this would be. So these were working papers that circulated around and I was an independent agent circulating around with them. We'd have little conferences on these papers. That was kind of my role. I wrote a major one on the issue of the master plan and its threats and that was for a special regents meeting of the topic. And that was an analysis of the really dangerous long-term threats that would threaten not only growth and viability under the master plan but also diversification, problems about continuing diversification through affirmative action and diversification of the student body and so on. And it became the kind of working document for a regents meeting that spring.

30-00:26:51

Rubens:

Okay. So we're not talking about the spring of 1994?

30-00:26:57

Smelser:

Yes. And it represented my best thinking and analysis about the trends and about the perils to anything like the master plan. It served a little bit of raw material for the later document I wrote on governance but it I was only background for it.

30-00:27:16

Rubens:

Are you referring to the essay in your book *Reflections on the University* –the “Governing of the University of California”?

30-00:27:26

Smelser:

Yes. It’s a tiny part of that. There’s a last section in there on the future and future challenges. Some of the ideas in there were developed as part of the working document I wrote for the regents meeting, but it’s not just reproduced. It’s just incorporated it.

30-00:27:43

Rubens:

This was an elaboration.

30-00:27:43

Smelser:

Yes. This was a separate document entirely. And some of the work I’d done for systemwide got into that document and the experience that I’d picked up and the perceptions that I had developed all got into that document. There was no single source or no single other activity that fed into it. It was a synthetic piece of work. And I’ll come to that document in a moment.

And then I would go to meetings of deans of research, by centers for research, deans of the graduate divisions and so on. They all had systemwide organizations. They would come to meet over at the system wide headquarters and I would sit in on these meetings, talk, contribute in an undefined way, in keeping with this free floating thing. I didn’t have a special role. Massey would ask me to go to them or come to them if he was chairing them and be part of the dialogue and have input. But that was consistent with this at large—which is a beautiful role. I can’t tell you how nice it is to be in that rather than a specialized targeted assignment. They asked me to continue a second year and maybe more and then when I left they hired, in the same role, with the same idea, continuity, same budget, Karl Pister. But they gave him an assignment. They said, “We’d like you to head up the efforts of outreach for the University of California.” So he was much more constrained than I was and maybe they would have asked me to fill a slot, which I wouldn’t have liked nearly as much as being this free ranging advisor, ambassador at large sort of. It’s a very unusual role and I liked it. And it was in keeping with my past administrative roles. Being in the position of some power and influence and involvement but not having papers to push and not having a title like dean or vice chancellor or provost. Except for the chairmanship, I didn’t have those titles. I liked it that I didn’t have titles. I had this, as I say, influence without responsibility, which, as I always say, is an extremely good mix. And though I

was responsible, there was no question that I was responsible in that position that I engaged, but nonetheless it had this ranging quality that I liked a lot.

30-00:30:48

Rubens: Pister was concerned with outreach. Did that mean affirmative action and—

30-00:30:53

Smelser: Well, it means, technically, it's going round to the high schools and community colleges, getting students who are minorities to encourage them to get interested in UC. In a way, it turned out to be the major alternative to affirmative action and that shut down by Ward Connerly and Proposition 209.

30-00:31:07

Rubens: Was Connerly a presence when you were at UCOP?

30-00:31:17

Smelser: No, he wasn't active. Connerly became active about two years after I left and I did not get into that. I saw Connerly. But he wasn't on the war path until a bit later.

30-00:31:42

Rubens: Now I interrupted you.

30-00:31:46

Smelser: Oh, well, yes, there is one other thing I would mention. Peltason had a big hiccup that year—a kind of scandal. And it was in the middle of these heavy budget hearings in which the university was not faring very well and he was going to Sacramento. I didn't go up there to Sacramento that year but he was up there and he was negotiating. He was fighting and he was frustrated and he made some comment that the press picked up that seemed to be anti-Asian about a legislator or somebody who was arguing for things that he didn't want. And so Peltason in an aside that was kind of picked up by the press—he didn't say it for public consumption but it got noted. And then a big stink in the daily press which would follow such things because what sells newspapers is newsworthy and so on.

30-00:32:44

Rubens: Yeah. And the university is such a target.

30-00:32:48

Smelser: And created a kind of a crisis in Peltason's office. What to do, how to answer, what to say? How to position the university? How to control damage? I remember he and Massey and a bunch of the other people, PR people, they were meeting all the time. And I just decided on my own, because I was very comfortable in this crisis role, I decided to go up to Peltason's office and enter myself into these discussions and was, I think, welcomed. I was welcomed. It was my old familiar role, often to be consulted at crisis time. It wasn't as big a crisis as some of the big ones on the Berkeley campus. It created a sweat in the president's office and we really had to have some major strategy meetings as to exactly how this could be defused. So that was yet another little side

that—and I was around for some of the Council of Chancellors meetings, when all the chancellors meet with the president. I would sit in on some of those discussions and do my bit with Chuck Young and all the other—Tien and all the other chancellors who were around at the time, as well.

There was a coincidence at the time that I was being invited to become director of the Center and their invitation for me to come for a second year.

30-00:34:33

Rubens:

It would have been your second year at UCOP.

30-00:34:35

Smelser:

Plus the announcement of VERIP 3 [Voluntary Early Retirement Incentive Program], which was the third installment of that very generous retirement scheme for senior faculty, that they would—to encourage expensive senior faculty to retire earlier than they might have.

30-00:34:53

Rubens:

Was that called the golden handshake?

30-00:34:55

Smelser:

It was. That was the nickname for it. Golden handshake. It had VERIP 1 three years before. They'd had VERIP 2 and this was VERIP 3, which was by far the most generous in '94. Spring of '94 was when it would take effect. That was just at the end of that year at the president's office. It was generous in the fact that they—if you decided to retire, if you were over fifty-nine and you decided to retire, they would pretend you'd been on the faculty eight years more than you had been from the standpoint of accumulation of benefits. That put me over forty years on the faculty, which was retirement at full-time salary because of the accumulated logic of that. They also gave you a cash payment to retire.

I remember the amount exactly because it paid precisely for a new Volvo that we bought in Sweden the following summer. But it was just a payment to get you out. So here I was. I was being invited to stay on in this capacity and it probably would have been more than one year additional because this is the way things happen. You get in a role and you—something they would want me to stay.

30-00:36:31

Rubens:

Plus you had been useful.

30-00:36:33

Smelser:

I'd been useful to them. Whether Atkinson would have wanted me to stay or not I don't know because he came in after Peltason. Peltason stayed one year after I left, then came in Dick Atkinson. And whether Atkinson would have wanted me to, I don't know. He probably would. I was a good friend of Atkinson as well. So I had these career alternatives here at age sixty-four.

VERIP and the Center were tied together. The only thing that VERIP forbade me to do was to teach again in the University of California. They didn't care what I did. I could do anything else. As a matter of fact, the university was criticized for saying this is a good way to get rid of good faculty because these people will leave and take another job. Just like I did. When the VERIP 3 was announced they said fifty-eight is the year you could retire. Tien was infuriated, saying, "I'm just going to lose my good faculty." They're going to go to other places. They'll take VERIP and run. And he tried to resign. And a group of us, including Bud Cheit and Balderston and the Kerr boys, as I call them, went and tried to argue Tien out of resigning because we didn't think it was an issue over which he should resign. And it is—

30-00:38:05
Rubens:

And that age fifty-eight was not moved up?

30-00:38:08
Smelser:

It was, to fifty-nine. So it was still—and a lot of people left. And I was telephoned once I took the Center job, telephoned by the *Chronicle of Higher Education* and the *New York Times*, saying, "Is it true that the University of California is going to lose good people like you just because—who have a lot of time left in their active career?" because we'd just been uncapped. I could have stayed around indefinitely on the university faculty. And so it was a—

30-00:38:38
Rubens:

What'd you answer? That was a delicate question for you.

30-00:38:42
Smelser:

I answered it with humor. I said, "Everybody owes it to himself to have been born during the Depression," meaning that these opportunities are coming and going. I didn't answer this guy, is the university going to be—I evaded that question. I was one of them. I didn't say anything that harmed the university in this regard. He was wanting me to. That's often—reporters are asking you, trying to get you to say things. I was diplomatic in this and kind of turned it into almost a personal issue rather than an institutional issue. But these were the things that I was facing at that time. I wasn't ready to retire.

I was sixty-four years old. I'm still active and vital and still engaged in the university. I certainly didn't want to simply retire. The timing of the invitation to the Center was completely fortuitous. It coincided exactly when—at the time when you had to decide on VERIP 3. Either took it or you didn't take it. And you couldn't say, "I'll take it next year." You had to do it then. It was absolutely then and it was exactly then when the offer from the Center came, complete an accident because Philip Converse, my predecessor, decided to step down and they were looking for a new director.

So I knew the Center job was a wonderful one already because I'd been so long involved in the Center. I'd never been a fellow but I'd been involved in the administration of the Center for years and years. And there was also a

tremendous financial advantage because I retired at full-time salary and took another job that was higher than my full-time salary and got both for seven years and joined a new retirement system at the same time. So it was an unbelievable windfall. There was also the one-time cash gift, with which I bought the Volvo—my graduation present from the university.

30-00:41:05

Rubens:

Oh, that had to do with forty years of service?

30-00:41:08

Smelser:

That had to do with part of the scheme of the retirement. They paid you as well as gave you additional years. See, I was officially something like thirty-six years. And they gave me eight and that put me over the top, so I retired at full salary, including calculating my enhanced salary at the president's office as one of the three years they calculated payments on. So everything fell in my lap. So financially, the—although I didn't really calculate it that closely. It was obviously a very big part of that transition and so I, after thinking about it, after going to all these orientations on VERIP and so on, I simply went in and told Peltason a few days after they'd offered me to come back a second year to the president's office that I couldn't do it. That I was going to go take the Center job and then take VERIP. That's the way it worked out. And I said in my own mind, "Well, I'm going to go to the Center." I made up a figure in my own mind how long I would stay. I said, "I'll go stay there seven years." It's higher than average but it's not going to lead me into old age. So this was in my own mind. I didn't sign a contract. That position was at the pleasure of the trustees. So I decided, okay, I had enough confidence in myself that I was not going to be fired from that position even though it's a—you serve at the pleasure of the trustees. You don't have tenure in that position at the Center. I decided to take the Center job. It was all amicable. Jack understood it. He'd been on the board of trustees of the Center himself. I knew him in that capacity, as well as a board member with me for at least three or four years, I think, and so it was all amicable and all very good.

I had learned a tremendous amount. Here I was in a part of the university that I'd had some relationship before when I was head of the senate and when I was on the regents. I got a feeling for systemwide, right, but I'd never been in the center of it. That was all a little bit more peripheral from my view. But when I got into it, I really got—it's like you get to understand things, see how things work, see how the staff members work with each other. You see their interests and how they're jockeying and you get the whole idea. And I was always advising Peltason and Massey on issues having to do with policy decisions and governance and programs and crises and whatever. I decided, for reasons that I don't know, to write something up about my experience and my perceptions about governance issues and governance strategies in the University of California somewhat from the systemwide point of view but not exclusively. That essay that I wrote—I sat down one day and it was like my book on the committees. I wrote it in three days.

30-00:44:41

Rubens: Just came out.

30-00:44:43

Smelser: Just all flowed completely out and I didn't—and it came out in more or less final form. I just had it in me and it all flew out. And it didn't require too much scholarship. It involved condensation of my knowledge and experience. I used past knowledge from the senate. I used some knowledge of the organization of the individual campuses and simply let it flow. It was for Massey. I presented it to Massey and Peltason looked at it. But it wasn't meant to be published or meant to be a policy document for them. It was a kind of—as I described it in the book in which it was finally published, it was a gift on my part. Just to—

30-00:45:38

Rubens: Kind of a catharsis, too?

30-00:45:39

Smelser: —share and had a catharsis element. It had a finalizing element because I knew I was going to be leaving. It was at the very end that I wrote this. But I didn't have any consequences in mind, that it should be used for this purpose or that purpose or as a bible or a guidebook or whatever. I just did it. It was one of these actions that I can't recall, explain, that I—why I did it but I found it enjoyable to write because here was this flood of ideas and issues about which I had thought which I had never put in final articulated form and it was, for me, good to do that and it marked a kind of very interesting transition document for me. I was in that world. I was going to leave that world and here's my best reflection.

Two years later, somebody had given it to Atkinson when he became president. And when I was down at the Center, been there a couple of years, Atkinson wrote me a note, handwritten note. He said, "Publish it," because he had been given it I guess by Peltason or by some staff member. You just don't know how these things circulate around. But I wasn't in any position to do any publishing of that sort. At that time I was into other things, I was writing other things, I was administering the Center, I was beginning my enormous work on the encyclopedia. So I just let it sit and then only in 2008 when I was doing *Reflections*, did I think about publishing. It was just a document sitting over in the president's office. In fact, I had to get a copy of it when I finally decided to publish it. But it's one of those things that doesn't always happen. It was a little bit like that committee book. It just flowed out and it became the kind of core chapter, policy chapter in that book. It was the longest chapter.

30-00:47:58

Rubens: It's put in the middle of the book, too. I always wondered kind of how you organized the book.

30-00:48:09

Smelser:

I was going to do it on a chronological basis. The University of California Press told me, “No, you’ve got to put that adventure story about 1965 at the beginning. That’s the sexiest part of the book, first of all, and also it came early in your own experience.” I wrote it later. I wrote it only in 2008 but I had experienced it in ’65, which was the earliest part. The whole thing isn’t chronologically ordered by topic.

30-00:48:41

Rubens:

Conflict and adaptation; diversity, affirmative action in the culture wars. Governance and coordination.

30-00:48:49

Smelser:

That’s how those essays were pulled together.

30-00:48:51

Rubens:

I thought the end one, “Marrying Analysis and Action”, I love that title.

30-00:48:56

Smelser:

Yes, well, that’s what it was. There were three major reports and that spoof report on Thanksgiving.

30-00:49:05

Rubens:

But the ’93 article, the one on California’s multi-segment system, you’re saying the beginning was commissioned by Arthur Levine?

30-00:49:15

Smelser:

Well, this was a book dedicated to Clark Kerr on contemporary trends, on the contemporary situation of higher education. And Levine approached me and said, “Won’t you write a book on California as a system?” It was completely independent of my experience in the president’s office. It was—

30-00:49:41

Rubens:

You had written the book on California higher education years earlier.

30-00:49:44

Smelser:

I had written the book on higher education and I had written a couple of essays on the Berkeley campus and conflict. And I was in a somewhat fortuitous place to write that article on the multi-segment system because in the two years preceding, the state legislature undertook a review of the master plan. Because I was still in the senate, I went out there all the time to listen to the proceedings, occasionally to testify. So I got involved in that review of the master plan quite independently of a lot of other things I was doing. And it was good background material for my writing.

I was also called to testify before the OECD [Organization for Economic Cooperation and Development] study group that came to study the University of California as part of its comparative study of institutions of higher education and recommendations for European universities. And I had had some relationship with that Paris based organization. It’s a kind of an

intellectual and economic arm of the NATO countries, developed countries and so on. I had a couple of projects there. So I was positioned to be perceiving a lot of the problems of campuses and systemwide organization and the issues and then the reports of both the OECD committee and the state review of the master plan came out just before I wrote this up, so it—they provided the raw material for that essay, which Levine accepted and it appeared in that book. The book was written in honor of Kerr. And Levine, of course, is one of the leaders in higher education scholarship in the country. So it was commissioned.

30-00:52:02

Rubens:

And so is it your phrase? Do you come up with the—I don't know how you say it? QUEEF?

30-00:52:06

Smelser:

QUEEF, it's an acronym. What happened is the California review came up with points and they named them all. Efficiency, freedom, unity, equity and quality. So I just said this is QUEEF. And so it became the QUEEF complex and I criticized it in this essay saying, "You can't maximize all those things at once." In fact, some of them are contradictory with another. And so it was a takeoff point for my analysis of the kind of built-in internal contradictions that this system was living by. But I was the one who invented the QUEEF acronym.

30-00:53:04

Rubens:

You mentioned paraphrasing a Gilbert and Sullivan lyric in the Gondolier? to your committees but it wasn't. This appears in this essay. You're talking about the ambition to do everything you said. It was a situation, to paraphrase Gondolier Sullivan's dreaded egalitarian utopia in the Gondoliers.

30-00:53:32

Smelser:

The song is if everybody is somebody then nobody's anybody. That's how the Gondoliers is. I recently made up an acronym about the somewhat frenzied literature on the directions of higher education that I'm going to probably incorporate into my Kerr lectures. This acronym is to characterize the apprehensions and dread of commercialization of universities that many academics write about. This acronym is GAMMA, the Greek letter, and it refers to globalization, academic capitalism, managerialism, marketization and accountability all business. All the people who are holding their heads and screaming about the corporatization of the university, which is a real problem and I'm going to address it as carefully as I can. But this is the way I organize it in my own mind. I invented this GAMMA. GAMMA as a way of capsulating the themes that are now dominating the literature.

30-00:54:47

Rubens:

That's great. I love it.

30-00:54:49

Smelser:

Well, you've got to give a little lightness and play a few times, I guess, in this world.

30-00:55:07

Rubens:

And what is academic capitalism?

30-00:55:10

Smelser:

Universities get into forming spin-off corporations. Biological research mostly and they collaborate with businesses in setting up joint enterprises. That's academic capitalism. They compete for students in the market. Things of that sort. Academic capitalism is a covering word for many sins. Then marketization. Managerialism refers to the death of the great moral leaders among presidents and leaders and now you get the people, all they do is manage, like businessmen, and manage conflicts and so on. And then accountability is the great movement that began in the eighties and nineties to try to make universities more efficient by making them report to state agencies on graduation rates, contact hours, the usual. A lot of indices of efficiency and so on. The movement is kind of a disaster because you can't really measure these things very well. But it's dominated state legislators and to some degree state executive offices as a movement to make universities more accountable, more efficient, perhaps more like businesses in their operation. Cost cutting is part of accountability, but mainly input/output analysis and the study of students and their graduation rates, their placement rates, the numbers who go onto graduate school and so on. All these quantitative measures about performance that don't—often don't give you a—they scarcely give you anything like a full picture of what's going on in the university.

And, of course, they're the bane of an administrator's existence and they are also—one thing I observed in the systemwide administration. When all these calls for reports on teaching load, on trends, on contact hours, on budgeting procedures and so on, these accounting measures, the university, among other things meets these requirements but it tends to get very adaptive and often very cosmetic and loading the statistics in different directions in a way to give a more favorable picture, which is totally understandable behavior on the part of an institution that's under surveillance. They report and then the people in Sacramento get these reports, they look at them, they file them. So that you go through the motions. This accountability motion makes for a lot of inefficiencies.

Ironically, you force the university to do a lot of new things for which they have to hire new staff and build new machinery to get these reports in as to how efficient we're being. So it's a real joke in a way and then, of course, in extreme conditions—I've reviewed the literature on this—these accountabilities promote actual deviance and lying and creaming and misrepresentation on the part of agencies. Like, for example, local homeland security agencies were once reporting all kinds of events as terrorist activities

just to show how active they were in being anti-terrorist. So you get all kinds of bureaucratic manipulations there.

30-00:58:46

Rubens:

So we'll be getting to that because we'll be dealing with your work on terrorism.

30-00:58:51

Smelser:

To some degree it will come up. It's become a pet peeve in my own life and I wrote a whole section of this brand new book on usable social science on the whole problem of accountability of different kinds of organizations. So I got pretty much into the literature of that.

30-00:59:10

Rubens:

Well, we are coming to the end of this tape—

30-00:59:13

Smelser:

So we'll end for today.

Interview #16 August 18, 2011

[Begin Audio File 31]

31-00:00:00

McIntosh: All right. So we're here on Thursday, August 18th, tape thirty-one, with Neil Smelser, resuming our interview after a break. . And Neil, at the end of the last interview, we were just getting to your retirement at Berkeley and that period in your career. So I was wondering if there was anything else that you wanted to say on that.

31-00:00:27

Smelser: Yes. I'd like to talk about the occasion of my retirement and why I did at that time.

31-00:00:33

McIntosh: Please.

31-00:00:34

Smelser: I was over at systemwide '93, '94, as it turned out, my last year of service, active service in the University of California. And we've discussed that, my role with Peltason and Massey and different aspects of that. And I think I also said that two-thirds of the way through the year, the search for a new director of the Center had started and I was—I'll get into that when we start to talk about my joining the Center. But I was given an invitation to join the Center as its director in the fall of 1994. Jack Peltason, in the meantime, had asked me to take on a second year as his special advisor at the systemwide administration, which I would have taken if I had not—or I would have come back to Berkeley to finish out my career. I wasn't ready to retire but here comes this offer to the Center, so it was the occasion of the retirement. Well, my own little idea was, "Well, I'll just sneak out of Berkeley and go down to Stanford and continue my career down there."

But both the campus and the systemwide administration decided to give me a very flattering goodbye parties on my retirement. Chancellor Tien organized the campus one for me. It was partly through the department but partly through other things that I'd been in and he invited me to—it was in Alumni House. A very big crowd of people showed up and there was a big ceremony. Tien was there the whole time. He spoke, he introduced me. He awarded me the Berkeley Citation. For the record, the Berkeley Citation is Berkeley's invention of its own honorary degree after honorary degrees by the university were discontinued because they'd become so politicized and people were threatening to blow up buildings if someone didn't get an honorary degree. And so they just sacked—and there were many demonstrations, sometimes against the people receiving honorary degrees. So the regents decided to hell with it, we just won't give honorary degrees on the system wide level and they solved the problem that way. Berkeley then invented the Berkeley Citation as

the way you give honorary degrees, and also to selected citizens on their own campus.

31-00:03:00

Rubens: Very selective.

31-00:03:01

Smelser: I don't know how many were given but it was a definite honor and Tien presented it to me at that meeting and I was, of course, very flattered. We had a gathering of people. It was a most impressive gathering of people who came to speak for me. I was most touched because my older brother Bill was one of them. He was teaching in the School of Social Welfare and he came and gave a very touching kind of biographical going back to our childhood talk and his impressions then, many of which I hadn't heard before.

31-00:03:41

McIntosh: Was any of the content of Bill's speech new to you or revelatory?

31-00:03:44

Smelser: It was a surprise. Some elements were a surprise because Bill said during the course of his remarks, "We were always impressed with how smart Neil was." Just blew me away because the family culture was that all were, right, and there was none of this kind of comparative. And he was comparing me to himself and to my younger brother. Because they both had successful careers but mine was more spectacular, if you will, and Bill acknowledged that, just matter of fact. So that's an example of this. Anyway, I was very touched. Students, several students of mine, spoke. One in particular, a Yugoslav named Velco Velacic got up and reported during the course of his graduate career that I had invited him and several others over to Christmas dinner at my house. The whole audience gasped, I was the only faculty member at Berkeley who would do that. It was kind of very touching. The mayor of Berkeley came because one of my students was working in the mayor's office and told him about it. The mayor showed up. He declared it Neil Smelser day in Berkeley. I asked him why they didn't shut the banks. Sort of an idle teasing. But anyway, it was just a beautiful, good-feeling sort of occasion for me.

31-00:05:19

McIntosh: So this seems like a major celebration.

31-00:05:22

Smelser: It was, in its own way. I have to say I had this kind of continuous streak of modesty about myself. It isn't self-degradation but it's not really thinking of myself—secretly and deeply I think of myself as a very special person but there's a level which I really don't and really felt that this was either excessive or a happy surprise and so on. And so, in a way, that makes it happier when you think, "Well, they've gone out of their way. I really deserved my career at Berkeley and so on and so forth and now I'm being recognized in this very special way."

31-00:06:04
McIntosh:

Well, it sounds like humility might be the right word for—

31-00:06:06
Smelser:

Well, that's one streak. But humility always overcomes probably a deeper narcissism sort of. My analytic background tells me that. And it's absolutely true. Anyway, Tien was very, very flattering and it reflected a lot—and he did it for me because we had developed an independent relationship during his chancellorship. The biggest thing that made that relationship was my service on the athletic committee early in his tenure. It was a report that was certainly consonant with his own, “Go, Bears,” kind of mentality. He liked it. I was invited to the Citrus Bowl along with the cheering alums and so on that year by Tien, to go on the chartered plane that took everybody to the bowl game that Cal went to that year. And then he called on me on an informal basis, though I can't say I developed a very close personal relationship with him. Almost nobody did. I suppose there's something cultural to it in terms of his own background. And he was quite instrumental and some people have reported fairly authoritarian in his chancellorial style. But nonetheless, we had this good relation and he wanted to honor me in retirement. So the honoring came partly organized by the department and several of my colleagues in the department spoke about me, as well. And then others in other places that I'd had contact over the years spoke. It was just a very nice occasion.

Two weeks later, Jack Peltason threw a goodbye party for me at University House and invited—a lot of the same people came but there was a whole contingent—I developed quite the network in the systemwide administration by being this floating sort of at-large advisor. And so he asked me to name a couple of dozen people in systemwide that should be there and I had no trouble doing it. So it was, once again, a kind of mixed party. Two of my children came, my younger son and my older daughter. And my older daughter, one of the nice touches of that party, was that my older daughter Tina read a poem and it was about me and about her feelings about me when she was—when I was separated from my first wife they were very young, three and up, so I did spend years and years of seeing them twice a week. I cared for them very much and Sharin did, too. She read this very touching poem everybody was really moved by. One of the humorous aspect of the poem was she—in the poem she said that we always listened to country western music as we were driving back and forth across the bridge, driving down to Arizona, driving, taking these camping trips and so on that I took them on. And when she mentioned country and western music—once again you got the—gasp you know.

31-00:09:47
Rubens:

I can imagine.

31-00:09:50

Smelser:

Oh, that's right. And after the official ceremonies were over, five or six people came up to me confessing their own interest in country and western music. Formed a little club on the spot there.

31-00:10:04

McIntosh:

Well, that touches on a bigger issue with these goodbye parties, which is that sometimes they will focus solely on the career accomplishments to the lack of focusing on the person himself or herself. And so do you feel like you as an entire person was being celebrated?

31-00:10:30

Smelser:

Yes, yes. Very definitely. It wasn't this sort of, say, he was elected to this and he was elected to that and he got recognized for this. There was some of that, of course, but nonetheless, there was a kind of a human aspect of it. And in my own response at the end—you're sort of called upon to say something at the end, I, because of this kind of quality of the party, I didn't plan a valedictory set of remarks. They kind of developed during the course of the party in my own mind. And what I described was the course of my development of love and affection for the university. That was the whole theme of it. And talked about the importance of the early critical years that I served in the chancellor's office that spread my wings further and my affection for the institution as a whole and even came to love a lot of these noisy alumni and so on and so forth. It was that kind of a—and Pelteson picked that up in his last remarks, as well, about what it takes to love an institution. So that was the dominant theme of that second party. So it was a personal thing really.

31-00:11:37

McIntosh:

The theme being a growing attachment to the institution.

31-00:11:44

Smelser:

Yes. Well, it did happen to me dramatically. I think I must have reported this when we were talking about my year with Martin Meyerson, is that I simply left my meaningful audiences, which were my department and my national sociology peers. Those were the audiences to which I was orienting in my early years at Berkeley—first five, six years at Berkeley. This experience in the administration, particularly the time of crisis for the institution, just opened me up, not only in terms of my connection with other people throughout the university that I never would have laid eyes on, in the engineering department, scientists and whatever, but it also—there was an internal transformation which I just came to have. They were not exactly old blue feelings but they were feelings of love for the institution as a valuable thing in the world and that I was now experiencing these feelings that I hadn't before. I always liked it here at Berkeley from the very—from minute number one it was a place I wanted to be and felt comfortable but it didn't extend to this really powerful sentiments of love and affection.

31-00:13:03

Rubens:

Plus a serious engagement with the problems and mechanisms that—

31-00:13:09

Smelser:

Well, it just kept growing because I would—after this very dramatic introduction to the larger university scene, I kept getting involved in different sides and aspects of it and each time I would, things kind of grew not so dramatically but in subtle ways every time I got involved. The labs even became part of me as I was involved in them. So I loved that theme that emerged without planning at that last party.

31-00:13:45

McIntosh:

It's interesting, and in a way it's also refreshing to hear this appreciation of the institution from a faculty member who's also involved in the administration, because at least in my experience at Berkeley over the last five years, a lot of the faculty members are harsh critics of the institution and often seem to cast themselves in a light of being oppressed or tyrannized by the administration and by the institution.

31-00:14:16

Smelser:

Oh, it's a standard part of academic culture, I'm actually going to talk about it in my Kerr lectures, the deep suspicion really stemming from what is an inherited kind of culture of arrogance on the part of the faculty. And as I read all this literature on contemporary higher education, which is doleful literature, really, there's a lot of deep criticism and bitterness. And a lot of it focuses on faculty but most of it's on administration. And the Berkeley faculty is as guilty as any faculty of this prejudice. And I'm not going to exactly spring to the defense of administration but I'm trying to take a more objective view of, well, why did these features, why are they so significant and salient not only now but in the history of the institution.

31-00:15:13

Rubens:

It also strikes me, however, that there are many, maybe not to the degree that you have become involved, but there are so many that really give to the university. Almost at the—not expense of but on top of their own research. I'm thinking about the faculty that I've known. The lines are out the door for students waiting to talk to them or they serve on committees. There are those who choose not to go to other institutions, though the pay would be higher.

31-00:15:49

Smelser:

There is a love for the institution and a loyalty and a lot of times people will, like myself, turn down offers that look—are so attractive to go elsewhere. The university labors hard to keep its people. I've always said that at a certain level, this focus on excellence and leadership in academia is very complex and on the one hand it is a tremendous benefit to have the feelings that faculty and others have about the university as such a special institution. At the same time, you will find these antagonisms, ambivalences, instrumentalism and so on. But I think that thread of loyalty is what accounts for what you said about it, that people will participate in things that are not exactly career advancing.

The senate is a very good example. I do think we have the best senate in the world and it's a kind of culture that drives it. It's not anything more than that. It's a culture of participation and responsibility. That deep participation is done by a minority of people. No question. That has to be the case and the majority of faculty doesn't manifest the loyalty in terms of active participation. But they do attend to their departments and they do attend to teaching in ways that is not the case everywhere. My alma mater is a fantastic institution, Harvard, but the faculty is, as Henry Rosovsky described it, really a collection of individual entrepreneurs much more than it is the case here. Of course they live on a fantastic history and reputation and quality. I'm not saying that isn't the case. But the faculty culture is different in my estimation.

31-00:18:03

McIntosh:

Now, to get back to the goodbye ceremonies. Saying goodbye to a loved one often entails a process of mourning. Did you have any sense of mourning about leaving the institution in this fashion or were you ready to go when the time came?

31-00:18:20

Smelser:

I was ready to take on that directorship and I'll describe the circumstances of it. Let me describe my feelings of being the—I described the love and affection but I also felt that, having been at the same institution all my career, I really knew it and there were a limited number of new experiences I was going to have. That was not a source of alienation but it cut down the—and I knew I was not going to live in Stanford; we never sold this house, for example. I made up my mind I was going for a given term and we were going to move back. So I was still a major citizen of Berkeley and living those years in Stanford never shook that loyalty to both the university and community, which continues. But I did not have a mourning sense. There was a nice sense of celebration that I've tried to summarize, but I didn't shed tears even internally.

31-00:19:23

McIntosh:

At what point did you know that you were going to the Center and assuming that directorship?

31-00:19:29

Smelser:

Spring. I think maybe March of the year that I went. September was the beginning time of my appointment.

31-00:19:35

Rubens:

Now, Stanford announces it—I don't know if this really matters—but in February of '94.

31-00:19:40

Smelser:

Oh, okay. It was then a little earlier.

31-00:19:41

Rubens:

But you don't take the position until September?

31-00:19:46

Smelser:

The following September 1st was the time. But after I was selected, it was almost at that time that Jack Peltason asked me to stay. So I think I had actually agreed to stay, but then when the Center job came up—Peltason had a relationship with the Center. He may have been on the board. He was certainly on the committee on special projects. He understood exactly. They didn't raise a fuss about my not staying. They said, "You do it." There was no complication about it.

31-00:20:23

McIntosh:

Okay, that was one question I had, was whether the people who were celebrating you understood the next chapter of your career coming up and it seems like they did.

31-00:20:33

Smelser:

At my retirement party, I gave a mythical piece of advice to Tien. I said, "Here's a place where everybody's salary is arranged in advance. There's no negotiation. They come there, they're completely free to do what they want." And I described the thing and I said, "And everybody has free parking." I said, "Chancellor Tien, I recommend this model for the university of California" because I had already accepted the job at the Center and everybody knew I was going so I made a few jokes about it.

31-00:21:03

McIntosh:

So before we transition into a discussion of your time at the Center—maybe this is a good point to ask you about some of your intellectual work as a major statesman in the field of sociology in the late eighties and early nineties. You're producing a number of articles that are about the future of the field and where you see it going. So what were some of your major thoughts during that time?

31-00:21:29

Smelser:

Well, there were several publications that I never really put together in a package in my own mind until I really started thinking about it for these interviews. And I saw this clustering of commentaries and analyses of the field, things that affected it, where it was going, what are distinctive characteristics, what are its relations to the other social sciences and so on, all clustered in this period between '89 and '94. They coincided very much with the period of high activity in the International Sociological Association. I was on the executive committee and ran for its presidency and was elected vice-president in 1990. So there was certainly an engagement with international sociology that intensified. I'd been going to all the international meetings all my life but it intensified in this particular period and was an occasion for further reflections.

There are five articles that I wrote in this period, all of which were commissioned. So, in a way, it wasn't just my initiative. And I accepted them and I felt that I had something to say about it. So the first one really was

with—at the invitation of Herbert Gans, the president of the American Sociological Association, who wanted a series of reflections in a special panel meeting of the ASA meetings in that year. He wanted to talk about different facets of the field and I had recently been involved, of course, in the—this was sort of the end period of my involvement in the National Academy of Sciences Surveys of Behavioral and Social Sciences during the eighties. So this was a reflective period. My contribution was called “External Influences on Sociology.” And I talked not only about funding patterns but I talked about the federal government and I talked about communal involvement and I talked about different social movements that sociologists might have been involved in, about the fluctuations of national moods through the activist sixties and so on. So it was a general kind of synthetic essay on what the external influences have been.

31-00:23:55
McIntosh:

A sort of sociology of sociology.

31-00:23:56
Smelser:

Sociology of knowledge, in a way, yes. And I didn't get too deeply into the precise lines of influence and reaction, though I did account for the risings and fallings of different emphases in the field, like social development, like sociology of youth, gender and so on, and social movements that really shook and shaped the field. That appeared in the collection that Herbert Gans put together of major contributions to that annual meeting.

About the same time, I was invited to a strange little conference in Colorado, in Boulder, organized by a psychologist named Richard Jessor. And he invited one representative from each of the major behavioral and social sciences to kind of reflect on the field and reflect on one's own role in it. And this was not a particularly spectacular essay but I did include a biographical section in it, which is what I remember best. And what I did was to analyze my own role in different aspects of the field to which I had contributed. It could be a role of synthesizer but it could also be called the role of compromiser—and, in particular, in my work on methods, comparative methods. I just laid out what the major approaches were and how I had, in fact, without really knowing it or without really planning it, I had in fact carved out a path in which I really tried to seek out the meritorious features of radical positivism and of case study anthropological approaches and so on and tried to articulate a synthetic but also systematic way of incorporating these things. What comes to mind is the comment of my seventh grade teacher about being a diplomat. But it was never done consciously. I had reexamined different threads of my own work and built up this particular series of observations. Because Jessor encouraged us to be autobiographical when we felt like it.

31-00:26:21
McIntosh:

In reviewing some previous transcripts of ours, the word “catholic” is a word that you used frequently.

31-00:26:28

Smelser: Yes, small C.

31-00:26:29

McIntosh: With a small C, obviously. So that seems to be a good guiding principle of your career.

31-00:26:36

Smelser: Well, that's part of synthesis and part of incorporating. I once compared the three of the social sciences as follows: that economics was like Catholicism in the sense that they spun out separate orders and subdivisions but always maintained this central article of faith in one church, right, which is kind of the culture of economics. I said sociology was more like Protestantism. Schismatic and sectarian and split up in ways in which the protestant church did after its own formation. And that political science was Platonic in the sense that they had the—they focused on the idea of democracy as a pure form, as an idealized form, and much of their analysis had to do with the way it's realized, what its essentials are, deviations from it, different types of democracy. And so I think of myself as falling into the catholic mode in a field that is much more schismatic than my own thinking is.

31-00:27:48

McIntosh: And who do you think the pope of economics was at the time? Gary Becker?

31-00:27:53

Smelser: Gary Becker was one of the major spokesmen for one of the orders. I compared the different fields of economics as comparable to the Catholic orders. In other words, they are kept within the church but nonetheless show a lot of de facto differences among themselves. But Becker was—yes. And I will speak of Becker with respect to one of these articles that I wrote. Third article was—well, the Jessor conference eventuated in a book called *Sociology Retrospective and Perspective*. Same time, about 1991, I was asked to contribute to a special issue of the *American Behavioral Scientist* on international sociology and I took the occasion to write my own reflection because I had been involved in the ISA a lot and I had a lot of international contacts on the nature of internationalization of the field. And I talked not only about intellectual internationalization and the way that international collaboration takes place, but I also did a little analysis of the International Sociological Association and its intellectual base and its political base because it was formed as a kind of UN. That is to say, every nation is represented rather than an individual based organization. And it develops all the politics that the UN develops. And so I did really a kind of analysis of scholarly organizations and what their implications are for the internationalization of knowledge. It was again a balanced analysis between the strength and futures of international analysis and the barriers to it. Status, political, cultural differences and so on.

31-00:29:59

McIntosh:

Did you see the creation of a global sociology or a diverse and internationalized discussion among sociologists as emerging?

31-00:30:10

Smelser:

Well, I didn't speak of them as things. I spoke of tendencies which would push in the direction of internationalization. Tendencies which would continue to push in terms of a national style or regional blocs or whatever. So it touched on those themes but I didn't get into the abstract notion of a global sociology. I didn't know how to handle that intellectually. I felt I could talk best in more particular ways of the forces at work and identify targets, the third world resurgence, the jealousies of European sociology that had dominated international association for years and years and years. The special role of Americans in international sociology, of both leaders but members and so on.

31-00:31:04

McIntosh:

So the UN model really bears itself out in terms of it in theory being a global association but in practice having some imbalances within the organization?

31-00:31:17

Smelser:

Well, I interpreted and still believe that the years from post-war up until the eighties were dominated by the Cold War. And the dynamics were that the Soviet bloc was a very militant and aggressive part of the association and in a way wrote the agenda for the association by insisting on equity and parity and so—once they decided to join—Russians didn't join in until the sixties. They, of course, thought sociology was a bourgeois subject and they didn't have anything to do with it. But then they joined and then it became a—I almost call it a poisonous element by just insisting on equality in everything, all the way from language to representation on major committees to where it was going to meet. Everything became infected with the Cold War contest, as did much of the UN.

31-00:32:15

McIntosh:

Did a number of the representatives from smaller nations get entangled in this, as well?

31-00:32:22

Smelser:

Third world sociology was, until very recently, very not developed, even though there were representatives from Latin American and Asian countries that would come to the ISA and they were—in a way, they were both courted by the western bloc and the Soviet bloc. But they didn't assume an independent political significance until later on, really, until, I'd say, around the eighties and then the third world became an important element in international sociology.

31-00:32:52

McIntosh:

And so of these articles that you're producing during this time, are any of them specifically informed by the end of the Cold War? That uncertainty

about where the field is heading and where geopolitics is heading, is this influenced by the emergence of previously obscured voices?

31-00:33:08

Smelser:

Yes. I would have to say they were influenced by it. I didn't get into speculating what—this was in 1990, right. I was writing just as it was caving in. Of course, the collapse of the Soviet bloc was long in coming and really Solidarity in 1981 sort of marked the downhill course of the Soviet empire. But no. The answer to your question is I did not sort of see some vast sea of changes coming by virtue of this change. It was more contextual analysis of the flavor and the politics and the intellectual structure. I singled out the research groups as being probably the most healthy ingredients of ISA. They were not tied up with this representative mentality—they were volunteer groups of people with like interests and I think I spoke earlier of my great activity in the economy and society group. I saw the intellectual dynamism of international sociology. Not so much this great umbrella organization that spent most of its time fighting political battles.

31-00:34:38

Rubens:

You're concerned with the intellectual content?

31-00:34:41

Smelser:

Intellectual. I had written in other sources and did subsequently with international intellectual differences of sociological styles and so on. So I didn't ignore that altogether. But in this particular essay I was more interested in the contextual and organizational aspects of international sociology. Somehow or other they struck me as being—they were more salient in my mind at the time.

31-00:35:08

Rubens:

When we had talked about the conference you ran about German theory, we talked about dominant trends and the Germanic beliefs that were shaping sociology. At this point in '91, was there something that was more ascendant?

31-00:35:29

Smelser:

I didn't refer to that separately. I agree with you that I could have talked about national sociological styles and so on and so forth. There was some of it in that but it wasn't the most pronounced theme. I wrote an article with Neidhardt, Friedhelm Neidhardt in Germany a few years later, in which we talked about national styles and national differences in sociology.

31-00:35:58

McIntosh:

So what else are you working on during this time?

31-00:36:01

Smelser:

Well, I was invited by James Coleman to write a theoretical essay for his new journal called *Rationality and Society*. It was kind of his own baby. He was converted midway through his career into the rational choice perspective and he became the dominant spokesman in sociology for that perspective and was

very much a banner-carrying kind of—I had always maintained a longstanding—even from my undergraduate years— a longstanding ambivalence toward the theoretical styles in economics and had written a lot about it. Coleman invited me—we’d had a long relationship.

Early in my career, when I went to this conference just when I was married, 1967—1968 in Philadelphia, I was one of the three people who gave a paper. It was Coleman and me and Peter Blau, who is a little bit older. We were the three young sociologists who were asked to give a paper. And at the time, some of the senior people who came—many of the past presidents of the association came and so on. One of the past presidents said, “The three sociologists who are really coming up in this world are you and Coleman and Harrison White,” who was later at Harvard. And he kind of identified me. So Coleman and I were kind of paired as same generation, both kind of upcoming stars, and our relationship was always civil. He was very competitive.

31-00:37:46
Rubens:

He was at Chicago?

31-00:37:47
Smelser:

He was at Johns Hopkins and then at Chicago. And he got tied up with the economics department there, with Gary Becker. So I wrote this paper, which was a balanced but I would have to say ambivalent treatment of rational choice theory and comparing it with other kinds of theoretical approaches in the social sciences, and was invited to go to Chicago to present it to this group, including Coleman, Becker, some other economists of the Chicago school who were, of course, very orthodox in their approach, and a few other sociologists were there, as well. So this was a lion’s-den kind of invitation that I later accused Coleman good-naturedly of cooking up to roast me. But I went and I started. And within five minutes of the beginning of the delivery of this paper, Becker broke in with a really negative intervention. And from then on, I didn’t deliver anything. It was all talk. And it was fruitful, spirited. I held my own. I didn’t feel as though I was being beaten to death. But one of the other members of the seminar came up to me and said, “Congratulations. You’ve lasted four minutes longer with Becker than anybody who’s ever spoken to this group.”

31-00:39:28
McIntosh:

Is that right?

31-00:39:29
Smelser:

Six minutes was my time. He said, “That’s a record.” He said, “Becker’s usually in there after thirty seconds.”

31-00:39:35
McIntosh:

That’s interesting.

31-00:39:36
Rubens:

Do you remember what the debate was? What the issues were?

31-00:39:38

Smelser:

Well, I was questioning the assumptions of rationality, the central core. I was getting into the central core assumptions and saying how they—there were some predilections for the criticisms that I developed in the presidential address from a different angle. But nonetheless, it was an attempt really to assess where economic models applied and where they didn't. I really was taking, once again, my balanced or catholic approach to the idea. But, of course, Becker was not a catholic. He was a dogmatic and, of course, my approach and his simply did not mix, though I didn't see myself as negative toward economic theory. I saw myself kind of in the middle. He was way over on the right. You might say the right wing. And so it was a vigorous occasion.

31-00:40:32

McIntosh:

Well, ever since your early work with Parsons, a lot of your work has dealt with the sociology of economic life.

31-00:40:42

Smelser:

Yes, yes.

31-00:40:42

McIntosh:

And looking at informal networks operating in what we assume are formal institutions. And so the model of rational choice has been something that you've been critiquing for quite a while, no?

31-00:40:56

Smelser:

All the time. It's been a steady object of attention on my part and it has to be if you're in the area of economic sociology because of its kinship with fields of economics or lack of kinship, depending on how you look at it. So no, this was very continuous with my own work. And in the recent book on uses of social sciences in practical life, I spent a lot of time on economic and quasi-economic knowledge there, too. So that dialogue is not ended. Coleman published it in his journal, along with a series of other things. He organized several commentaries and I had a response to them in a section of one of the issues of his journal.

31-00:41:49

McIntosh:

Do you feel like you were given a fair shake?

31-00:41:54

Smelser:

Absolutely. Absolutely. Coleman was complete—it was not a setup in any sense of the term. It was a good intellectual dialogue. Vigorous and expressing a lot of fundamental differences but an honest intellectual dialogue.

31-00:42:08

McIntosh:

It's interesting, because I was listening to a lecture of John Searle's the other day and he told an anecdote about going to Chicago and arguing against the rational choice model with these exact same people. So the Berkeley/Chicago conflict seems to be continuing in the present day.

31-00:42:29

Smelser:

Oh, yes. Well, Chicago has conflicts with everybody in the sense that it just marks this orthodoxy associated with Milton Friedman more than—and Becker more than anybody else.

31-00:42:42

Rubens:

There was Strauss who influenced Milton Friedman and a lot of other people.

31-00:42:50

Smelser:

Yes. But it was some time ago. It is the seeds of orthodoxy and it was, of course, the close link with the Reagan Administration when it came in. There is where the advisors came from and so on.

31-00:43:05

Rubens:

Maybe this is a little off topic. Ninety-two is that conference. And I think that's the same year Becker gets the Nobel Prize —no, he was awarded the Nobel Memorial Prize, right?

31-00:43:32

Smelser:

Yes. I think I spoke that I was consulted on his appointment and argued against it and lost.

31-00:43:43

Rubens:

Was his prize for his identification of or discussion of rational choice?

31-00:43:49

Smelser:

Well, I think it was the extension of rational choice to the analysis of racial discrimination. He became involved in the analysis of family. It was derisively called home economics -economics of the family and marriage rates and marriage bargains and so on. He did it in strictly economic terms. And he and his colleagues developed a rational choice theory of drug use. They just went on and on and on. Criminality was one of the major areas of application, and race discrimination. All within a very orthodox economic framework.

31-00:44:45

McIntosh:

What were your arguments against Becker receiving the Nobel Prize and—

31-00:44:52

Smelser:

Well, I played exactly the devil's advocate for the value of these extensions of economic reasoning into areas which did not lend themselves to it. In other words, I thought it was an intellectually erroneous adventure on his part and bold, to be sure, and consistent and persuasive, as Becker always is. But nonetheless, I thought it was not intellectually worth what was claimed for it and the results that were argued for. I simply had a judgment that this was not the right way, right intellectual way to approach institutions which were fundamentally not economic in character. That's my take on what is called economic imperialism. And so I didn't run him down in a totalistic way but this was the line of argumentation that I put forward in my evaluation.

31-00:45:57

McIntosh:

Okay. I just wanted to make sure we got that on tape. So there's another article that you were—

31-00:46:02

Smelser:

Yes. This was also at the initiation of another sociologist. Kai Erikson of Yale. He's a longstanding friend of mine and my friendship with him predated my relationship with his father, Eric Erikson. So Kai and I were longstanding and really close. Our families were close. He was very instrumental in getting me a job offer extended at Yale in 1970. We stayed in their home. My bags got lost on the way to New Haven. He lent me clothes so I could appear, so on. And so we had a good relation. And he invited a number of, you might say, social science intellectuals together for a conference at Yale that he was chairing and it was commissioned by a new journal called the *Tocqueville Review*, dealing with the history of thought —this is a journal of social thought rather than social science. But nonetheless, he was commissioned to do it. He invited me to give a paper of my own choosing at this, but reflective of the field of sociology, right. So the title of my talk was called "Sociology as Science, Humanism and Art." And I extracted the essentials of these three mentalities and really treated sociology as having all of them and tried to interpret the internal conflicts in the field according to whether or not the impulse was scientific, humanistic or social reform, you might say. Or one which is not much talked about, the artistic, because sociology is often described as an ugly field. Well, that's an artistic term and a great deal of thought can be expressed in artistic metaphors. Mathematics, for example. These mathematicians, the highest word they can use about a theory is beautiful. Well, that's an aesthetic appreciative kind of—so I took this, you might say the artistic or aesthetic mode of thinking as a way to understand—it was a way of commenting on the internal tensions and differences and styles in the field. It was yet another angle in commenting on my own field. That's why I grouped it together with these other articles.

31-00:48:43

McIntosh:

I was going to say that the science and the humanist parts, I could sort of see where you were going with that. But then the art aspect is more—

31-00:48:49

Smelser:

The artistic side was more—got into the use of jargon, to whom are you speaking, is this a beautiful theory, it is that, and illustrated it from writings in the field. And so that was a kind of more original twist. The science-humanist is a deep running division that affects every department in the country.

31-00:49:12

McIntosh:

But looking at the creative potential of sociologists is interesting. I've never really thought about it in that light, of seeing the creation of new theories and the creation of new jargon as actually a poesies of sorts. Creating something that wasn't there before. That can be done either elegantly or—

31-00:43:34

Smelser:

Inelegantly. I believe in this article but certainly elsewhere I have talked and written about what is—I just learned the other day a term for it which I didn't use called neophilia, meaning the compulsion to make something look new or creative and different from what was in the past, which is in part an aesthetic impulse. In part it's a status driving impulse. Part is the generational conflict impulse and so on, which could be assimilated to, you might say, the larger aesthetic quality of the field. So I guess I would want to say that that was the more daring part of that paper.

But, as I say, these five essays all clustered in time and I believe they reflected my environment, my status in the field at that time, which was as sort of statesman and viewer of things. Certainly consistent with where I was in the field at the time. And reflective of my institutional involvements, as well. It foresaw a lot of the things—when I wrote on the problematics of sociology in the Simmel lectures, I didn't use these at all but it was in the same thrust.

31-00:51:11

McIntosh:

There's one more article from this period. It's about the future of sociology. I forget the exact title. But it isolates three terms in the title: centrifugality, conflict and accommodation.

31-00:51:25

Smelser:

Oh, yes.

31-00:51:27

McIntosh:

Would you mind just flushing that out, of what you were using those terms to identify?

31-00:51:33

Smelser:

Yes. That came a little bit earlier, I think, or at least early in the phase. And I didn't list it, though I could have. It was written for a Canadian journal of sociology, by invitation. A sort of comment on the field, same way, right. Centrifugality was really the tendency for specialization and subspecialization and was not distinctive to the field of sociology in general but certainly endemic on its own. And then I predated a lot of later analysis about the kind of religious analogy of sects and sect formation and schismatic tendencies in the—that's the conflict side of it, which I outlined and developed and illustrated at great length. And then went back to the theme of, well, what do you do when you see this expanding universe, right? Is there any way you can get your hands on it from the standpoint of unification? Of course there I was talking a lot more about myself rather than what most people do in the field. Most people get specialized, get interested in the status of their own specialization, get defensive about it, get aggressive about it, get conflictual about it and so I then introduced the third and my own more personal line of commitment, though I didn't identify it such. Well, what about synthesis?

31-00:52:53

McIntosh:

And accommodation.

31-00:52:54

Smelser:

What about bringing together, what about unifying, what about frameworks that are going to make sense of these apparently disparate lines of centrifugality and conflict that characterize the field. That was my first stab at that line of thinking.

31-00:53:09

McIntosh:

Great. Well, I wanted to make sure we got that in with this group of related articles, as well.

31-00:53:15

Rubens:

During this time you're elected to the National Academy.

31-00:53:32

Smelser:

In 1993 I was elected in the National Academy. This election, I have to say for the following reasons, was a bit of a surprise to me. The National Academy of Sciences recognized the social sciences in kind of successive order as latecomers. Anthropology and psychology were the first, and economics was earlier than sociology and political science. And as a matter of fact, the division in it is not sociology and political science. It's called social and political sciences. They're the last ones to have joined. And in each of these fields there's tended to be the dominance of the "scientific," quantitative science modeling and so on that have dominated the selection of members. In anthropology it's been mainly physical and biological anthropologists. Fewer cultural anthropologists. In economics it's been econometricians and the big data set people. Not your labor economists and not your growth economists, which are regarded as "softer" and less scientific. In psychology it's mostly—it's hard experimentalism, animal psychology with clinicians almost not represented because they are softer and humanistic psychology never. And sociology is dominated by demographers, by stratification theorists, by organization—the "more scientific" side of the field. And I am not in that. I've used a lot of the data and I write—certainly don't shun quantitative work and it was part of—a lot of my empirical work was quantitative and certainly historical empirical. But I was not known in that category of the tougher harder branches. So I thought, "Well, this is not"—They don't take historians, at all, or philosophers, except a small group of philosophers of science.

Talcott Parsons was kept out of the Academy after a huge fight. His work wasn't scientific enough. And at the height of his career. It was a huge fight. George Homans was taken in. Talcott Parsons was not. And it was a public fight in the Academy about whether—is this really science or is this just some kind of a philosophical speculation that Parsons is engaged in? That kind of mentality fed into my skepticism, thinking I might not be elected to the Academy. But I was, with not very serious opposition, nor feeling that I was being let in as an exception or anything like that. It was apparently a normal election. My chief informant, Gardner Lindzey, who followed the internal workings of the election, said there was never any real debate about my worthiness. I just hadn't been nominated before and got it. So I took Sharin

and my son, younger son, to Washington. It was a grand ceremony and I loved it.

Interestingly, I had been very active. One of the reasons I think I was probably elected. I had been very active in the Academy even though I wasn't a member. That whole sequence in the eighties when I chaired these major working bodies of the National Research Council. That was all Academy work. And it isn't the case that you earn your way into the Academy by doing service for them. That's not a principle. But sort of increased my visibility. My co-chair, Duncan Luce was already an Academy member. That past activity certainly contributed to my recognition. But apparently I was nominated and I got in the first year. Many people don't. The Academy has one of the most complex and arcane electoral systems in the entire world that no government should ever adopt. So that was a very nice moment in my career.

I had been elected to the other two main honorary societies considerably earlier, American Philosophical and the American Academy of Arts and Sciences, and this made a nice kind of recognition and rounding out of recognition as a social scientist.

31-00:57:54

McIntosh:

Do you feel like your election and the ease with which you were elected, as Lindzey reported, represents a shift within the Academy of how they viewed sort of the more humanistic approaches to social science?

31-00:58:09

Smelser:

No.

31-00:58:09

McIntosh:

Or is it specific to you and your accomplishments?

31-00:58:10

Smelser:

No, no. It was something about me because I know my service in the Academy since—I found this bias. I've written about it.

31-00:58:18

McIntosh:

Really?

31-00:58:21

Smelser:

The science bias. In a way, it infuses into it. And I actually talked about it in one of the more personal autobiographical boxes in my work on terrorism and working within the Academy on the issue of terrorism and this came up, the science/non-science social technical aspects of the issue. So I commented on that. No, it's still a big part of the culture of the National Academy.

31-00:58:49

McIntosh: Okay. Well, why don't we change the tapes and then get into the Center on the second tape.

[Begin Audio File 32]

32-00:00:00

McIntosh: Will you talk about your initial experiences at the Center for Advanced Study in Behavioral Sciences and assuming the directorship?

32-00:00:27

Smelser: Well, for people that might not be immediately familiar, the Center is a specially created institution in 1954 by the Ford Foundation—a child of the Ford Foundation. Gave a heavy endowment to it and built a physical location near the Stanford campus, on Stanford land. And it was designed, to some degree, a copy of the Princeton Institute for Advanced Study, though it never had a permanent faculty. All it had was visitors. Anywhere between thirty-five and fifty visitors a year from the social sciences, later spread into the humanities, history, philosophy to some degree. But it was always dominated by the social and behavioral sciences—with the occasional journalist, occasional physicist. Occasional biologist would come, as well. But people would come for a year, selected usually on the basis of recommendation and they would spend that year—they described what they were going to do but that was not—mainly the criterion was the promise or excellence of the people who were chosen. So it was a very elite group from the very beginning and they would come and spend the year. And the main thing they would spend it doing was working on their own research.

32-00:01:42

Rubens: Who chose them?

32-00:01:46

Smelser: The board of trustees of the center, which came along with its original formation. All academics were on the board of trustees. It wasn't one of these business and corporate and legal boards, even though we had a couple of members. And so the board was responsible. The director was responsible for coordinating the institution, for making stipend arrangements with fellows. We paid stipends, usually to supplement a sabbatical leave or something like that, to report to the board, to advise fellows when called for, to enforce—it's a place without many rules. It had no rules at all. We had a very strict rule that you couldn't teach while you were at the center. In other words, you couldn't take a job—moonlight. You couldn't moonlight at Stanford or Berkeley or anyplace else. That was a rule that we enforced rigidly and I had a little crisis over that rule at one time in my directorship. And we also made every effort to keep the fellows from traveling that much, because some would continue to do so and we thought that was against the spirit of the Center. So we would

make every effort to try to help them in turning invitations down if they wanted it, things of that sort. Never really hit anybody on the head, though I had a number of informal conversations with fellows who were, I thought, abusing that privilege of taking off when they wanted to while we were in fact paying. We didn't take roll, we didn't check the studies or anything like that, but it was one of those expectations. But by and large, you were a colleague with these people. I had lunches with them. I would go to all the seminars with them and so on. The quality of this thing, the freedom, was the great—and it also developed into an extremely cohesive and warm community of scholars. People from other disciplines got together. All lunches were collective, they were subsidized. Special groups sprang up spontaneously. That's the general culture of the Center and it's maintained that until very recently.

32-00:04:09

Rubens:

I just want to add that in a statement in Stanford's news release announcing you're becoming the fifth director. It says that you were selected from more than a hundred candidates after a national search.

32-00:04:24

Smelser:

Yes, it was a large search.

32-00:04:30

Rubens:

And we're going to talk about your long association with the Center in just a minute. You're quoted saying you think the Center is: "The leading American institution in fundamental research in the behavioral and social sciences."

32-00:04:41

Smelser:

Yes, I think I said that. I believe it. At the time, I'd say, yes, it was the kind of leader. Princeton was still more dominated by mathematics and physics. Radcliffe Institute hadn't been formed yet. There was a humanities institute in North Carolina, was a cousin and copied after the Center. Russell Sage Foundation was smaller as an enterprise. So I had these comparison groups in mind. It was, I think, a well considered and not just a heroic and celebratory statement at the time.

Well, anyway, I had a long association with the Center, largely as a result of my long association with Gardner Lindzey, who we discussed at an earlier time, was the chair of my undergraduate honors essay at Harvard and we maintained a collegial relationship. Over time, though, he was a psychologist but never in the same community as I. When we came back from my directorship of the Education Abroad Program, he asked me if I would be on the board of trustees of the Center. I had never been a fellow at the Center, so this was an unusual invitation, I think reflecting Lindzey's special relationship with me. I'd been authorized as a fellow. And once you're made eligible as a fellow, your eligibility at that time continued forever. Later we had to renew a person after six years. But I was made a eligible when I was in the early years here at Berkeley.

32-00:06:21

Rubens: Do you mean fellow or trustee?

32-00:06:22

Smelser:

No, I was made eligible to come there as a fellow, as one of the fifty. To just take one of my years off and just go to the Center as a fellow. So I had been made eligible as a fellow but I had never—in my first sabbatical I stayed here, largely. It was my choice not to go. I stayed here in my first sabbatical to be near my children and to continue my psychoanalysis. Later I felt I wanted to go abroad and go to exciting places in New York and so on. It was a lot more exciting than going down the peninsula forty miles from here and spending the year. So I never chose it. It was not a good tactical move if I wanted to be later involved in the governance of it. But this appointment to the board came in 1981. I served two terms, six years each. I went off the board in 1992, two years before I was chosen as director.

I played an active role on the board. Gardner Lindzey had a special body. It was called the Committee on Special Projects. Special projects is a group of four or five fellows, usually from different disciplines, who come in to focus on a given topic, such as human evolution or primate behavior or some cross-disciplinary topic. They meet, they come and they form a group and they usually publish a book together or co-author a lot of articles together. And they meet together separately from the rest of the Center and they're nodes of integration. There were three or four of them per year. And, of course, we had a subsection of the board of trustees, plus some supplementary scholars who chose which programs to sponsor or to accept and recommend who among the different people were going to run the programs and who should be eligible for a Center fellowship. And so I was on that committee for a number of years. Gardner Lindzey chaired it but he found it very arduous because he was director of the Center and chair of this committee and he was always kind of a conflicted role. And one day I took Gardner aside and said, "Gardner, you shouldn't do this. You should get somebody else to chair this and it takes you off the hook. You can be the director. You'll be there but you'll be the director." He said, "Okay, you do it." So much for these positive suggestions. You usually get roped in. So I chaired it for four or five years.

32-00:09:00

McIntosh: Were there any experiences as the chair in this situation that were particularly memorable or formative?

32-00:09:07

Smelser:

No. It was kind of a consensus committee. Colleagues who were kindred in their appreciation of interdisciplinary activities. We had a lot of debates, a lot of disagreements on specific projects. To be sure, you always do. But it was extremely civil and I'd say for that reason unmemorable. There wasn't any blood-letting in this body. And it was quite enjoyable. Met once a year. Made the decisions. Occasionally generated some projects from people we knew.

But anyway, in 1988, as it turns out, there was a search for a new director. Lindzey retired after fifteen years. He was a long-time director of the Center, and in a way he manifests its personality more than any single person. They ran a competition and I was nominated as a longstanding member of the board to become the director. I went to Washington, DC, where the selection committee met, was interviewed at length, and I knew that there was this large number of candidates at that time, too. I approached it with ambivalence. I wasn't sure that I wanted to be director at that stage of my career. It was just a feeling. I didn't analyze the feeling. It was a feeling I had. It turned out to reflect some unconscious wisdom. If I had been chosen in '88, I would never have finished my book on British working class education. I would have gone straight out of the senate into this administrative job. A lot of these other directions would have been closed off to me. And I have to tell you that when they told me, sort of in an apologetic, "Sorry to hear you didn't make it," sort of way, that Phil Converse had—I was second, came in second. Phil Converse had been selected. You cannot believe the surge of relief that I felt. It was matched only by my surge of ambition and pleasure when I finally got it when I was ready. See, I just wasn't ready and I do not know what I would have done if I'd been offered the job at that moment. I really don't. I would have been so conflicted about taking that position in 1988 just because of where I was. I'd been so involved in all these other things. It meant fully, in a way, giving up—not giving up but committing myself to this administrative or governance track that I had been so heavily involved in in the eighties. And while I didn't figure it out, my feelings were those, that I just wasn't—I wouldn't want it.

32-00:12:21

McIntosh:

But there was clearly something attractive about it, no? It's a very prestigious position. So that kept you in the race?

32-00:12:25

Smelser:

I described the job when I got it—another thing they didn't quote—as the best job in the Western world and I used to say that to people. It's not CEO in the strong sense of the term, though I did have CEO duties with the staff and board and external agencies, so on. It was a CEO job, minor to be sure, but it was more collegial. The intellectual relations with the fellows were the dominant core of my identification with the Center. It was a beautiful setting. It had the quality of a civilized institution and so I was very attracted, no question. It was a high prestige position. That didn't figure in my thinking at the time. And I didn't in any way conceive that I was going to be asked again.

32-00:13:26

Rubens:

Now, you had been chairman of the board of trustees, is that right, '85 to '86?

32-00:13:35

Smelser:

Yes.

32-00:13:37

Rubens: And that appointment must have been pretty honorific, prestigious.

32-00:13:40

Smelser: Well, yes. They usually had some Nobel Prizewinner or president of a university as chair of the board and Gardner wanted me to be chair. And Bill Bowen, who was president of Princeton at the time, didn't particularly like the idea of my being chair because I wasn't in one of these other big positions. But Gardner insisted so I served two years. The most memorable thing about my being chair was a big fight we had over the Reagan Library. One of the places that the Reagan Library was going to be was up the hill from the Center in the Stanford Hills. It ended up elsewhere.

32-00:14:28

Rubens: Oh, because of the Hoover or—

32-00:14:30

Smelser: Well, he had links with the Hoover and the political campaign to get it up there certainly was partly inspired by the Hoover Institution. Stanford faculty had a big antipathy to the Reagan Library being there and began making noise immediately. It was going to be located beyond the Center and the road that was going to lead through it was going to go almost through the Center's parking lot. And so we took a real interest in this. Of course, we had the same kind of usual faculty, academic liberal antagonism to Ronald Reagan. We opposed it. I petitioned Stanford as chair of the board not to have it there. Not at Stanford but said this was going to be a very disruptive influence on the life of the Center and its particular setting, its particular culture. I said jokingly, "We don't want these busloads of right wing children going up visiting the Reagan Library." As it turned out, the Stanford faculty kept up this warfare on the administration not to come to an agreement with the people who were representing the impulse. There was a committee, Reagan's, to staff the library there. And the faculty fought such an unrelenting war against it that the Reagan people just gave up at a given moment and the whole thing—it's located in Simi Valley now, Southern California, in a political part of the world which is friendlier to it than Stanford campus would have been. But this was the most dramatic aspect of my role as chair.

32-00:16:20

Rubens: Now, just a couple other questions. What were you responsible for as chair?

32-00:16:27

Smelser: I was responsible for the following. Of course, I chaired the board meetings, three times a year at the Center, hosted by the director. There was a national committee, occasionally an international member but mostly a national committee of eminent academics in the social sciences mostly. Some academic administrators. Bill Bowen is an example. Henry Rosovsky was on it for a period of time and others who had administrative roles, even though they were academics by background. It was pretty academic. The one exception was John Reed, the banker, with whom I later collaborated with on

this new book on usability of social sciences. But he was a member of the board of trustees during my whole time there. And I'll say something about my relationship with him. He was on the trustees with me and then he was a continuing trustee when I was director for seven years.

32-00:17:33

Rubens:

Your responsibilities, of course, was to chair these meetings—

32-00:17:37

Smelser:

Chair these meetings. To keep more in constant contact with the director. I was in contact all the time with Gardner Lindzey. There was a little executive group, me and a vice chair of the board and Lindzey formed a little executive committee that would make interim decisions between board meetings and so on. So I was really down there more often than I would have been if I were just a member of the board.

32-00:18:05

Rubens:

Selecting fellows, the director?

32-00:18:07

Smelser:

Well, the board did that as a whole. The chair of the board simply organized and monitored the whole process of the board meeting.

32-00:18:17

Rubens:

And finally fundraising. Did you have to do fundraising?

32-00:18:19

Smelser:

I didn't do fundraising until I became director. The board did not engage in fundraising. The director was the only fundraiser in the organization.

32-00:18:28

Rubens:

And why were you interviewed in Washington when you were—

32-00:18:30

Smelser:

Because it was a national search and members of the selection committee were in—some of them were Eastern seaboard people. They just located it there for travel. And I flew to Washington. But that's just because I was on the West Coast.

Well, in 1993, of course, they reopened a search because Phil Converse sort of had announced in advance that he was going to be director for five years, which he was, and after five years he said, "I'm leaving." So it was by just the sheer timing of the opening of it that a search opened up again. And I was nominated. This time my attitude was completely different. I really wanted the position. I was in the systemwide position. I had in a way left the Berkeley campus. I saw my return to Berkeley as five more years, or ten years of doing what I had done all my life. This was exciting for me. I'd come to really quite love the Center as an institution during my long association with it as trustee, and my special affiliation with Lindzey, who continued on in the board of trustees after he was director. He even continued on the board of trustees

when I was director, and I'll say something about that later. But I really, really wanted the job. Lindzey went to bat for me. He was still an influential character in the Center. And Bob Solow, the chair of the board at the time, an economist, called me up and offered me the job. I said, "I'll take it." I really didn't even negotiate a handsome salary or anything like that.

But the timing of my choice of the Center was—also, I was not ready to retire and if I had not gotten the job offer at the Center, I would have either continued on in systemwide in some way or other for a year more and gone back to the department and retired age seventy or something. It'd been uncapped by that time. I didn't have to retire at any given time. But VERIP 3 came in, the third retirement benefit package for senior faculty came in 1993, as we discussed in a previous interview.

I joined a new retirement system when I went to Stanford. I have a secondary retirement with TIAA-CREF. And so I didn't take it for that reason. It just happened to be the luck of the—the god of timing was on my side at this time. But I would not say that was the most decisive reason. The reason for my decision was I loved and wanted the job and was euphoric when I was informed that I got it. It was just a magnificent moment in my life.

32-00:22:41

McIntosh:

And so can I just back up and ask a painfully naïve question? So the Center is about getting these very promising scholars together in an informal way to do scholarship and pursue their own work. What does it offer that a university doesn't and how is that—universities are designed for promising scholars to do their own work. So what does something like the Center or the IAS, what is their mission? What is their justification basically?

32-00:23:20

Smelser:

Your question reminds me of one I once got from the National Science Foundation when they were considering renewing a grant for us at the Center. This guy was a hard nosed administrator from the government. He said, "Well, these people, what's the difference? They will spend that year at the beach. What's your value added? What do you—" "He did it in a hostile way. You're doing it in a friendly way."

Well, I'll answer the question in the following way. First of all, the freedom is complete. I always decided early in my career that to spend your sabbatical on your home campus is a big mistake. People don't believe you're on sabbatical. That happened on my first sabbatical and I vowed never to take my sabbatical at home again. Freedom. Okay. Furthermore, absolutely full-time. You leave your community. There are no committees at the Center, there's no governance in which anybody's involved. They're completely free. We have no social classes. No ranks. That was a very important insight that developed in the beginning. There was a huge debate at the beginning: Should this be like Princeton and have a permanent faculty and a visiting faculty? Or should

we just have a faculty? Should there be titles between senior and junior? Huge fight. It actually personified the fight between Paul Lazarsfeld, the European Austrian scholar who had more European—he thought you should have senior fellows who brought your assistants. Right. That should be the nature of the Center. Clark Kerr, who was very active in the design of the Center, said, “No, everybody has to be the same.” So there were no ranks built into it. It was absolute community. And some senior scholars sort of didn’t like this because they’re more used to having deference and so on, on their own home campuses. But it was all equal. Equal parking privileges, equal office space, equal everything. And no competition over stipends because they were all organized in advance. So there you’ve got a freedom and the capacity for a community, a community of scholars to develop that does not exist in the more siloed and stratified university systems. So it’s a very unusual kind of collective experience.

The reactions of fellows who go there is—I once went over the entire past year end reports that are required of fellows. It was like a collective eulogy. You just did not find anybody who did not find it sort of the best year of their life. And I used to orient the fellows when I was director. I said, “This is going to be the best year of your life and next year is going to be the worst.”

32-00:26:10
McIntosh:

So there is an implication, just in the existence of the CASBS and the IAS and other places like that, there’s an implication that university life is lacking in some aspect, right? That for—

32-00:26:26
Smelser:

Constraining.

32-00:26:27
McIntosh:

Constraining might be a better word. So is constraining in some aspect and that for really brilliant scholarship to flourish, there needs to be a different environment than the university provides.

32-00:26:37
Smelser:

The active interdisciplinary aspect was another feature. In other words, you were thrown in to meet people on an interactive basis which you never get on your own campus. You could spend your lifetime on this campus not knowing anybody except in your own department. Doesn’t often happen but it’s siloed in that sense. Here we have this kind of continuous interaction. Everybody’s at lunch all the time together. We have these interdisciplinary seminars and other features every Wednesday night. We had a seminar, people presented their own work back and forth, talk. Friendships developed. Those friendships developed into cliques that lasted into the future. It’s absolutely amazing the number of groups and personal loyalties that developed in this kind of setting in the Center. I guess that’s a response to your question as to what the value added might be of such an institution as this.

32-00:27:28

McIntosh:

And I just think it's a shame that these are only isolated institutions, right, and that so many scholars will go to a place like the Center and say that it was the best year of their lives and then it's just cordoned off in a few places around the country that offer this. It seems like something that should be more incorporated into university life as it is, no?

32-00:27:59

Smelser:

Well, we always talked a lot about this. We didn't seem to mind that it was a one year special event. We did have some fellows come back a second year and a few a third year. A tiny minority. And it was only—we'd do it if we hadn't filled up a spot or something like that. Low priority appointments but we did give people—and if they came back on a special project, that was the main way of getting a second year, if they were—

32-00:28:25

Rubens:

Why would you not have filled a spot?

32-00:28:27

Smelser:

Oh, people drop out at the last minute. Out of fifty people, you'll get three or four who drop out. There were between forty and fifty fellows in my time. It's shrunk since that time but in my time there were always forty to fifty and we filled all the offices up. They're all isolated. The whole place is isolated, kind of like a monastery. In fact, the architect thought of it as a monastery and it kind of surrounds an open area where you have dining outside, which you can do at Stanford. The lunches are taken outside. And it looks like a cloister, a secular cloister. Has a monastic quality to it. They wanted to isolate the fellows so much that for years they didn't have telephones in the offices. If you wanted to telephone, you had to walk across the quad to a public phone and talk to anybody you wanted to talk to. A lot of fellows griped about that and in the end it was defeated by the rise of cellular phones. You couldn't do anything about it. Email came in. So that isolation was only a temporary possibility.

32-00:29:35

McIntosh:

Sounds pretty fantastic to me, I have to say.

32-00:29:37

Smelser:

It's a great place. It's a great place.

32-00:29:39

McIntosh:

So coming in as director, did you see your primary responsibility as to maintain what was already existing or did you have new initiatives?

32-00:29:48

Smelser:

When they asked me in the interview what innovations I had in mind, I said, "The Center is a wonderful institution and if you ask me how much do you want to change it, I'll say not much." And I did name one initiative that I was bound and determined to undertake and did undertake, and that was to increase the international flavor of the institution. It had very, very small

numbers of foreigners there. Maybe two, three a year, and I had a target of seven to ten a year out of the forty or fifty. We didn't have funds for foreigners and foreigners did not tend to get sabbatical leave in the same pattern as Americans do, so it was a more expensive enterprise, but I determined that I was going to take the initiative and that subsequently involved me not only in expanding the network by which we chose the fellows but also securing money from Europe for the Center. Grants for the Center. I'd say that was the main innovation. I was constantly aware of the age distribution of the Center. It tends to creep upward by people who are well established because they're known. The criteria are accomplishment or promise and the younger people don't get recommended quite as much because they're not as well known. So it was a constant fight to get young blood keep coming in. And you never won that war but you had to fight it all the time.

32-00:31:19

McIntosh:

Well, I assume you need to have some way of knowing that the investment that you are making—or I guess maybe investment isn't the right word, but that the resources that you're extending to these people are going to be used properly and so for younger people who are offering promise, it's not as safe a bet.

32-00:31:44

Smelser:

You're a safer bet for older scholars. The board kind of fell into this. They never articulated it but, nonetheless, that was the kind of mentality that was working. It was well established. That person is a leader in this field or that field. Well established. Whereas the young people were usually protégées or recommended strongly by senior people we respected. That tended to be the pattern when you were leading with the—the selection procedure changed afterwards in various ways but didn't change radically while I was there.

I also had, as it turned out, an innovative impulse with respect to another issue that I did not have on my agenda but it came there, and that had to do with the role of spouses at the Center. These institutions often turn out to be very indirectly discriminatory against spouses and more are women than men. Fellow comes there, fixed up immediately, put him in the office. A computer expert comes in, teaches him how to operate the system. He's immediately there, lunches begin, studies begin, library privileges begin. We had a link with the Stanford library. You could get books delivered at any number you wanted by just saying you wanted them. They'd show up two days later. That kind of thing. Fellows really had tremendous perks. And we designed the place to make it absolutely maximum effectiveness for their own scholarship. Spouses would come along, sometimes with children. They were new in the housing, they didn't know the community, they were not involved in the Center life particularly, and oftentimes there were serious adjustments and oftentimes, I dare say, marital conflicts would arise out of this differential

status of the spouse. Some fellows would come without spouses and there was the issue of commuting. So that also became an issue.

So in subtle ways I fostered some innovations that would incorporate the spouses better. Trivially, we put their pictures up as well, the spouses' pictures, as well as the fellows' pictures up in the public entrance to the Center. We invited them to all seminars in the evening, to all the parties. We arranged later on babysitting during the parties on the premises so spouses could come and bring their families and have kids play in a given room under supervision during the social event. Whenever we had any free space—mostly we didn't have much free space, three or four offices at maximum because it was the crowded era in the Center's history when I was there. Any free space we had we would offer to a spouse to come work at the Center. You're not a fellow but you have full access to office space and interactions with the other fellows and so on. So it was more this informal incorporation of spouses I think made a marginal difference in the quality of life. Contrasted greatly with the Russell Sage Foundation where I went as a fellow, which spouses were just—didn't exist in terms of their being incorporated into the life of the Foundation. And the space with the other foundations around the world that we were affiliated with. Germany, Sweden, Holland and so on. So that was the third innovation, to respond to your question.

32-00:35:12

McIntosh:

Wonderful. Recognizing the different aspects of the scholar himself or herself, right? That the scholar is really a person with—

32-00:35:23

Smelser:

Well, they have to lead a life. We had another feature at the Center that was really very valuable. This gets into what my roles, different roles were, but we had longstanding—I think it was out of the sensibility of Gardner Lindzey who himself had undergone psychoanalytic training like me. One of my role models in that regard. He hired a staff psychiatrist part-time from the Stanford faculty who spent about half his time, at the Center. Advertised himself as an advisor to the fellows, helping them with advice about medical plans and coverage, things that will always change when you come to a new community, giving them advice on physicians and hospitals and facilities in the Stanford area, and serving as a person to whom they might talk if anything comes up. Never said, "I'm a psychiatrist and I'll give you treatment." But anything that comes up. And, of course, a lot of things come up in people's adaptation to a new community and fights with other fellows occasionally. There are all kinds of personal dimensions, plus the fact that I always felt that this was one of those institutions that you come with very high expectations about what you're going to get accomplished. And if you fall behind or get distracted or what have you, it takes a personal price. People get depressed. People get unhappy with it. They're wasting the year or that they're not living up to—all kinds of very deep and quite personal concerns and conflicts and occasionally depression come up. And this guy served a very valuable role. With my own

psychoanalytic background, I did not try to take over that person's role, but I worked closely with him. I never pried into who was coming to see him or why and he only reported to me when he thought something might have implications for life at the Center. So that was the agreement that we had. It was a great, great device. Added an element of humanity to the community that it otherwise wouldn't have. I'll tell you what I did the first year.

32-00:37:32
McIntosh:

Please.

32-00:37:33
Smelser:

When I first arrived. I decided that this international initiative was going to be real, not just talk to the selection committee. So I immediately began to exploit whatever European linkages I might have to get money and approached foundations. For example, a Swedish Banking Foundation. Other places I knew about. A couple of Italian foundations, a couple of English foundations. And I had some luck. I got grants from a Swiss foundation especially interested in adolescents and development and they gave us a lump sum of money which we could use to finance fellows coming in that area. The Volkswagen Foundation gave us a very sizable amount of money to finance Eastern European scholars. The Cold War had ended three or four years before. The rebuilding of social sciences in the Eastern countries was going on and we got into part of that, so we had a couple of years out of that grant. And other sources and got the Ford Foundation to give us a grant so we could get Third World scholars or people outside Europe and the United States. So we had some Latin American scholars. An occasional Asian scholar came under those auspices. So I was extremely aggressive and also I turned out to be a quite aggressive fundraiser. That's a part of the job that I knew I didn't like — liked the least, put it that way, but I decided to throw myself into it and in the first year I think I probably established myself in the heart of the board better than anything else I could have done, because I just augmented the income of the Center, which is always in need of money. There's an endowment there. When I went there it was about twenty million but it got nowhere toward covering all expenses of the Center, which are about five million dollars a year. So I had to raise on an ongoing basis income between three and four million dollars to keep the place going.

32-00:39:40
McIntosh:

On a yearly basis, right now?

32-00:39:41
Smelser:

On a yearly basis. We had a big grant from National Science Foundation that lasted during much of my career but then was discontinued and I had to do some real hustling to replace that. But fundraising was a big part and I took it aggressively and I started with a bang. During the first year I just said, "This is what I'm going to do." I'm going to try to augment and diversify the sources of funding. They were mostly private foundations. Mellon, MacArthur. Later on I got a lot of grants from Hewlett. But I decided to

expand the number of foundations. Batting average is very low in this business. Anyplace you go you're usually hearing, "Goodbye. We don't do that sort of thing." But occasionally you strike and actually I turned out to like it better than I thought I would, mostly because I got money, not because—the activity itself is not very—you're kind of a salesman.

32-00:40:36

Rubens:

Did you have dedicated staff?

32-00:40:38

Smelser:

Nobody on fundraising. That was one of the archaic features of that institution; the director did everything by way of fundraising.

32-00:40:45

McIntosh:

So it was really that you were drafting the grants and meeting and doing the interviews.

32-00:40:49

Smelser:

My associate director Bob Scott was helpful in this regard and I would farm out some drafting and some research to some staff members. But I was the one who did it. I was the one who visited all the places. I had no agents to go to these other—to these funding organizations. And some of it was done by correspondence, particularly in Europe. But I went personally to the Volkswagon Foundation in Hanover when I was there.

32-00:41:15

McIntosh:

Yes. I wanted to talk about the financial model of the Center but it sounds like there's a relatively small endowment and then it runs also on yearly—

32-00:41:27

Smelser:

Feeding.

32-00:41:29

McIntosh:

Yearly feeding from foundations.

32-00:41:30

Smelser:

Grants. Non-endowment grants.

32-00:41:32

McIntosh:

That must take a lot of time, especially if it's just only the director in charge of it.

32-00:41:39

Smelser:

Well, it was a big part of my job. I'm not able to assign it a percentage of the time I spent at the Center but if you call it just 20 percent of just engaged in fundraising and the complications associated with the fundraising that's a lot.

32-00:41:52

McIntosh:

And that is a model that seems a little unstable.

32-00:41:59

Smelser:

Well, it turns out to be unstable because the Center didn't survive under that model. It merged with Stanford after I left for strictly financial reasons. Now, I was blessed. I came at the end of the depression of 1991-93. I witnessed that recession from the president's office. It was really deep. It was the most serious one that the university's ever experienced up to that time. We're in a more serious one since. But '94 I went to the Center. That time the great prosperity of the late nineties kicked in just about the time I arrived. Such luck I could never imagine. During my time there, the endowment grew, mostly because of investment, plus I did get some endowment money. But not very much. The foundations don't like to give endowment money. They don't like to give their endowment money to other people's endowments. That's the kind of rule of the game. But the endowment rose in my years there to fifty million. From twenty to fifty. And it was mostly the boom years of the economy—plus the fact that I never had to depend on it because I was successful enough in keeping that three to four million flowing in each year. I didn't have to dig into that income and run the endowment down at all. So it could expand at its own rate.

So I'm very realistic and I say that my beautiful relationship with the board of trustees probably depended on that factor as much as anything. I never brought them economic woes about, "We've got to do this, we've got to cut the stuff, got to do this." I never had that in my whole time there. Again, the god of timing was with me because I could have been—my next director was buffeted around and had to cut and had to slice and I saw the endowment begin to shrink radically and funding drying up. So I can't take any personal credit for that. I was active but it happened to be the character of the times that I was there. The seven years I was there, it was just beginning to cave as I left. I got out of town, you might say, just in time.

32-00:44:26

McIntosh:

So a bit of a boom time, it seems like, and then with the Center thriving and one of your goals being to bring in more international scholars, was there an international reputation for the Center and were you able to bring in the people you wanted?

32-00:44:42

Smelser:

It had a sufficient international reputation that anybody that I—any invitation we sent out to European scholars was snapped up in a minute. We had our choice. Our big problem was locating them because we didn't have the network, especially the Eastern European. We were absolutely without resources to locate scholars. So I had to establish networking for selection of people in other parts of the world.

32-00:45:06

Rubens:

And how were people located? Through the US? You're saying it's the responsibility of the board of trustees to make the selection but did you have a systematic way to recruit?

32-00:45:17

Smelser:

No, I would just bring lists of recommendations to the Center, to the board.

32-00:45:21

Rubens:

Well, where did the recommendations come from?

32-00:45:24

Smelser:

All over. Former fellows played a big role in supplying names. We had to rely on more informal bases of networking in parts of the world where we didn't have former fellows and others. I knew scholars in Europe because of my international linkages. Will you please help us and give us names? And give us the names of other people who could give names? So I—

32-00:45:50

Rubens:

But in the US was there any formal kind of—

32-00:45:58

Smelser:

All network, no structure. All informal network. And I was most comfortable doing that. And I used some imagination in fundraising, to get back to that. I went to the Getty Foundation. They were having a group themselves. They have a residential research program. They were having a group on aesthetic theory and we were having a group on neuroscience, a lot of whom were interested in neuroscience of aesthetic experience. So I went to the Getty Foundation and suggested that we form some kind of liaison this year and have a couple of conferences between these people and, "By the way, would you pay for the whole thing?", which they did. They paid for our fellows as well as theirs. And it turned out to be a disaster, the conference. All these humanists that they had down there and it was the scientists—they couldn't talk to each other. They had a couple of meetings but they ended up in impasses, unfortunately.

32-00:47:02

McIntosh:

Well, the focus on neuroscience gets to one thing I wanted to ask, which was under your directorship what were some of the thematic focuses year by year that you all took on?

32-00:47:16

Smelser:

We had two types of special programs, one organized in advance and one of these I got money from a combination of federal agencies on applying knowledge to social problems such as drug use and criminality and so on. That was one of those special projects. We've had projects on human evolution, psychological evolution. Earlier projects included primate behavior, I think I mentioned. And just three or four a year. Diverse subjects. Opportunistic, picked up by some leader or pair of people in the outer world who wanted to come and get together on their chosen topic and write something. We had informal groups that sprang up. Almost every year there was a group on feminism. Feminist scholarship because they're women and they've got a common interest. They formed an informal group. We had a group on emotions in organizations. It so happened that there were several

emotional theorists of organizations there that year and some psychologists. They got together and so on. We had one really highly developed special project in which I took the initiative on cultural trauma, which lasted over a long period of time. These were opportunistic subjects, almost all interdisciplinary. We almost ruled it out if they were only people from the same discipline working on it. And we did not want special projects to become dominant. We always wanted to limit them to three or four a year because we felt that they would form sub-communities too much within the community. So we limited the number and we limited the total number of people participating in special projects, total number of fellows, to about a quarter of the full cohort and we always reminded them, "You're doing your own research, as well." That was a message I always gave to the special project people. And so it was a balance between this one big collectivity plus focused inquiry on special projects.

32-00:49:34

McIntosh:

And so it seems like you've respected the mission of the institution to bring these people in and then leave them to their own devices and I was wondering, in asking that previous question about thematic focuses, if, as director, you came in and said, "Okay, here are some major issues that I think we need to address as a center over the next decade or so and it seems like you are—"

32-00:50:01

Smelser:

No, I didn't use the fellows for that. I developed a lot of personal relations with the fellows. I went to every lunch. I went to every seminar. I talked to them on a one-to-one basis. I joined some of the special groups as though I were a fellow. I went to this emotions and organization one because I was interested in it and sat in on a kind of equal basis and so I made a big point — it wasn't a calculated strategy, but I mingled. My style was to mingle a lot with fellows and kind of become part of the intellectual discussion. I was very active in the evening seminars, asking questions, pointing things out.

32-00:50:39

McIntosh:

Could we talk a little bit about your cultural trauma work?

32-00:50:42

Smelser:

Yes. I organized this partly at the initiative of Jeffrey Alexander, my former student, who was at UCLA at the time. And he was very much interested in collective identity and cultural memory. And he and I got together and he wanted to come and be a fellow. He was obviously qualified to be a fellow but he wanted to come and he wanted to be a part of a special project. It was on collective memory at the beginning. That was the way it was defined. I picked up this initiative and went to the Hewlett Foundation and I said, "I would like to make this a mission of the Center. I'd like to have it extend over three years. I'd like to have two conferences on related topics, on collective memory, and then have a year in which we gather perhaps six fellows in a special project on the topic and I'd like you to pay for the whole thing." Which was a considerable grant. Maybe \$400,000 to get this going.

Alexander and I jointly organized the conferences. I participated in both of them. We edited one book and one special issue of a journal out of these two conferences. The book was called *Diversity and its Discontents* and then there was a special issue of the *American Behavioral Scientist* that we also did. It was on issues of post-modernist thinking about cultural identity. And then the year of the fellowship I told Alexander I wanted to be a full participant. He thought it was a good idea even though he was going to chair it. And we had four European scholars and we began meeting earlier in the year and it gradually coalesced that the best and most promising line of inquiry that we should engage in was cultural trauma because it tied in with collective memory, tied in with cultural identity, tied in with a lot of interests of these people. So this group met consistently and each one of them was working on a related topic. And Alexander and I and they organized a collective volume on cultural trauma that was subsequently published by UC Press with all of us listed as co-authors in it. It was a very intellectually vibrant special project and they were a combination of Alexander's acquaintances and mine. Goes back to the 1980s, of the German American Theory conference—a couple of them were German. One was Swedish, one was Polish and Alexander and I were the only Americans in it. But it turned out to be most fruitful and very exciting and I was more a fellow than a director in this. And it was all very comfortable in that regard. I didn't take any special leadership role.

32-00:53:53

McIntosh:

And was working with Alexander an easy partnership for you? Did you guys see eye to eye on most things?

32-00:54:02

Smelser:

Easy. He was my student. He wrote his dissertation with me and Robert Bellah as his two main advisors and my relationship with him continued. Bellah's faded. I kind of helped him out a lot. He was going through a movement from very radical to really quite conservative social scientist and he wrote his dissertation on Parsons and so he and I had a lot of dialogue during that time and I kind of once again had a kind of middle role. I didn't share his radical rejection of the Parsonian framework, nor did I accept his enthusiastic embrace as he changed his own political orientation. But we kept a close relationship. I tried to get him hired here at Berkeley a couple of times when he was at UCLA. Didn't succeed because the flavor of the department wasn't really his flavor. But he and I had an extremely good working relationship for life. We still do. And so it was a positive. It was a big plus and we had no fights. In fact, we operated as kind of two co-leaders of the group and we actually had to mastermind a serious conflict between a German and a Polish scholar going all the way back to the hardened sensitivities developed out of World War II. Especially the Pole. He was really aggressive and so we just had to take him aside and said, "Cool it. We're not here to re-fight World War II." We succeeded but it was a delicate issue.

32-00:55:54

McIntosh:

Because that is what a lot of the European scholarship on trauma has focused on, at least in my experience, is memory after World War II and the trauma of the Holocaust.

32-00:56:05

Smelser:

Well, German. Especially German memory. Coming to terms with the Holocaust is probably the single major theme of that. But Sztomka, who was this Polish scholar, wrote a most interesting contribution to this volume on the triple trauma of Polish history in the late twentieth century, first being conquered by the Germans, second being freed by the Germans and entering into the Communist domination and third the freeing from the Communist. He treated all three as traumas that exhibited all the manifestations of a cataclysmic historical event. It was a beautiful article.

32-00:56:44

McIntosh:

Well, we're getting close to the end of this tape and there's obviously more about your Center directorship that we should cover next time.

32-00:56:52

Smelser:

How much time do we have?

32-00:56:53

McIntosh:

We have about five minutes here.

32-00:56:54

Smelser:

Let me talk about my Humboldt lectures.

32-00:56:56

McIntosh:

Please.

32-00:56:57

Smelser:

Sometime during the year, I think in February or March of that first year, I got an invitation from Humboldt University to give the Simmel lectures, which were annually given by—almost always by a European scholar before. But Hans-Peter Mueller invited me. He had taken me on. I was a kind of role model of his and he had gone to Humboldt University and he succeeded in extending the invitation to come and give four lectures before the entire gathered faculty and interested students at Humboldt University on a topic of my choosing. A great honor. They were finally getting around to honoring Simmel, that they didn't treat very well when he was there because he was Jewish. But they now have these Simmel lectures and there was anticipation that they would be published. And so this was a big invitation for me because I was in my first year at the Center. It was going to mean an absence of a month in the spring of my first year, before the Center was closed. It was going to mean having to prepare those lectures because there was anticipation of having them published.

So I was sufficiently driven, however, that I didn't have too much trouble deciding that I would give them and that occupied—again, I was always a

busy boy at the Center all the time but I did some original scholarship and brought together my most considered thoughts on the nature of sociological analysis. And I organized it around micro, meso, macro and global sociology as four independent but penetrating levels. And I gave a lecture on each for the series. The lectures were most enjoyable. I got good sized audiences, always participating and interested. I gave a seminar to students, informal seminar, most of whom were originally from Eastern Europe and that was, of course, of great interest to me to explore with them their politics and their current outlook on the situation in Germany because it was a few years after the fall of the Wall and the politics of Humboldt University were much influenced because it was in the eastern sector and was only itself retransforming into a first class free university, which it wasn't before that time.

And so it was a good stay. Developed a beautiful relationship with Muller. I went back to Berlin several times at his invitation and have carried on a continuing link with Humboldt University.

32-00:59:34

McIntosh:

And these lectures were published as *Problematics of Sociology*, correct?

32-00:59:38

Smelser:

Yes. I approached Jim Clark, with some trepidation, at the University of California Press, because it's not the kind of book that would sell very well. A scholarly book addressed mainly to—Jim, ate it up. He said, "Send it to me," and within a year it was published. I was very delighted with it.

32-00:59:56

McIntosh:

And if I remember correctly, I believe you were saying in the book that the meso level was one that was particularly kind of overlooked by sociologists and the global. That the macro and micro were—

32-01:00:09

Smelser:

Yes. The micro link had been explored and I was part of that exploration. Did the meso, which included formal organizations, social movements in their capacity as linking somehow or other the institutional side of life with the interpersonal and personal side of life. They were never cast in that role so much and they almost didn't have a name, that middle level of social organization and process. And then the global is kind of just coming into being, following the changes in the world really.

32-01:00:36

McIntosh:

Is there any commonality between the term civil society and the idea of the meso?

32-01:00:48

Smelser:

Yes, yes. Close. Because of the inclusion that I made of social movements and voluntary organizations into this category. They're not the same as institutions like medicine and law. There are other social structures. They're more group

based, interpersonal mid-level. But at the same time they're not individual social psychological matters, as well, so that you're absolutely right, that that connection is—

32-01:01:16

McIntosh:

And just with the couple of minutes we have left. Could you just get down for the record the connection between the macro and the global? First the distinction between the macro and the global and then how they interpenetrate each other.

32-01:01:31

Smelser:

They fit into one another. The macro normally has been historically analyzed as institutions that are generally integrated on a national basis. National economies, national political systems, national cultural institutions, even medicine and so on. But the nation is the main unit of analysis at this level and that was kind of the defining characteristic of the macro level. This, of course, blends into international but the unit of analysis is no longer the nation in the global. It's relations among nations. It's systems. It's systemic impacts. It's taking into account the growing interdependency of the world through markets, through international organizations, through some degree cultural diffusion and so on, which really treats the nation state as part of a larger system in the relations among nations and in the world system.

[End of Audio File 32]

Interview #17 August 22, 2011

[Begin Audio File 33]

33-00:00:01

McIntosh: We're here on August 22 with Neil Smelser. This is interview seventeen, tapes thirty-three and thirty-four. Good morning, Neil.

33-00:00:10

Smelser: Good morning.

33-00:00:14

McIntosh: Just before we started the interview, we were talking off-camera about your role on history committees. We just wanted to get your take really quick, before we move on to the Center and your work after the Center, on what you saw your role as being.

33-00:00:33

Smelser: Yes. I had a lot of outside students in several departments. History, psychology, political science, were the main ones, but a couple in the business school, occasionally in economics, just because of my own interests. People had to get an outside member for their orals or dissertation committee, so I frequently showed up. In history in particular, I was sort of designated as a British historian, especially for the Victorian period, which was my own research —had written two major books. Since my dissertation, I was known as being in British history early. Many students were directed to me or came to me to do that, as an outside member. Every student has to have two inside and one outside members, so that means quite a few students. Over the years, I had maybe a dozen and a half total. Maybe that's a little high, but that's not bad for history. I played a probably more active role than most outside members. I was obviously interested in most of the topics that came my way. But I also conceived of myself, quite naturally, because this was the kind of historian I was in my own work, of somehow or other pushing these students to more general interpretations. Analytical or theoretical. Not forcing them by any means, but in a supportive way, giving them leads to literature, asking them about the larger implications of things, and I think probably exercising, via that route, a little more influence than one frequently does as an outside member. Usually the outside member goes along, has a few conversations with the graduate student, watches to see what the chair of the dissertation committee is doing by way of approval and guidance. If the chair of the dissertation committee and the other inside members generally approve of the dissertation, you usually don't make too much noise.

There was an exception to this in the school of education, school of social welfare, when I was also someone's outside members, and there were some questions of quality in some of the dissertations. I sort of played a role of the spoiler on those, and I felt bad about it because these are units with their own approaches and their own dignity, but somehow, some of the dissertations fell

short. I had to exercise a somewhat more activist role in terms of guaranteeing the quality. Actually, you have a lot of power if you don't sign; the dissertation is not approved, and the degree is not given. I never played any games of blackmail by holding up a student who was going to get their advanced degree in those fields, but I did play a role of censor or quality control. I played a little bit more insistent role than I did in history. I never had that role in the history department at all, or other letters and sciences. That's just the way it is.

33-00:03:31
Rubens:

You did mention one student that you developed a bit more—

33-00:03:35
Smelser:

Yes, William Sewell, a junior—the second, I guess you'd call him—was here in history in the sixties, I believe. He sought me out as an outside member for his dissertation, which was on the history of working classes in Marseilles, which was right down my line at the time because I had published my own book on British working-class history and really resonated with that topic, as well as with him. He was a very brilliant young man and showed it at the time. I developed probably the closest relationship with him of all the outside members that I had. We talked a lot. His father was a sociologist, an eminent sociologist, at the University of Wisconsin. I've mentioned him before. Bill obviously was exposed to a lot of sociology in his life. He didn't go for his father's line of sociology, which was highly quantitative, interested in social mobility, stratification, so on. He was breaking away toward the brand of humanistic history and social science that he later became so renowned as a spokesman for. He and I had a lot of theoretical discussions about sociology and social theory. He was already inclined. Later on, he came under the influence of Clifford Geertz, who was a very qualitative, sensitive cultural anthropologist, very much interested in values and subjectivity and so on. Certainly a cultural relativist. I teased Bill, the younger Bill, that I was his transitional object between his father and Clifford Geertz. He smiled when I told him that, but I'm not sure whether he liked such a comment on his own life.

33-00:05:29
McIntosh:

Just to segue a little bit, you're no stranger in your work to values and subjectivity as well.

33-00:05:36
Smelser:

Absolutely.

33-00:05:38
McIntosh:

Picking up where we ended during our last session, with the *Problematics of Sociology*, there are some specific emphases in that book, which is not schematic in a pejorative sense, but it is laying out a theoretical model. But there are emphases in that book upon issues of love, and “deep love” is another term, and affect. I would love it if you could just talk for a little bit

about where you saw those issues fitting into that model that you were building of the micro, meso, global, and the macro.

33-00:06:20

Smelser:

They came in at several levels. One would obviously find them appearing at the micro, because you're dealing in large part with social psychology as well as, if you stretch it, even in-depth psychology. There are, of course, a lot of tensions in the field of psychology and sociological social psychology as to how much emphasis to give to affect. There are threads of rationality in both of those fields that tend to downplay the importance of emotion in decision making and in social life in general. I stretched, as I recall in that book, stretched the applications of topics like intimacy and love further, because I got into the family structure, and talked about the family structure as a regulatory institution for intimacy and the changes that had gone on in that, and how the family had been differentiated or broken into multiple forms.

This has really tremendous implications for the larger society. Intimacy is something which is both coming from within, as with all emotional life, and strongly regulated by society through institutions like family, friendships, small groups, even voluntary organizations. All of them have an affective regulation quality, because they're all dealing with, in many respects, deeper beliefs and deeper feelings, and conflicts that arise out of them. I extended it into the rather more general levels of social structure and group life. In particular, there's a discussion in that book that preceded a lot of subsequent writing on my part, and that had to do with the social psychology and sociology of trust. There's a big section in there on that. That was just beginning to move in as a major focus in several social sciences around 1990, so I decided to talk about trust as an affect, trust as an aspect of interpersonal relations, and trust as institutionalized. Even in financial markets, all kinds of exchange relationships. Depersonalized markets run a lot on trust. This, of course, is out of the Durkheimian tradition, and nothing new in the social sciences, but that issue of trust reappeared in my chapter on groups in this forthcoming book on usable social science. I have a lot of discussion of the institutionalization of trust as it appears in networks, which have also gotten such a large play in contemporary literature, along with social capital.

33-00:09:16

McIntosh:

Related to the concept of trust seems to be the idea of allegiance as well. Where you are willing to commit to certain bigger—

33-00:09:29

Smelser:

Allegiance is a stronger concept, because it includes trust as one part of it, but it's a strong trust. You can have trust in a market in which you don't have any allegiance at all to the merchant, for example. You might hate him. But the actual transaction is based on a lot of understandings and trustworthiness of the two. That's what contracts are all about, in a way. Contracts can be seen or read as a way of guaranteeing trust when it's not likely to be fully operative on a completely informal basis. As Durkheim pointed out, they all have a

subterranean element of trust that's not spoken in the relationship. Allegiance is a stronger quality, and of course, if you have feelings of allegiance, it certainly enhances the degree to which trust can be generalized in a relationship. It doesn't have to be totally situational. It can be a kind of resource on which the relationship can have some flexibility in it because of that generalized level of trust. The term "generalized trust" has appeared in the literature, and that means you are predisposed to treat this person as honest, as non-exploitative. Of course, that is a big core of what you refer to as allegiance.

33-00:10:53

McIntosh:

During our previous interview, when we were talking about the problematics of sociology, you focused on getting beyond the nation state as a unit of analysis. I was wondering if allegiance came into play in terms of analyzing the disparity between the macro and the global, or if allegiance was more on the level of trust—

33-00:11:18

Smelser:

No. Actually, I could refer to some very late works of my mentor, Talcott Parsons. He wrote a somewhat lesser-known but nonetheless very interesting essay on the international system. Leave it to him, of course, because he was so focused forever in his own work on the issue of solidarity. He played up the issue of integration and allegiance, in selected ways, in international relations. I wrote an essay on Parson's economic sociology and his ventures into the international arena. I said everything he did was limited, because it's only one of several very important equilibrating mechanisms at the international level, one of which is military and handling of conflict. Parsons just didn't extend his analysis far enough, because he was so locked into this whole idea of international loyalties. It wasn't just reasonable alliances and that sort of thing. He saw there was an international integrative system, which I certainly appreciated, but myself criticized as being too limited a view.

33-00:12:45

McIntosh:

The international integrating system, though, is that not what the global level of analysis is doing?

33-00:12:51

Smelser:

One aspect of the global level of analysis. There's economic competition, there's political conflict, and there are semblances and perhaps a general international, you might say cultural or normative systems, I would call them, because they tend to be isolated into state departments, international organization of different sorts—what do they call them?—NGOs on the international level, scholarly organizations. They're highly selective, and the UN is a pale representation of what you would call general global integration, even though it plays its own role. Yes, I see international integrations as being kind of real but fragmented in its character.

33-00:13:45
McIntosh:

We had a chuckle many months ago about Parsons avoiding conflict in all realms of his work and personal life, but it seems like that has been extended even to his analysis of the international realm.

33-00:13:59
Smelser:

Yes, it was absolutely consistent. As I've pointed out in one of my essays, Parsons gave no quarter to his critics at all. As a matter of fact, when you talked to him over dinner or over a drink or something, he was kind of bitter on this point of view, or, I'd say, polemic, more so than he ever was in his writings. Even though his writings are absolutely consistent. He didn't change his line the last twenty years of his life, really.

33-00:14:27
McIntosh:

Now I had one final question before moving on to resuming our discussion of the Center. That is that the old school of doing social theory and doing political thought was to take the person and build a framework on top of how you saw the person as operating—emotions, reason, even the elements of the “soul” and so forth. When you started talking about love and affect and issues of psychology, such as allegiance, it almost seemed as though there was a return to “what is human nature?”, and then how can we take that and build upon it? Was that anything that you were consciously doing?

33-00:15:15
Smelser:

No, no, I wasn't consciously doing that. Now that you mention it, what I was doing was taking that strand of sociology and social psychology and holding it up to the mirror of more abstract and more inclusive levels of analysis. Of course you had to discuss a lot of the issues that are really classic in the field. Psychological determinism, issues of initiation, issues of agency, so on and so forth. Talk about synthesis being the core of my intellectual life. This was maybe a certain high point of it, because I insisted on the reality of these different levels of analysis.

33-00:16:09
McIntosh:

Love exists in the unit of the individual, but as you also were laying out, love is institutionalized.

33-00:16:17
Smelser:

And you can love institutions.

33-00:16:19
McIntosh:

Right. So these issues that are normally sort of seen as strictly being relevant at the personal level also seem to be relevant at the meso and macro level.

33-00:16:32
Smelser:

Even global levels. They echo upward and get organized in different ways at more general levels of social organization.

33-00:16:41

Rubens:

I think, explicitly, as well as implicitly, there was a critique of the way in which all systems theories had been laid out.

33-00:16:48

Smelser:

It was a critique of all of them. Theoretic innovation is usually picking out one kind of level, or one kind of approach within a given kind of level, and pushing it as hard as you can. That's what the schools of any given social science are known as. It's a source of infinite impatience on my part that that's the dynamic. It's almost built into the rewards system of the discipline, of being original and creating something that is highly selective, and then running with it, and then getting into all kinds of conflicts over it, which is intrinsic when you make that kind of selectivity. That's the dynamic of the field that I don't regard as being especially productive.

33-00:17:33

McIntosh:

I'm glad we got this on tape and thank you for indulging me and speaking about this.

33-00:17:37

Smelser:

Pleasure, always.

33-00:17:41

McIntosh:

Now is probably a good time to get back to the Center. One episode that we did not get to discuss last week was a tragic one at the Center, which is a suicide.

33-00:17:50

Smelser:

Yes, this was during my first year at the Center. Though I didn't try to show it, I was feeling my way as director of the Center as well. A situation arose. We had a political scientist who came to the Center. It was his second year. He had been there once before. I will not name the person. He was somewhat quiet, but as far as anybody could determine, not especially a disturbed man. The psychiatrist on the staff that I mentioned last time, Herbert Leiderman, was surprised by his suicide, which occurred in May. I was away. I was delivering the Simmel lectures in Berlin, and of course was heavily involved by email with Bob Scott, my associate director, who had to deal with the repercussions of it among the fellows. It was close to graduation time. The whole community was completely shocked by it, as you would expect in a community as tightly-knit as this, and they held collective meetings. They wanted to talk to Bob Scott a lot, and they came to talk to me after I came back. It was very tragic. This poor man had come to me just before I left for Europe and had requested being away. He said his psychiatrist had recommended it to him. He wanted to be away for a period of rest—away from the Center. It wasn't exactly permission, but I gave my blessing to his absence, saying, "However, you should come back for graduation." Well, this guy left, he checked himself into a motel in Palo Alto, and he slit his throat, and was discovered by the management of the motel. Everybody, of course, was extremely shocked. They held these meetings, and one of the interesting

things they discovered was that nobody really knew him. Pleasant talk, and I talked about his work to him sometimes, and so did others, but it interestingly turned out that no one really knew him.

Anyway, they came to Bob Scott, and then later came to me, and said, “You’ve got to do something about it so this will not happen again.” This was a natural sort of response in the community. They made a suggestion that I should, in my orientation, talk to the group and tell them about the darker side of life at the Center, and even mention this suicide. We talked about this episode some in an earlier interview when we discussed my Phi Beta Kappa lecture on California. I rejected that completely. I said, “I’m not going to treat these people like children and tell them what they’re going to be feeling during the whole year.” I wasn’t rude with the people who were talking to me about it. I was receptive to what they had to say, but I decided on my own that I wasn’t going to do that.

I was moved by the whole thing, and so I decided that every year thereafter that I was director of the Center, I would give them this California speech, which emphasizes the dark side of life in a utopia. Never mentioning the Center, but always just talking at this more general level. Of course, it was a topic which they resonated to. They’d all just come to California. Most of them were not California people. They were from outside. They were always extremely lively. It’s an interesting subject to everybody. There was always a lot of back and forth. But on two occasions, one of the fellows came up to me at the very end and said, “Are you talking about the Center? Are you talking about California or are you talking about the Center?” I didn’t open up to them about the reasons why I decided to give this California talk to all the gathering of people at the beginning of each year, but that was the background of it. I wanted to get my own little bit of depth psychology in through the back door, because it did resonate with the kind of experiences that they had. There’s a lot of idealizing of the Center, a lot of talk about it as being a utopia. The fellows all invent names like Eden and Magic Mountain and Shangri-La to talk about the Center as being this unreal and perfect world that they’ve been in. That is one of the elements. Of course, if you’re living by that fantasy, then you’re going to have a darker side always, and that was my communication to them by giving this California lecture.

33-00:22:38

McIntosh:

That’s interesting. I was reading a little bit about it over the weekend, and the language that was used to describe it was, it’s paradise, heaven on earth, for academics. Things like that, that’s so far on one side of the spectrum of idealization that it’s almost inevitable that—

33-00:22:58

Smelser:

Yes, and they would give a skit at the end. There was always a skit that the fellows would give at the, quote, “graduation” from the Center. One of them took place in the “Garden of Eden”. It was all around. I guess it is not

necessary that that kind of idealization should take place, but that community had those features, as one side was this, here's the perfect year.

33-00:23:25

McIntosh: So the suicide is probably an extreme example, which I hope was not duplicated throughout your tenure at the Center.

33-00:23:33

Smelser: It was the only one.

33-00:23:34

McIntosh: Were there other issues in that vein?

33-00:23:41

Smelser: As I say, I shielded myself from the psychiatrist. I thought I should honor that relationship that he had, the staff psychiatrist, with any people who chose to come to him with their problems. I became close to a few of the fellows. A handful, over time, talked to me about personal issues, and we had our occasions of infidelities and affairs that started. A couple of people changed their sex identification during the time that they were there. Nothing that disrupted the generality of things. You kind of kept your finger on the pulse of anyone who was going through some major transition at the time. Because the Center had opened up to women more than it had before, and because the Center had more people who came on their own, with their spouses remaining in the home community working, you had more complications of separations and dealing with those among fellows at the Center than you'd had before, when nearly everybody kind of brought their family, or their spouses, anyway. Those are complicated kinds of situations as well, but in a different way. The suicide kind of crystallized my own mind on this thing, perhaps made it more salient as something to keep my eyes on during the course of my tenure there.

33-00:25:26

Rubens: I just wanted to revisit how people came to the Center. I know Richard [Cándida Smith, Director Regional Oral History Office] came, and there was a group of about four or five historians at the time. They formed, I think—

33-00:25:42

Smelser: Special project group.

33-00:25:43

Rubens: Yes. Very strong relationships came out of that, especially for Richard with Tom Holt, who was an African American historian at Chicago. When I talked to Richard, he remembered that there was also a group particularly interested in sociobiology.

33-00:26:04

Smelser: Yes, there was a sociobiological group there.

33-00:26:10

Rubens: We talked about the effort to get people from abroad.

33-00:26:17
Smelser:

Here's the general picture. I don't think I covered it very well. Early in the Center's history, there were mostly self-referrals. People who'd been to the Center referred other people. It was a real old boys network. Gradually, over time, we began to expand the basis for recruitment beyond that. We still used it, but in the annual call for nominations, we began to include a membership list from the American Academy of Arts and Sciences, National Academy of Sciences, American Philosophical Society. We also taught young talent. Students of people who had been honored by going into those societies. We began to look at lists of all other awarding societies, like Guggenheims and so on, who got fellowships. This was a source. We began writing to selected—it had to be selected—maybe fifty—departments around the country, to the deans of the social sciences, to chairs of departments of all the relevant departments that we were interested in, and asking them for suggestions.

We began to spread the network out. There was a residue of ongoing criticism of the Center, that it was an old boys' society, and that people were selecting each other, and it was cozy and everything like that. It had ceased being that by the time I had gotten there, and I sort of tried to expand it, including the international searches that I organized to get people from abroad, where we didn't have much good information. As a matter of fact, it became a tension between the Center and some of its donors, who thought that we should be more democratic in our selection procedure. We did advertise the Center in some selected outlets, like the *Chronicle of Higher Education* and other places where people might be, and in the disciplinary job-shop journals that each discipline has. We had gone that way, but still, you can't satisfy enough a person of an egalitarian frame of mind. In the extreme, I'll refer particularly to the National Science Foundation, which was very much interested in our making a completely open application system. We were already talking self-nomination when I was there, and we had a lot of self-nominations. It was a perfectly legitimate way to get yourself before the board of trustees and get yourself evaluated in our system. But nonetheless, the Center, just about the time I left, a couple years after, went on a completely competitive, open system. I think that external pressure was a part of it, even though NSF was no longer giving money to the Center.

It still works that way. It was a kind of an interim system, diversified system, when I was there. We had panels for every discipline. We had something like fifteen panels, some of them interdisciplinary, to which we would send all the names of all the people who came to our attention, to rank them, to comment on them if they knew about them. These panels, many of them were former fellows, but not all. They were just experts in the fields of the disciplines. These evaluations from our panels came back to the Center, and on the basis, largely, of all the information that had come in, we made decisions at our board meetings on who was eligible. It was not just who should come next year. Who was eligible. We had a pool of eligible people, and our approach to those people, if you can't come next year, you're available to come for the

following six years, after which case you'll then be reevaluated. Most of them were reevaluated positively, unless they'd disappeared into deanships and given up their scholarship or something of that sort. So it was a highly diversified thing.

33-00:30:22

Rubens:

It sounds labor-intensive, too.

33-00:30:24

Smelser:

These panels worked, and they did their jobs for no money, conscientiously. Usually loyalty to the Center if they were past fellows. I was on the sociology panel for years. That was the main basis for selection. Then on top of that, we had these special projects, in which we entertained applications for special projects, from both former fellows and from new, who would get together three to five people and say "we would like to come and talk about this". Primate behavior. We had one on—the name escapes me. It was on sharks and dolphins, and social life among them. We had several people there for one special project. It was really interesting to me. Then we had this committee on special projects that I've already mentioned that would sit and judge on these projects and say, yes, they really should be able to come here as a group.

We had a little tension, because sometimes the special projects submitted people who were not eligible and who could not be made eligible, because they just weren't of the standards that we set, which were quite high. We wanted the top people to come to the Center. The board was always forever searching its soul for some way in which this person might be made eligible. Not cheating, exactly, but it created a kind of tension, and it created a real tension when there were spouses involved in the special project, and one was judged eligible and the other one wasn't. What do we do? You can kill the project by saying, no, you can't come, but your spouse can. I'd say that was a delicate issue that the board faced from time to time. I don't think they ever truly cheated, but it hung over the decision making process. That was the nature of the thing.

I turned over a lot of the work, the administrative work, for the decision making to my associate director, Bob Scott, who was an absolute wonder. He had been associate director for ten years when I was there. He was a person that I knew and I liked and had dealt with when I was on the board. Extremely competent and efficient person. A tremendous personality in his relations with the fellows. I just can't say enough about him. We were a very good team. We were both sociologists. That didn't determine that we would get along together, of course, but nonetheless we had a very, very good working relationship, and we resonated well together and seldom disagreed on some kind of intellectual issue. He tended to handle the enormous processing and coordination of staff. We were forever sending out for evaluations and handling the paperwork—that as you say, labor-intensive process of selection.

I sometimes initiated people to the board, saying, this person doesn't quite fit, perhaps, but I believe that we ought to consider this person for a fellow. One of them was the executive director of the publisher W. W. Norton. Not a scholar, not a faculty member anywhere, but he had taken a great interest in intellectually excellent projects. I knew him. We were both on the board of control of the University of California Press. I knew what his mind worked like. I knew what his intellectual values were. He did have a project working on how presidents publicize their own persona in their regimes and in their memoirs. It was a legitimate intellectual project. I pushed him because he wouldn't have made it otherwise, because he wasn't on the faculty of any institution and he didn't fit the academic mold. I did that for two or three other people, who, in fact, turned out to be very good fellows. The board almost always accepted my recommendation.

33-00:34:46

Rubens:

Are there a few other projects or people and their work, which, particularly stand out in your mind, which were real triumphs?

33-00:34:58

Smelser:

Well, I have to mention the one that I talked about last time, and that was the one on cultural trauma. That was an intellectual phenomenon as far as I was concerned. We proved to isolate a very important phenomenon, bring new kind of perspectives on it. It had been dealt with mostly by historians on collective identity. Historians of traumas. I pushed my way into the thing by saying we should talk about the general features of this kind of phenomenon and do what we can. I'll just add this at the time. The book was coming out in 2002. The one with Alexander and all the rest who were in that special project group. UC Press accepted it. They thought it was good. I had an essay, a theoretical essay, on the relationship between psychological trauma and social trauma—or cultural trauma. It took off from a psychoanalytic tradition, and I'd say it would stand mainly as a theoretical essay. The University of California Press said, we can't publish this book unless we have something on 9/11. Because 9/11 had happened just about the time they were beginning to process this, through their own process of evaluation. So they came back to Alexander, who was coordinating the publication of it, and said they wanted another chapter. Now, I had just joined the terrorism groups in the National Academy, so I was obviously the guy. The last chapter in the book was called "Epilogue: 9/11 and Cultural Trauma," which I authored as a second contribution to the book.

There's one on whales and porpoises and dolphins, that I mentioned. I went to all those things. I talked to the people. I remember the participants in it very well. One of them was the son of David Tyack, a college mate of mine who was on the Stanford faculty in the education school. It was a kind of moving on. I think I mentioned a couple of others. The informal one on emotions and organizations that I actually went to myself. Those are examples that stick out in my mind.

33-00:37:31

McIntosh:

As the Center began to fall on hard economic times towards the end of the 1990s and the beginning of the next decade, did the influence of donors and the need to justify the Center to donors affect the groups that were designed and the people that were selected at all?

33-00:37:54

Smelser:

No. We tried to keep those things separate in our procedures. Sometimes we would get a special project that I knew that I could go somewhere to try to get it funded, but that was usually after the fact. The funders didn't come to us. We tended to go to them. If we could try to find funding for something, we'd go to a consortium of federal agencies. We funded a couple of special projects out of applications that I was able to successfully support from a Swiss foundation interested in personal development, socialization and development, and from the W.T. Grant Foundation in New York, which was also interested especially in adolescents. They gave us a grant to bring in fellows studying in those areas, and I believe we financed part, at least, of one of the special projects with those funds. It was two independent operations. We brought them together as much as we could. I think I mentioned the Getty Foundation one on neuroscience and aesthetics. I got Getty to pay for that after convincing them it was a good idea to think jointly about it. It was all kind of ad hoc. You do what you can under those circumstances. You look around, see what you can do.

33-00:39:27

McIntosh:

In your role as a trustee, after you retired, did the Center have the same approach towards funding?

33-00:39:353

Smelser:

No, no. It actually was a source of tension. I had a blow in the middle of my tenure at the Center. The National Science Foundation, which had been generously supporting the Center for twenty years, decided they didn't want to subsidize us permanently. There was a lot of pressure within the National Science Foundation from the representatives of disciplines like sociology, economics, psychology, and anthropology, that they wanted this money for their own funding programs, and they were the ones who were biting at the director of the social science division in the National Science Foundation. I believe it was in 1998—I can't tell exactly—halfway through my term, the National Science Foundation began to make noises that they weren't going to fund us anymore, and they didn't. It was a million dollars a year. That's a big chunk of a five million dollar budget. What I did mainly was ad hoc it. I went to other foundations to try to get it compensated for. I mainly went to the Hewlett Foundation, where David Gardner was president. My old linkages with David came into the open, and he was extremely generous. He couldn't give us an outright grant freely. He had only limited discretion over that foundation's funds. So he did give us some general funds, but he also arranged that we got financed for several special projects. That helped compensate.

Now, later, funding became sort of the key issue about the time that I left the Center and went onto its board, because that was the dot-com bust. My successor was an amateur and was disinclined to be very aggressive about fundraising. In the meantime, these other sources began to wither away. Really quite soon after the time that I retired from the Center, the fortunes of the Center began going down. Its endowment dropped because of stock market collapses and the new funds weren't coming in. My successor lasted four years, but I just contrast my tenure as director with his. He was being sniped at all the time by the board. It was ugly sometimes. I felt sorry for the guy, and I didn't join in on the roasting or the attacks, because I felt I had a special role as being former director of not messing around too much in the affairs of the Center. It was in the last year of his directorship that the writing seemed to come onto the wall. The one who came in after him, Claude Steele, really was the one who negotiated the union with Stanford that took place during the last couple years of my time on the board. It saddened me greatly because I had such tremendous sentiment for the Center as it was during my time, which was just the glorious independence of an institution that was high status, high prestige, wonderful communal aspects, and someplace you'd love to be. I opposed it, but never voted against the merger, because I saw it was inevitable.

33-00:43:24

McIntosh:

Was it necessary for the survival of the Center?

33-00:43:27

Smelser:

Yes, it would have gone out of existence in five years, given the trends that were going on. Stanford picked up its endowment, added something to the endowment. Began, then, however, to be a unit of Stanford. I said, look, the little people are going to get their way in this business. Gradually they come in, and if you want to hire somebody new, they say, "Well, in what category does that fit? No, we don't pay that much for this kind of work on the Stanford scale," and so on. Bob Scott went back for a one-year shot a couple of years ago, and he was much discouraged by the infusion of the bureaucratic level, even though at the general level, the Center can still choose its director, can still choose its trustees, can still choose all the fellows it brings in. There was a lot of independence still residing in the Center, but it was this infrastructure that began to mirror—John Reed said when a big organization joins with a little organization, the little organization becomes like the big organization. That was his aphorism.

33-00:44:37

Rubens:

You wanted to mention your particular role with John Reed.

33-00:44:40

Smelser:

Yes. John Reed, I had known before, because he was a fellow of the Russell Sage Foundation, whose board of trustees I had joined in 1990. Just the year after I was there as a fellow, Eric Wanner, who I developed this great—he's the director—great, warm relationship with, and staff and everything, so he

put me on the board of trustees in 1990, and John was there already. I had known John also because I believe he was on the board of trustees at the Center before 1990, and I was on the board of trustees of the Center, so we served together, but both as co-trustees. Reed was a very tough-minded businessman and chief executive officer. He was really always ahead of the finance committee, because he was the only corporate person on the—

33-00:45:35

Rubens:

He was the CEO of Citibank?

33-00:45:37

Smelser:

Yes, during this whole time. He was CEO up until 2002, so for fifteen years. He was there during the whole time while he was at Citibank. He's tough-minded. He was always telling the Center it was spending its money the wrong way, these long-term trends were looking bad, and so on and so forth. He was always a very tough-minded guy and painted the worst scenarios for us, which I thought was a very helpful role. Well, when I became director and he was on the board of the trustees, of course your relationship changes. You're, in a way, a servant of the board. But at the same time, I was a colleague with John. We had good relations. He had remarried in the middle of all this, and we came to have very fond feelings for his new wife, and it became more of a family kind of relationship, gradually, over time. But nonetheless, he played this very tough role in the Center. He wasn't above being pretty direct and pretty harsh in his judgments, even though I never took them personally.

I also, because I did have a friendship with him, felt better about talking back to him, even on these issues. I'll give you one anecdote. During the time when we were opposing the Reagan Library, that I mentioned before, when I was chairman of the board, John was on the board at the time. He said, in the letter we wrote to Stanford, agitating with Stanford not to take the library, "Just tell them we'll leave. We'll find another spot. We'll go to wherever. Another campus or another institution." The board was shocked, of course, by this suggestion, because they were so enamored by the current location and ambience and everything of the Stanford situation. But nonetheless, we did it. It was kind of a threat. We didn't begin negotiating with anybody else, and the Reagan Library issue disappeared. They decided to put the library elsewhere.

One time in my time, the issue of the Center's finances, which were always under scrutiny and looking for funding, John suggested—we were talking about rental rates in Palo Alto. That was the only thing that really kept fellows away, was the rents they had to pay for housing when they came. They didn't get that much at Wisconsin or Vanderbilt or wherever for their own houses. They lost money coming to the Center. We couldn't help them out. We didn't have a housing allowance or housing fund. We did the best we could. The cost of living in Stanford was about the highest in the country, and so we were forever fussing and fighting about how to deal with this. The pay for our own

staff was an issue in this regard, because we couldn't undercut that market completely. We had to keep up with Stanford in our paying of staff. It was always a source of economic tension, and John Reed seriously said we should move. This was right in the shadow of Citibank moving their credit division to South Dakota. John was pushing this thing. I had learned to stand up to John. I didn't really have any qualms about talking back to him. He's a very forceful personality and pushed his views hard. I said, "John, are you seriously going to suggest that Bellagio should move to Dusseldorf?" That kind of ended the discussion. It was a way of saying, look, we have a special cultural relationship with the world here at Stanford, and I'd really like to take this issue off the table and not seriously consider going to South Dakota—that was an absurd parallel—but even to another campus. Stanford had been fought for by Michigan, San Diego—I'm not sure San Diego. Michigan, for sure, was after the Center, one Southern university, and Berkeley went after the center.

33-00:50:09

Rubens:

They wanted it?

33-00:50:10

Smelser:

Berkeley wanted to put it down here by the Richmond Field Station. Clark Kerr fought hard for it. He was one of founding committee members of the Center. At a given moment, he wanted it for Berkeley. Somehow or other, Stanford prevailed. It was competitive. When the National Humanities Center was formed several years later, I think in about 1990, around that time, various universities around—I think that's when San Diego came in, was for the Humanities Center. That ended up in the Research Triangle in North Carolina.

33-00:50:46

McIntosh:

Where did John's interest in the social sciences and behavioral sciences come from?

33-00:50:49

Smelser:

Well, that's very interesting. He was an economist by training at MIT. Majored in that when he was an undergraduate. I cannot tell you how he got so interested in the social sciences, but he began being supportive and he joined the Twentieth Century Foundation, as it was called then, and got himself in the Russell Sage Foundation soon, and then became a member of this educational foundation in Chicago that we got money from the Spencer Foundation. He got very enamored of the Center. Never was a fellow. I think Gardner Lindzey made contact with him on the Russell Sage Foundation board and got him on the Center board. He loved the social sciences. I always used to tell him he loved them more than we did. Of course, his great support of the social sciences came out after I was retired, and he suggested to me that we do this book on usable social sciences, and we've now collaborated and it's now going to be coming out. I cannot answer your question, but it was certainly a deep love. I gave him my handbook on economic sociology in 1995, shortly after it had come out. His wife told me that he took it along on

their honeymoon, which I continuously tease him about, taking the world's most boring book on your honeymoon.

33-00:52:40

Rubens:

I have a couple more questions about your personal and then the Center's relationship to Stanford. So the sociology department—

33-00:52:50

Smelser:

Yes. It was a standard procedure for the home department of the director to offer a courtesy appointment, which they gave me, on paper and as an honorary professor. I talked to the chair when I went there and said that I really didn't want to teach. I couldn't teach, wouldn't teach. That we didn't let our fellows teach, and I wasn't going to teach, and that I didn't want to come to meetings. I didn't want to get in there. The issue of pay—I didn't want any pay, in case they were thinking about it. It was definitely a courtesy. I knew some of the individual sociologists and saw them socially from time to time, just because I had known them before.

33-00:53:38

Rubens:

Where did their sociology department fit in the ranking of schools?

33-00:53:42

Smelser:

What was it then? Stanford had a long season of being a second-rate department. The basis for that department being second-rate was that it had decided it was going on a very special mission of having a small, experimental, super scientific methodology group. They had a group there of four or five people. They had come from Harvard. There were students of Robert Freed Bales at Harvard. They were all ultra-positivistic and missionary in their idea that small group research was really going to yield the kind of findings that a true social science could be built on, and they didn't want to change the department character. And they didn't change the department character for decades, and it hurt them, because they turned out these highly-specialized, brainwashed graduate students who didn't have general sociological training. It remained, I'd say, a second-rate department, until the administration self-consciously decided to go on a diversification kick and enlarge the department and get super scholars from around the country.

When Harvard sociology broke away from the social relations department, they got Marty Lipset and Alex Inkeles to go there. Jim March joined the department part-time, because he was in political science and education already. They began recruiting aggressively in a general way. The department, by the time I went there, had moved well up in the ranks. I would say it would be in the top ten departments. It is now, too. It's continued its striving to be a good general sociology department.

33-00:55:22

Rubens:

Was Marty Lipset there when you were there?

33-00:55:25

Smelser:

Part of the time.

33-00:55:26

Rubens:

Any reacquaintance with him?

33-00:55:32

Smelser:

I have to tell you, the year before I was named director—that was '93, '94—I had agreed to go to the Center as a fellow. I didn't mention this. That was the year that I got the offer to be at Peltason's office. I had already accepted and had already agreed to live in Marty Lipset's house. He was going away, so I had agreed to live in his house. When I got the job offer from Jack, I told the Center that I wasn't coming. I withdrew a matter of months before going. As I learned later, if I had wanted to cultivate being director, that was the worst thing in the world for me to do, was to withdraw after having agreed to come. When I went down there as director, I learned in the culture of the Center that a person who said they were going to come and then withdrew was an object of vilification among the staff. A few of them did that, and we cursed them all. I've often said to people, if I were campaigning for that job, that's the last thing in the world I would have done to refuse to be a fellow after forty years of eligibility. But as it turned out, history went the other direction. Yes, I knew Marty. We didn't have especially close interactions. He went to George Mason University in the middle of my time there.

33-00:57:17

McIntosh:

While you were at the Center, weren't you also sought after by the people who were attempting to form the Radcliffe Institute?

33-00:57:25

Smelser:

Well, yes. This was near the end. Radcliffe was in a state of great crisis, because Harvard had basically closed the college a few years before. There was a tremendous interest in not having Radcliffe disappear. The big question was how is Radcliffe going to survive? They had gone through all kinds of iterations and fights because Radcliffe alumnae were an extremely strong political force. The idea came up that they should form an institute for advanced study, and it should be the Radcliffe Institute for Advanced Study, and it should be located right in the Radcliffe Quad, which was extremely close to Harvard. Extremely valuable real estate in the Harvard setting. They more or less committed themselves to go in the direction of a center for advanced study, and women's studies was going to be the thread. It was going to be modeled after some of these different institutions that were around already—the Humanities Center in North Carolina, the Princeton Institute for Advanced Study, several European centers—I'll talk about that consortium that we had that I was in. They had many models to go by.

In my last two years of director of the Center, they chose, even before the thing was fully constituted, they choose Drew Faust, who subsequently would become president of Harvard, to be the director of the center there. I had

known Drew a little bit through the Guggenheim Foundation—she was a history reader for us—but not well. She came out just after she was selected to be director and spent a whole day with me, talking about the Center and its organization, and then put me on a special advisory group that included the director of Princeton and several others, to come for a couple of times to Radcliffe to talk with them about planning, and to talk with Harvard about what it was going to be. It was now Harvard's project. I welcomed this. I advised them very aggressively on special issues. In particular, whether or not it should have a permanent faculty, like Princeton, or whether it should be after the Center's model of yearly appointments. I fought vigorously against having a permanent faculty. I came up against rock-solid opposition on the part of a number of Radcliffe alumni on this issue, because they wanted a faculty. Radcliffe had never had its own faculty, and they were still burning from this historical fact. They wanted to have fellows for five years, or maybe permanent fellows. I said, this has caused a lot of mischief at Princeton, and I don't know that you want to get into that business. I spoke very forcefully and repeatedly on that issue.

33-01:00:53

McIntosh:

Neil, I'm sorry to interrupt, but I'd like to resume this conversation once we change the tape. We just need to do that really quickly.

[Begin Audio File 34]

34-00:00:08

Smelser:

The second issue I advised the Radcliffe people on was how exclusive to make it a women's center. The alumnae were extremely divided on this issue as well. There was a militant group that said we don't want to have anything to do with men, we don't want any men fellows, and we want the topics to be defined as women's studies. That was a strong, I'd say, feminist impulse there. Then there were other various opinions to make it more general. In my advising, I consistently came down on the side of general. I said, you should have emphasis on women's studies, and you should perhaps have an informal understanding that the majority of your fellows are going to be women. That's absolutely perfect. But don't make it a matter of official policy. Keep yourself open. Keep yourself open to academic fields, keep yourself open to how many people are going to be in women's studies, and keep yourself open with respect to all these issues. This narrowly-defined, militant—I didn't use those words exactly, but that was in my mind—will be less effective and less flexible as an institution. I won the day on that, despite the fact there was still a lot of grousing on the part of Radcliffe alums who were in on this advisory committee.

Then, later, I came back to a special session on what to do with a certain sub-institute, because Radcliffe had come to include a lot of other institutes. It had a library and so on, and it was multifunctional. I sort of argued that it should work its way towards being, more exclusively, a center for advanced study

and not have all these extra involvements that it did. So I came back on a special advisory session on it, and then I was put on the visiting committee a year later. I had a very intimate relationship, and still do. When Drew Faust went to the presidency, Barbara Grosz became the director. She and I formed a relationship. She was in the American Philosophical Society, and we huddled there all the time. She's now resigned as Radcliffe director, and she's going to be at the Center here in Palo Alto this year. I'm going down to see her next month. That's been a linkage in my life.

34-00:02:22
Rubens:

Did your view that there should not be a permanent faculty win out?

34-00:02:28
Smelser:

Yes. It was a rather delicate relationship with Phillip Griffiths, the director of Princeton. In the end, he had had so many headaches with his permanent faculty that he didn't put his model up and fight for it.

34-00:02:43
McIntosh:

In addition to directing the content of the Center and being consulted by other people, such as Drew Faust, on starting similar initiatives, what other functions, administratively, did you serve—

34-00:02:57
Smelser:

Well, I had a staff. There were twenty people who were of an administrative staff. We had an editor. We had a kitchen that we had hired two or three people who prepared all the lunches for the fellows. We had a secretarial staff of three or four in the front office that served me and the associate director and the assistant director. We had a library and the people who dealt with getting library books from Stanford. We had a graphic artist. We had two computer experts and one statistician, and a budget officer and an assistant—so you can see how it built up into a sizeable staff.

34-00:03:39
Rubens:

And a housing person, too.

34-00:03:40
Smelser:

Yes, we had a housing person, and that person spilled over into arranging social events, like parties, at the Center. You can see how it adds up to twenty. This staff was decentralized. There's always a delicacy when a new director comes in. People are always wondering what in the hell is going to go on. Is this guy going to be permissive? Is he going to be a stickler? Major policy changes? What's going to happen to funding? All these things seize a staff when a new director comes in. I knew all about this. I had studied the literature on organizational succession of leadership myself, and I knew all the delicacies that come up in organizations when a new leader comes in. I decided to kind of take a more proactive role in my first period. So I went around, I visited every staff member individually to get to know them, to get their take on their own position and any views they wanted to express to me on the Center in general. I did that in the first two months of my time there.

Went to everybody individually and had an hour, at least, with each one of them, which I thought was the right thing to do. Get a little bit more personal with them.

I had meetings with the staff from time to time, particularly when there was some troubling event that took place. I certainly had a meeting with them when the NSF funding stopped, and I simply said, “Don’t panic, we’re going to go on as usual”. With each individual staff member, I tried to develop a kind of individual relationship. Some were remote themselves, and some were kind of chronically a little dissatisfied, as you get in any group of employees. You’re going to have different outlooks. My personal relationship with some was much easier and closer than with others, but I tried to be able to keep an individual relationship going. I didn’t have any, what you call major collective uprisings on the part of the staff. In fact, I saw that as a quite smooth relationship. I thought my style, which was pretty open, nondirective—my successor wanted to bring in kind of a dress code for the staff at the Center. That was the farthest thing from my mind that I would want to do, because it was extremely informal and so on.

34-00:06:18

Rubens:

I meant to ask what you, as director, wore.

34-00:06:20

Smelser:

Except when there were visitors coming or fundraisers or something, I wore turtlenecks. I had about a hundred turtlenecks.

34-00:06:31

Rubens:

It wasn’t coat and tie.

34-00:06:33

Smelser:

No. I used coat and tie when the trustees came, et cetera. Special occasions. It was an informal place, so I didn’t fuss around about dressing codes or clean desk campaigns, that kind of stuff. I was a permissive director and tried to direct throughout by personal influence. We never had any collective disturbances or delegations from the staff coming to see me or anything of that sort. I had to deal with individual problems of merit increases delicately, but I did that entirely with individuals because there was a lot of dissatisfaction among those who didn’t get what they thought was enough by way of merit increases each year. The trustees voted a certain percentage, and then I had to allocate it according to merit, and that was always delicate. I had a few personnel problems. I had to fire four or five people during the course of my time there. One was an incompetent housing officer who came here the first year. She just didn’t do her job. I guess that was delicate, but I simply had to tell her she couldn’t come back, and that was it. We had a custodian who was an alcoholic. The custodian drove the Center van. We had a van. His driver’s license had been revoked six months before I discovered it, but he was still driving the van around. You can see what kind of situation this is. I could just see this guy, carrying no license, killing a child or something in

Palo Alto, as he was driving around drunk. I just simply had to call him in and say, you're out of here, gone. Of course, he had been there for ten, twelve years. He had some affectionate relationships with staff members. It was difficult. I had kind of gotten to like him, too, but it was very, very difficult to do. There was a receptionist was using the Center's credit card to buy her personal furniture and toys for her children.

34-00:08:55
McIntosh:

That's a pretty clear-cut—

34-00:08:56
Smelser:

Yes, had to do that. We had another one that I'll just have to tell you about. We hired an assistant to the librarian late in my time there. A person who did more routine work, filing and book sorting and that sort of thing. One person left. The librarian was from Santa Cruz, and she wanted to hire someone that she'd known in the Santa Cruz library, a young man. We had our usual search, and we interviewed four or five people. We had a shortlist. We finally decided on this guy. I interviewed him myself at the end. He seemed good. He seemed to have the right kind of experience, and he certainly had the backing of the librarian, so I said, okay, you're on. I talked a little bit about the community of the Center, what kind of community it was and so on, just giving him a little orientation. He said, "Oh, I know all about communities. I'm in this special community down in Santa Cruz." I let it go by.

Then about an hour after my interview, I said to myself: community, Santa Cruz? What's going on here? I said, let's do a little more checking up on this guy. Just kind of a feeling I had. So we call up one of his referees, and she said, "Yes, there's a problem with this guy, and I'm not going to tell you about it." She wouldn't get involved in saying anything negative about him. She said, "However, I can give you the name of a person who might be willing to talk to you." She gave us the name of another person. We call the other person up. This new employee was in some kind of commune down there. Not a commune, exactly. It was some kind of a community. He had gotten himself in the soup in that community, apparently for showing the wrong kind of movies to kids, young boys. This woman told us about it. We had hired this guy, and here I was, faced with a situation. We had children around the Center, baby-sitters for our parties, our fellows' children. I just said there's no way we can go ahead with this. I did consult with the Center's lawyer extensively on this issue. Said I saw no way out except firing this guy in advance of his coming. I didn't want to have him on the premises and then fire him. The lawyer said okay. I picked up the phone and had to call this guy, and I had to tell him what we'd heard, because I just couldn't say, "We can't do it." I told him, "We'll give you two weeks salary before you come, but we can't have you here." I told him what we'd heard. Actually, it turned out he didn't confess to it. There was this long silence. He says, "I hope you will not make this available to anybody else." So in a way, that was a confession. That was it. I thought I got away with it. Skin of my teeth on that one. I didn't have

many personnel situations like this, but they were the usual CEO things, and they were all delicate in one way or another.

34-00:12:08

Rubens:

You sure had a freer hand there than you would have at Berkeley, don't you think?

34-00:12:11

Smelser:

Oh my god. At Berkeley, when I was chairman of the department, my assistant turned out to be an alcoholic. She came to work and she couldn't take notes. She'd go to sleep in the middle of the day. It was just a total incompetence situation. I found myself helpless. I had to go six months even to get a transfer of this person into a less sensitive position in the department. We isolated her, and then finally she quit. Typical atrocity story you hear in the Berkeley scene of not being able to fire anybody. No, no, I was much freer in that situation.

Now, I would like to talk a little bit about my entertainment role. There's a social dimension to it. Each year, we had two huge parties at the beginning of the year. Divided the fellows into two categories: micro, macro, or whatever you want to call them. We had a party at our house, right on the campus. A hundred people at each one of them. It was a big do. We tried to get the Stanford faculty there, so that they might interact informally. We encouraged that. Even though we didn't encourage them to teach, we encouraged them to interact with the Stanford and Berkeley communities. I innovated inviting Berkeley faculty to those parties as well as the Stanford faculty, but they hadn't done that before, so I added that to it. Sharin and I went out of our way to do some extra things. We had breakfast for fellows during the course of the year. Maybe ten over the year. They'd come to our house. Sunday morning was always a very relaxed time, and so we'd have a breakfast about 9:30. It would go on practically until lunch every time. It was always a nice setting and a good way to be a little less formal with the fellows. We chose them not always by common interest, but by people we knew knew each other and would work out.

There was an entertainment function that was built into my job when I arrived, and that was, at the end of each board meeting, we had a dinner at our house. It could be a catered dinner, but it was at our house, and all the trustees came. It was sort of a nice, social tension-release after the board meetings. Sharin decided when we went to the Center—her life had changed. She'd been teaching at Head Royce School, where our younger children went for a number of years. She was teaching photography there. She was there for five or six years, decided not to teach there anymore, but then the Center came up. Changed her life completely. We moved out of Berkeley. Friends—we still kept them, but it was a little more difficult on a day-by-day basis to see our friends. New social network and an entertainment role that was expected with the trustees, with the fellows, and so on. She decided, well, in the time that

we're going to be there, she's going to change her way of life, and she's going to take this role on seriously, particularly in entertainment of the fellows. I was so blessed. She became the dessert expert for these dinners. For every meal, she would prepare, on her own—we got the main course catered—she would prepare, on her own, three separate desserts, all delicious. They're in three different categories. One chocolate, then maybe a fruit pie or something. Three different ones. Always three, and always good. It became a kind of cult, these desserts. Our fellows could talk about nothing but these desserts. The spouses of all the trustees would usually come out for a weekend in California, so we had a lot of spouses come to these. There were thirty people there, or thirty-five people there. She got herself a name as being the dessert queen of the Center. I always used to joke, just saying any goofs that are made or any flack I got during the course of the meetings was completely forgotten after the desserts were served. It was always very open and nice. People would always get up spontaneously and thank each other and say nice things about me and about others. It was a really wonderful aspect of it. The social life of the Center was one major dimension of it.

34-00:17:10

McIntosh:

I see a picture developing of your time at the Center as director, which I believe is from '94 to September 1, 2001, as being one that's engaging you on multiple levels as an administrator, as an intellectual, as a fundraiser. Even the social aspect, which seems actually quite important to the Center. When it came time for you to resign, was that a difficult decision for you?

34-00:17:48

Smelser:

No. No. When I got the job, I said, I'm going to take this job for seven years. I actually made the number of years explicit in my own mind. The reason I chose seven years was that it was a little bit longer than the average director. No, it wasn't longer than average. Phil Converse was there for five. I thought that was a little short. Gardner Lindzey was there for sixteen, well I was of a certain age—I was sixty-four years old when I took the job. I wasn't going to be there for sixteen years. So I said this in my own mind. I said, look, I'll be seventy-one years old. That seems to be a reasonable time to retire. In a way, that schedule never broke in my own mind. I never told people at the beginning that I was going to be seven years. A year and a half from the time that I was going to retire, I told the chairman of the board, Steve Stigler, the statistician from Chicago, that I was going to retire. It was that simple. I guess my inner feelings were such that I had kind of come to love the job so much that I at least entertained in my own mind extending it a little beyond that time. A couple more years, maybe. But then I—what am I talking about here? It basically feels kind of right to retire. This little conflict about staying on a little longer wasn't what I'd call anything I lost any sleep over. I said no, I'll just go according to my regular plan. I wasn't in any trouble with the board. It was certainly my decision, and there was no way in which I was forced out. No one even mentioned my leaving during the course of my tenure there. I

just said, well, now you've got a year and a half to search for a new director, and I'll be leaving in September of 2001. It was a relatively clean leaving.

34-00:19:48

Rubens:

Did you have work in mind already that you wanted to do? We haven't talked about the encyclopedia, which you had been working on.

34-00:19:55

Smelser:

We can start talking about that in a minute. I think we've kind of wound up on the Center. [Discussion of successor in Interview #19]

34-00:19:59

Rubens:

Were you thinking, okay, there's other intellectual work in me that I'd like to pursue?

34-00:20:05

Smelser:

I had in mind writing a book on the odyssey, immediately after coming back to Berkeley. I didn't want to come back and teach here, though I did teach the first year of a special course in writing to graduate students. They persuaded me to do that. It was for one year only. I decided to do that in 2001 and 2002. I decided I was going to go to work on the odyssey book right away. I said there's not going to be a stop in my career. I'm not going to kind of sit around the house. I had this project in mind, and I really started working on it, more or less immediately after coming back. Working on it formally. The idea had formed in my mind. Then, of course, the terrorism project materialized immediately, because I retired September 1, 2001. Here comes September 11. Then the academy got me involved and so on, and I put the odyssey book on hold and did the terrorism book, and then came back to it. I went more or less immediately into action after coming back on my scholarly work. Then, within a matter of months after I came back, the opportunity to join this postdoctoral seminar in the school of public health came up, and I decided that any teaching I was going to do would be in that program. I've been involved with that ever since.

34-00:21:25

Rubens:

I don't want to fully leave your time at the Center because there's a whole swath of involvements that you have while you're there, with the Guggenheim, your presidency of the ASA, and then the National Academy, but particularly writing the encyclopedia, if that seems like a legitimate way to proceed. I just had one last question regarding your time at the Center, about your relationship to directors of other centers.

34-00:21:59

Smelser:

Oh, yes, yes. We had a club that was in existence when I came to the Center. We had a joke about it. We called it Some Centers for Advanced Study. It was only six of us, and we jealously didn't let anybody else in. It was an informal group. There were three American centers: the Center here, the Princeton institute, and North Carolina. There were three European institutes.

34-00:22:29

Rubens:

The three US were social science, basically, humanities—

34-00:22:35

Smelser:

Humanities was the North Carolina one, and Princeton, while it had a social science component, was mostly physical sciences and math. They were specialized, but nonetheless we had similar formats and a natural grouping of people. The European ones were the Wissenschaftskolleg in Berlin, NIAS, the Dutch National Institute for Advanced Study in Social Sciences near Rotterdam, and then the third one was the one in Sweden, Uppsala Scandinavian Center for Social Science. There were a number of them scattered around later that never were in it—well, they've been expanded slightly. Radcliffe is now in it, and I think the Budapest Center for Advanced Study is in it now. It expanded after I left. We met once a year. We didn't have too much function. We did have a fellowship we gave out to a European scholar, honorary one. We had some money for it and did that. That was about our own real duty. It was a group of mutual support, information exchanging, socializing. We'd travel to one of the centers each year, so I went to all these different centers and hosted it once. Every six years, it comes back to one center. I hosted it one year at Palo Alto. Just a wonderful time. Took them down to Monterey and showed them the area, and we did some business.

34-00:24:19

Rubens:

So they were productive—

34-00:24:20

Smelser:

Oh, yes. We exchanged valuable information for each other as to problems that developed. We had a joke. We said we're going to have a policy called fellowshipping. Fellowshipping meant that we were each going to be given the opportunity to choose three fellows that we hated and ship them to another center. It was our own little bit of psychotherapy for each other with our difficult fellows.

34-00:24:54

Rubens:

What would have been the occasion of hating someone? Personality?

34-00:24:58

Smelser:

Demandingness. There were always a few fellows who wanted everything. Taxi rides paid for to come to the Center every day. The diets were a huge issue in the Center. It drove the cook crazy because everyone had something special they wouldn't eat or would eat. There is this phenomenon which I've described in my own writings of the transition from privilege to right to entitlement. Once the fellow was there, they'd nudge around about a little more salary for this, for housing help. You always just imagined a number of special things people could dream up that they wanted. There was always a subclass of fellows in it, particularly a kind of black list that the staff kept, because a lot of these demands would hit the staff more than it would hit me. But people would come and ask me to try to get them jobs at places, in different universities. They thought I had some kind of magic. Parking place

at the Getty in Los Angeles. They thought I could get parking places for them to go to the Getty. All kinds of things would come up. There was an undercurrent of some fellows being a nuisance.

34-00:26:14

Rubens: Maybe we should calculate what we cover next. How much time do we have left?

34-00:26:19

McIntosh: We're almost at thirty minutes.

34-00:26:21

Smelser: Why don't we talk about the *Encyclopedia* I think we can go on about the Center, but I think I've given you a good flavor.

34-00:26:23

Rubens: We can talk about the Guggenheim later, too.

34-00:26:27

Smelser: Yes, those different involvements. So, the *Encyclopedia*. *The Encyclopedia of the Social Sciences* was a standing institution, though it hadn't been done by the same people. The first one was published in 1933, with an economist and a political scientist and an editor. It had a kind of standing reputation of being a first-class publication. I think Macmillan published it. Then, just about thirty years later, the stirring came up in the 1960s that we needed another one. It was out of date. I believe, again, the Social Science Research Council took an active lead in it, but the publisher—I have the copy upstairs. It was Macmillan. The first was about twelve volumes. The second was about seventeen volumes. I contributed one article to the 1968 edition on economic sociology, and then three years later I wrote a review—I think we've talked about this—of the entire encyclopedia for social forces. Kind of an intellectual enterprise on my own.

Then, in the 1990s, there began to be stirrings for a third issue. The logic, never articulated, was that every thirty-three years, there should be a new encyclopedia. This was a sort of completely informal understanding. No one planned it out. Publishers began to take an interest in this. In fact, when I was a fellow at the Russell Sage Foundation, we had a meeting with Bob Merton and several other scholars on the feasibility of another encyclopedia, to be published by the Free Press. Agents from the Free Press came—editors. We had a meeting at the Russell Sage Foundation, and everybody was enthusiastic about the idea in 1990. But as it turned out, a number of publishers were thinking about it, but none of them were willing to undertake the capital investment. It's a huge enterprise of a lot of upfront money on a project you're not going to know about the outcomes of. So it took a publisher like Elsevier, which had a huge capital base—it's one of the two biggest publishers in the world, I think. The other one is in Germany, Bertelsmann. And Elsevier became a huge enterprise. Elsevier was originally a rather modest Dutch

academic publisher, but it grew and grew and grew. Got into a lot of journals, a lot of texts. It's a huge enterprise. The executive office is in Holland, but its main publication office was north of Oxford.

Well, they had gotten the idea that they might want to publish—and they had gotten into enough planning, they indicated that they wanted it to be, in a way, a super encyclopedia, twenty-six volumes, five thousand entries, and to bring forward the obviously antiquated knowledge as of 1968. The way it worked out, the planning of it, was they came to see me at the Center almost immediately after I was elected director. In the first year, I believe it was, or second, at most. A couple of high-level editors came to me and asked me not to be editor, but to host a conference of maybe—a meeting, really, not a conference—of maybe seven or eight leading American scholars in the social sciences on the feasibility of publishing an encyclopedia. I said I would be happy to do that. No talk of who's going to edit it at that point. I said I'd be very happy to do that. They kind of hinted I should pay for it. I said, well, that won't be the case. You finance the meeting. They immediately said okay. So we had a gathering of the head of the Social Science Research Council, I had Gardner Lindzey in on it. I decided to make it interdisciplinary, so I had people scattered in different disciplines. All statesmen, you might say, in their own fields. A couple of women were there.

34-00:31:02

Rubens:

The purpose of the conference is to?

34-00:31:05

Smelser:

Feasibility. Is this a good idea? Are we still in the world of encyclopedias or not? This kind of thing. And if so, what it should be like. Open-ended. But the idea was to get a good feeling from these scholars as to whether it's time for it or not. It was a publisher's way of getting good input, mostly. We had this meeting in Stanford. It took about ten minutes for these people to come to an agreement it would be a super idea to have a new encyclopedia. The entire meeting was basically engaged in hypothetical planning. I chaired the meeting, because I was the host. Not that we were designing an encyclopedia, but that's where these people's minds turned to. How can such a thing be organized? What should be the basis of it? What should be the main theme of this sort of thing? Should it have biographies or not? A lot of operational details.

Everybody left after this. They didn't come to a decision. I constantly said we had to get Europeans involved if we were going to think of an international encyclopedia at this time in history. Couldn't be an American publication the way the first two were basically American publications. I said to the people at Elsevier, we can't have that. If you're really interested in feasibility, you ought to have a second meeting, and it should be made up of Europeans. That was my advice that I left them with. They said, okay, but would you come to that meeting? So I said yes. It was going to be in Uppsala. The director of the

Uppsala institute hosted it. I said yes, I'd come to the meeting. Meantime, I helped compile a list of people who would come to the meeting, and among those people was Paul Baltes, the director of the Max Planck Center in Berlin on human development. He was a psychologist. Outstanding international psychologist. I said, you should invite Paul Baltes there, because he's one of the scholars who would be most knowledgeable and thoughtful. I didn't know Baltes very well, but I knew him by reputation very well. Gardner Lindzey was a close friend of his, and so Gardner said yeah, Paul's a good choice. But Paul declined the invitation.

We had this meeting in Uppsala, but one month before the meeting, they asked me to be editor of the encyclopedia, because they had made their mind up, but they said, sure, it's good to have this European meeting as well. But they invited me to be editor, and I didn't accept at the time. I didn't un-accept. I said I don't want to decide on this yet. So we had this European meeting. The outcome of the European meeting was much the same as the American meeting. Everyone was enthusiastic about it. They liked the idea. They had further input and so on. It lasted a day or two, this meeting, and it was all positive, and here I was with this invitation to be editor. The main complications in my mind at that time were three. Do I want to do it? Encyclopedias aren't the most rewarded things in the world. They're kind of like editing a journal. They're positive signs in your career. Even a monster enterprise like this isn't like an original scholarly contribution. There was always this little bit of ambivalence, kind of like editing a journal. It's a plus, but it's a little bit tinged with, is it at the center of your real, solid, true reputation as an academic? So I had this hanging behind me. I also had the idea of the time that would be involved and the very delicate issue of how to deal with the trustees on this issue, because I was fulltime Center director at that time. Third, when I finally accepted, I said, I will not accept unless I have a European coeditor. That had to be. We couldn't properly claim to be an international encyclopedia if we didn't have it, and they agreed.

I resolved the encyclopedia issue. I said, okay, I'll do it. I resolved the third issue, the coeditor, by telling Elsevier I wanted Baltes. I combed over maybe ten European scholars in my own mind and I ended on Baltes as being the best. They said, but he didn't come to our meeting in Uppsala. What's his problem about an encyclopedia? Elsevier said that to me. I said, it doesn't mean anything. He didn't want to come to a planning meeting. Let's approach him anyway, or I'd like to approach him anyway. They accepted my judgment, and I then contacted Paul and we had a long series of conversations. He finally agreed to do it. He's a gracious man. He said, "I'll do it only on the condition that we list you as senior editor," meaning I had taken the lead and so on. Alphabetically, he would be first, but he said, "I want it the other way." He already had this kind of cooperative mode in his own mind.

34-00:36:30

Rubens: He was a younger man as well.

34-00:36:32

Smelser: Ten years younger than I. He's now dead, but he was ten years younger than I. Not that that truly made a difference. We were both senior scholars in the field. We both had international reputations. That was the main thing. I wanted someone who was really widely respected and good in his own career. The Center board, I dealt with very, very straightforwardly and said to them before I accepted that I wanted to make sure the board knew all about this and that the board was comfortable in my undertaking this. I did tell them it was a tremendous significance to the Center. That it reinforced our absolute centrality to leadership in the social sciences to have this happen. I did give them this little bit of a sales talk. I expressed confidence that I could carry out my responsibilities for the Center and take on the editorship. The board was queasy. The majority seemed—if you want to do it, do it. There was a minority who thought it was going to hurt the Center if I did it, in terms just of my own commitment.

34-00:37:53

Rubens: This was only within two years of your—

34-00:37:55

Smelser: Yes, it was in the second year that I had this conversation with the Center.

34-00:37:58

Rubens: You didn't have to prove yourself?

34-00:38:00

Smelser: Well, I don't know. I wasn't unpopular in any way with the board, but they were worried about, could I do this? Could this come off? Would I neglect things like fundraising? Things that were absolutely essential that I do. Three or four members expressed reservations. Some opposition. Eric Wanner, director of the Russell Sage Foundation, who was a board member of the Center board, finally, after the end of this discussion, which was not hostile—it was reflective, but it was mixed—Eric Wanner said, “If Neil thinks he can do it, let him do it.” That sort of capped the whole thing off—the board couldn't resist it. He said, “If you want to be dictators, tell him not to do it. But if he thinks he can do it and can still run the Center, let's take this chance.” So that ended it, and it ended in a benign way. I will, however, tell you that at every board meeting thereafter, I would open the board meeting by talking to them about the progress of the encyclopedia, what was going on, what stage we were at. I didn't tell them I was spending too much time on it. I didn't bring that issue up again. I kept them really, really informed about the encyclopedia.

So that was a prelude to the whole thing. Baltes and I more or less went immediately to work. I organized the board to approve Baltes to come back for a third year. He wanted to come back to the Center for a third year. He had

been there twice already. That year was the key planning year. We started planning more or less after we cemented this contract with Elsevier. It was the organizing year, and I want to get to that, but I just want to say that Paul was a more hardheaded guy than I in this whole process, in terms of dealing with Elsevier, which is, of course, a huge capitalist enterprise. We later had trouble with them. He was the one who wanted to insist on certain features of the contract about what they would do by way of processing the whole thing. Good division of labor. He was right. I wasn't going to neglect these issues, but he was the one who really wanted to say, we've got to have it. They've got to nag the authors. We're not going to nag the authors. That kind of thing. He insisted on a lot of these details. We got their agreement on it, and that proved to be a very, very valuable set of assets for us, because we could do the intellectual work, and the staff work was more or less delegated to them and to my own staff and to Paul's own staff that they financed for it.

Anyway, Paul and I started planning on its content, organization, and architecture, if you call it that, more or less immediately. We held two meetings, one in the United States, one in Europe, to get guidance on this matter. How were we going to divide it up? Are we going to just say, this is how we're going to do it, according to the academic disciplines? We said immediately we can't do that. They were giving us associate editors, called section editors, for different divisions that we would be superintendent of. They were going to go up to forty of these associate editors for the different—according to our needs. We said how are we going to organize this thing? We knew immediately that disciplines wouldn't do it. That there was a lot going on in the world that was not covered by the standard disciplines. How are we going to extend this list? Paul and I put our heads together. I did a lot of thinking about this. We came up with the idea, yes, we'll have a standard list of disciplines that we know are in the social and behavioral sciences. The usual ones. Economics, sociology, anthropology, and so on. We added philosophy, history, psychiatry—it wasn't psychiatry. It was medical studies. Law. In other words, things that aren't normally considered social sciences, but have enough social sciences in them to be disciplines that are relevant. We ended up with fifteen of those.

Then we said we've got interdisciplinary areas, too, that we can't assign to a given discipline. We're going to have editors for each of these. Gender studies, religious studies. We classified education as one of them; it had enough social sciences in it. We came up with about eight or ten of these interdisciplinary—public policy studies, things of that sort—that didn't fit in any given of the social sciences but had representation from lines of research in many of them. We called these intersecting areas. We went on to say there are some issues that apply to all the social sciences. We're going to have biographies in this. We decided, finally, after a huge debate, to have a limited number of biographies, all people who were dead. That was a very big decision on our part, not to have any living people's biographies. So we had biographies. We had statistics, we put in methods, several methods,

comparative methods. We put history of the behavioral and social sciences. These things that overlapped all of them, so about six more. Methodological mostly. We had a section on social sciences as institutions in universities and other places. This was still a third. We called them overarching issues. We then had a section, which was miscellaneous, on applied social science there were four or five other—urban studies. We had ended up with nearly forty subheadings, which kind of cut across each other. At the same time, it had to be multiple in character, because we couldn't exhaust them by any one of the principles that we had in mind. It was a very complicated architecture that we figured out.

We used advisors on this a lot. We consulted widely. We had a couple of meetings on the organization, meantime working one-on-one before the organizational meeting. Once we got that organization in place, we had to say, how about the section editors? That's a very important role, because each of them were going to have between 150 and 200 individual entries that they had to superintend—to house the authors for, in the first instance, and superintend. We had to form an international advisory body, which turned out to be almost a hundred scholars from the whole world. We had to assemble that and choose it, and decide what their role was going to be in the encyclopedia. These were the major planning decisions that Paul and I had to make, and we involved a lot of help. Once we got the section headings worked out, we had to decide on and approach editors who would take on that kind of responsibility, which was not necessarily an automatic yes on a thing like an encyclopedia.

34-00:45:42

Rubens:

There would be forty section editors? That's huge.

34-00:45:54

Smelser:

Forty. Yes, that's how it turned out to be. We had forty section editors. Then we had to recruit them. Paul and I decided, for the forty sections, we would divide them in half. He's a psychologist. We just simply, arbitrarily, did a lot of the division into two parts, and we would be the coordinators with twenty editors each, though we would each read the whole thing. Both of us would read the whole thing as it came in. But we had this initial administrative responsibility for that number of editors. We had one mass meeting of editors at the Center. Nearly drove Julie, my secretary-assistant, Julie Schumacher crazy, because we had to put all these people up, transport them. The usual thing of a huge conference. We had this meeting of these forty people. Almost all of them came. Paul and I were able to get our first choice in almost every case. We just simply used personal persuasion to bring them on. I had known a lot of my people before, and a lot of it was a matter of continuing a personal relationship. A couple of them said they wouldn't have taken it unless I'd asked them. That was sort of a networking that we had.

34-00:47:22

Rubens:

There was some stipend for this?

34-00:47:23

Smelser:

Ten thousand dollars per section editor. If we had two of them—some of them said, I'll join, but I want a second section editor. They were able to bring them on if they wanted a coeditor. Maybe ten of them or so wanted coeditors for their work. We had a mass meeting of these editors. It turned out to be a real delicate operation, because in the beginning there were a lot of major issues that had to be sorted out. There were a couple of disciplinary fights. There was an anthropologist who was really very softheaded, who was the anthropology editor, a Swedish anthropologist, and the statistics editor, got into a fight. Really kind of basically, what are the social sciences all about? Public fight early in the meeting of these people. We had to kind of throw water on that, talk to these guys about that, it wasn't what we're here for and so on, privately. One of the editors said, "How are you going to make this truly international? How are you going to get China involved in this? How are you going to do this?" He really gave us a terrific hard time about how we're going to internationalize it properly. In the end, this guy, who was from the University of Chicago, chose almost all of his individual contributors from the faculty of the University of Chicago. I got a laugh out of that. We had to tell him to diversify. In the end, these big square one issues got variously resolved early in the meeting.

Most of the meeting was then taken up by individual meetings between scholars, deciding how they would chop up their fields without overlapping with each other. A person who was in charge of women's studies, feminist—it was called gender and women's studies—would meet with the sociologist or the economist or the others—the historian—and they said, we realize we have research in our own field that deals with this, but we'll decide—trying to draw boundaries. It was almost a kind of horse trading. Most of the editors were happy to get rid of certain sub-aspects of their own field, because then they could concentrate on what they wanted. It didn't turn out to be territorial or possessive. I want this aspect, I want that aspect. I'd call it a trading or a negotiating out. Because overlapping in that encyclopedia was an enormous issue. It was a huge issue. This was an extremely helpful early phase of working out that whole issue on overlappingness.

34-00:50:19

McIntosh:

So it was the associate editors who would go and draft up a tentative plan of what entries they wanted? Or would they consult with you about that?

34-00:50:29

Smelser:

They would do both. We had divided the fields up according to what we saw was their breadth. We divided them up and said, you get fifty entries, you get a hundred entries, you get 150 entries, and you get 200 entries. We gave 200 entries to all the major disciplines, and then went on from there. Religious studies, I seem to recall, was fifty entries. We just made an ad hoc decision, a partially informed decision, as to how central or how extensive was the research in these areas, and totally laid out the numbers to each of the associate editors.

34-00:51:08

Rubens: Was there also a maximum length?

34-00:51:10

Smelser:

Two hundred. Oh, length. They could choose what they wanted. Anywhere between 2,000 and 5,000 words per entry, and they could work it out. The economist didn't take his whole 200. He decided he wanted to concentrate it into fewer, longer essays, with the upper limit observed. The political scientist editor wanted to cover the—he had completely 200 and shorter essays. So we gave them that latitude. Length was dictated, in the first instance, by the publisher, but then we worked out the flexible numbers of entries for each sub-field and length for each sub-field. They were constrained from the very beginning. We couldn't sort of say, this is going to be forty volumes. They had that in mind and they dictated it. The publishers dictated the overall length of it.

Then we said to each section editor, make yourself a list and make yourself a number of contributing authors, and send them to us. Paul and I reviewed our respective halves. We were active in reviewing the lists of everybody. We sent the lists of choices of authors and topics to the international advisory board, getting feedback from them on things that hadn't been covered, things that were given too much emphasis, et cetera. We pooled all of these things together and negotiated out a final list for each of the section editors. Lists of topics and lists of authors.

34-00:52:59

Rubens: The data management was monstrous. Are you working on an Excel spreadsheet for this?

34-00:53:06

Smelser:

I had a staff to keep everything going, what was going on, so I was able to call up anything at any given time. I hired Julie Schumacher 70 percent of her time. They gave us an administrative budget of \$75,000 a year. I used that administrative budget mainly to hire Center staff members to do different things for me. I hired the Center editor to do a little research on the emphases and content of the earlier encyclopedia, for example, for my own information. Knowing where we were in relation to the earlier encyclopedia and so on. I didn't have a spreadsheet, but I had the equivalent in terms of people who could supply me with this information at an instant's notice.

34-00:53:48

Rubens: Was a lot of this being done after hours?

34-00:53:58

Smelser:

Mostly during, because I had my staff there. It was absolutely helpful that my staff was on call. So I did most of it on the job. It was a fragmented part of my life, because I would always turn to this and turn to that and so on. The associate editors were very open, as a rule—section editors was their technical name. They were open to suggestions. I didn't have any fights with any of my

section editors. Once they'd agreed, it turned into a kind of cooperative enterprise, and Paul had the same relation with his cadre of scholars. We had problems in choosing both entries and entry authors in terms of internationalizing it. That was a real issue. We also had gender issues, because in the course of this highly decentralized selection, women were getting to be underrepresented, just because the majority of men—the usual mechanisms were at work. Paul and I devised a way to try to expand this thing internationally and by gender. When they were listing potential authors for their encyclopedia, we asked at least to list four or five authors, send them to us. If you have someone who's kind of tied for first, and one is a woman and one is a man, take the woman. We said if you have someone who's tied for first and one of them is non-European or non-U.S., take them. So we tried very hard to get representation from Asia, from Latin America, and from—Africa was a huge headache. There are a very limited number of scholars who could contribute to it, though we did our best to try to get names of them as well. We just tried to fight the skewing. I'll put it that way. We didn't have quotas. We just tried to get these special mechanisms built into it, to diversify in that way.

The other thing I would say is we decided not to make the entire encyclopedia thematic. Now, this was a very interesting decision. We decided to get just the best of what's going on in all the fields, without worrying about unity, except as it was expressed in the overall architecture. This was very interesting, because midway in the project, which took five years, I met with David Sills at a meeting of the Social Science Research Council. It was a social meeting. It was a party connected with the Social Science Research Council in New York. Sills came up to me. He was the editor-in-chief of the 1968 encyclopedia. Of course, we were brothers under the skin, for this very reason that we were both encyclopedia editors. One of the first questions he asked me, he said, "What's your theme?" Well, the point is, he had an idea of a theme. It was the dominant, you might say functionalist, empiricist view of the social sciences as of 1968. Parsons was involved heavily. Merton was really directing the thing behind the scenes in my reading of that encyclopedia, because Sills was almost a junior to Merton. They really did have a theme. Not everything went into that theme, because economics couldn't be classified under that, or psychology or whatever, but they wanted to make that kind of dominant emphasis. As a matter of fact, I picked that up in my own review, and raised the question about the degree to which it was reflective of sociology as of that moment, or social sciences as of that moment, and discussed this whole thematic thing in my long review for *Social Forces*. But Paul and I decided differently. We took a much more catholic approach—small "C"—and we decided just to go after topics that we felt were not the strongest, you might say. We did take a somewhat more representative view. I don't think we compromised the quality of the encyclopedia, but we decided that the social sciences simply could not lend themselves to that systematizing as of the year 2000.

34-00:58:31

McIntosh:

It's a tradeoff, it would seem, between Sills' way of doing things and your way of doing things. One allows for more thematic continuity, and the other allows for more breadth, obviously.

34-00:58:45

Smelser:

I discussed this issue in the introduction as well. I did, actually, more of the authoring of the introduction than Paul Baltes did. He did some parts of it. I took the intellectual leadership in drafting the long preface to the encyclopedia.

34-00:59:04

McIntosh:

We're basically at the end of the tape at this point, and so we'll have to resume talking about this next time. I am curious about the addition of philosophy to the encyclopedia and what you saw that as contributing.

34-00:59:21

Smelser:

There is such a thing as a philosophy of social sciences. That is one sub-field in philosophy. We also decided that, in fact, a lot of the substantive issues in the social sciences had philosophical roots and that we wanted to make sure that philosophy was represented. We had several philosophers in the biographies, and we had several articles on classical philosophy, hoping that they would yield more emphasis on the social side of things. We didn't have a problem about including philosophy. We had an issue with, interestingly, one of the members of the international advisory board, Jurgen Habermas. He saw the philosophy list and he said, "This is not philosophy. This is analytic philosophy of the Anglo-Saxon variety, and you've left out continental philosophy altogether," which was his philosophy, "and if you don't reconstitute this list, I'm going to resign publicly from your advisory board." He really laid it to Paul, and of course Paul—they're both German—Paul was really in a snit about this sort of thing. We simply said, let's appoint a coeditor. We said that to the already-appointed editor, who was, in fact—most of his entries were analytic philosophy from the Anglo-American tradition. He was an Australian, but he was in that genre. We simply said to him, look, we've got this situation. Are you willing to have a coeditor? He said fine.

34-01:01:01

McIntosh:

Problem solved just like that?

34-01:01:03

Smelser:

The problem was solved. Habermas immediately said fine. We basically gave in to Habermas but it turned out to be right. He was right in this fight. We joined his side just—

34-01:01:18

Rubens:

Did he become the coeditor?

34-01:01:20

Smelser:

No, no, no, no. This was some European scholar that we picked.

34-01:01:26

McIntosh:

I think we're going to have to wrap up on that episode there and resume next time.

34-01:01:30

Smelser:

Okay, very good.

Interview #18 August 26, 2011

[Begin Audio File 35]

35-00:00:00

Rubens:

Hi, Neil. It's Friday, August 26th. We're here with Jess McIntosh and it's the eighteenth interview. Last time we ended with your detailed account of taking on the editorship of and creating an intellectual architecture for the encyclopedia. Before we take talk more about the encyclopedia, I want to make sure we talk about a couple of things we've skimmed over in the chronology of these interviews. For instance, the fifth edition of your sociology text with Prentice Hall. Each edition I think you revised. You had been working on this the last year or two before you left Berkeley and came to the Center. Do you want to make some comments about it particularly?

35-00:00:38

Smelser:

Yes. That text, *Sociology*, appeared in the early eighties, its first edition, and I think I indicated in an earlier comment it was quite successful. It picked up a certain niche in the upper level institutions for introducing sociology for those instructors who wanted to use a text. It had that kind of level. It was not much used, I believe, in the community colleges because its level was too high, really, for most introductory courses in those institutions. However, it sold well enough that Prentice Hall just kept saying to me, "We would like to help with the next edition." Two or three years was the interval between these additions. And each time I undertook an addition, I cannot estimate the percentage of the book that was rewritten or revised. It was significant. They supplied me always with a research assistant. I sent this research assistant in directions I knew in which ideas or new lines of research were developing and I then incorporated them back into the book and also revised in accordance with my own, whatever, evolution of views I had for other parts of the book. So I would say it was not just a boilerplate re-edition.

And in the course of this, I began to get tired of it. Your sense of creativity withers to almost nothing when you're dealing with this. You like to do a craftsman like job. That's about it. But you don't get this new sense of discovery and so that's why absolutely I knew in my heart of hearts I was never going to undertake a re-edition of the encyclopedia. It just wouldn't have any of the—I decided on the encyclopedia I'm not going to be involved in the second edition just because I had this experience maybe in the text of running out of enthusiasm and love for the project. But anyway, the last edition, and I told them at the fifth, "This is the last, my friends. If you want to get it done, you get someone else to be listed as a co-author and let them do the work," right. So I knew that I had run out of steam but I was willing to do that fifth edition. I can't tell how much time it took from my life. I didn't make a calculation of this sort. But I went through the usual revision for that last edition.

35-00:03:12

Rubens:

This is while you were in the president's office?

35-00:03:17

Smelser:

Let's see. 1993-94? I guess it was. It came out in '94. Yes, it had to be. And so I had to do it with my left hand. Like all the revisions, I had to do it with my left hand and a certain level of resentment that I wasn't doing something more intrinsically gratifying. But nonetheless, it was a successful book. Not many texts go through that many re-editions or get that kind of a—and it had a translation into Italian, in which—it was an unusual translation in that the translator took a very aggressive stance and actually would introduce empirical materials from Italian society into the different sections. I didn't do this, the translator did it. And I approved of it. I said, "This is fine. Why not make it relevant to the Italian audience rather than having mostly American or at best European references?" And so that was, in a way, more than a translation. It was kind of a re-edition. I never expected exactly what the—how it changed in its character. The theoretical framework and the chapters all remained the same. He just elaborated it to make it more relevant to Italians.

Then, later in the nineties, I got this request from my friend Yadov in the Soviet Union, the Russian Academy of Sciences at the time—who wanted to bring out a translation. They chose the third edition. They were a little bit behind time but nonetheless, they chose the third edition. So I gave my approval to that and I agreed to write a special introduction, which I did, to the Russian edition. I tied it in with the transition that Russia was going through and how they were looking outward and so on. I made an appropriate time for a text to be put—it was the first text to be translated into Russian since Ogburn and Nimkoff in the sixties. They had not gone into representing American sociology particularly even though a lot of the scholars in Russia, as I discovered, keep track of American social sciences. The book was published. I do not know how many hundreds of thousands of copies they printed because there was still this kind of mandatory centralized text assignment.

And there was a little question of royalties, of Russian royalties. And Yadov, who was in the middle of the Academy, who was handling this, said to me, "We can't possibly pay you the kind of royalties that this deserves. We offer you a lump sum of \$3,000 for translation into Russian." Then he added, "Would you care to give a donation of \$3,000 to the Russian Academy of Sciences?" It came right along with it. [laughter]

35-00:06:32

Rubens:

Prentice Hall isn't brokering this? This is—

35-00:06:34

Smelser:

No. All this was directly with me. I think I had the copyright, I'm not sure, but anyway, the negotiation carried on with me. They went through Prentice Hall bureaucracy and got it. It was internationally legal. It wasn't pirated. So I thought about that and I said, "Look, I'll give you half. I'll give you \$1,500.

Appreciate this being translated into Russian and so on, so I'll do that." He said, "Fine, that's good." Then I got another communication. It said, "We are having trouble getting this money into the United States. We have a dollar problem," right, international currency. So how it worked out was some Russian, some of his buddies, brought \$1,500 in cash to the United States, deposited it in some Michigan bank and the Michigan bank sent me a check for \$1,500. It almost seemed like you were going through the Caymans or some other international laundering kind of operation. But anyway, I did get a some royalties from this book.

35-00:07:38

Rubens: That's a good story.

35-00:07:40

McIntosh: Seems like such a pittance, though, for the amount of copies that were—

35-00:07:43

Smelser: Oh, it was nothing. It was basically a token, a complete token. Well, they didn't have the money. The country is broke, right. That Russian edition, I later learned, was pirated and published in Mongolian, I talked about that earlier. And it was used as kind of the national text, if there's such a thing as a national university system in Mongolia. It was used as the text and very recently a faculty member from Mongolia National University looked me up here in Berkeley and we had a session together down at the coffee shop, La Strada, and he was telling me that every student in Mongolia now knows sociology through my work.

35-00:08:24

Rubens: And do you assume it was the third edition that had been translated into Russian?

35-00:08:27

Smelser: Or I'd say just picked up the Russian and somebody translated it. Russian is kind of a second language in Mongolia. Made sense that it got translated out of the Russian. But anyway, I'm sort of proud of that text, though I can't say that it involved as much of my libido as many, many other projects.

35-00:08:46

Rubens: As we get back to the encyclopedia, I read that in your introduction you note that Gardner Lindzey came to all of the planning sessions for the encyclopedia. And it reminded me to ask if Gardner Lindzey was also a presence continuously at the Center, if you care to comment on that.

35-00:09:21

Smelser: Yes, Gardner Lindzey was a—in a way, his name is synonymous with the Center. He came in as its third director. I don't know the exact date. Maybe about 1970, maybe, a little bit before. In that period. It had been in existence for fifteen years and it had two directors before. He actually turned the thing into a much larger scale operation. He was successful in getting the federal

government through the National Science Foundation and helping support the Center. He was very entrepreneurial in generating special projects. He had an almost corporate style. He was in Washington giving lunches to potential donors all the time and going to different foundations. He was much more active in that regard than I was. In fact, he probably set up a network system that I was beneficiary of. He and I, I think you know, had a lifelong closeness and I have often said that I owe half of my career to Gardner Lindzey because he was forever getting me on national committees, boards, foundations, linkages, supporting me, trying to get me to contribute to his own *Handbook of Social Psychology*, which I never did because I just didn't—other things were going on. I didn't want to.

He offered me the associate directorship of the Center with kind of the assurance that I would be director after he was, which I declined because I wasn't in the mood to do that earlier in my career. I was about fifty years old when that happened, I think, and I was not going to take on that kind of assignment at the time. And he was very supportive of me during the time I was being considered to become director. He was my biggest fan. And when I was appointed by the board, he called me up. We went out to dinner in San Francisco. It was all a very big celebration and so on.

Gardner Lindzey remained on the Center board after he was director of the Center. An extremely powerful force because of his knowledge and past. And he remained on the board during my entire time as director and he also secured an office at the Center which he would come to every day. And he was physically present and he would come to my office all the time because we had this close friendship. Generally speaking, he was about as gracious as you can imagine with respect to giving me the kind of room to move and did not really criticize me at all really. Except that I learned over time that this man had his own ideas about what the Center should be and it was a model of how he ran it. I made some decisions that were not popular with him, and he groused about them, not to me but to others and then word kind of creeps back to you. So there was the beginning of some cracks in our relationship even though they didn't surface into the open.

He also had the idea he wanted to be a board member for life. It was part of his expectation. I always said he loved the Center too much. In other words, he personally became identified with it. And there was a certain moment, and this was an open tension, at which Gardner wanted me to put forward in effect a lifetime appointment for him and get the board to approve. This was when I was director. And I hesitated and I said, "Gardner, the best I can do is to ask the board to give you a three-year renewal." Because he'd already been on the board for life practically and I said, "This is what we've got to do." And he didn't like it. And when I went to the board meeting to put forth this proposal to the board, which I thought was a favor to him, he showed up at the board meeting. I thought, "This is totally and completely impossible for him to be present." So I actually, before the board meeting opened, I went over to

Gardner. I said, “Gardner, you can’t be here.” I didn’t say conflict of interest but I said that’s really kind of—“You’ve got to excuse yourself.” And he stormed out, went to his car, drove wildly down the hill, I was told, and was in a state of real fury at me for this. And later we had a conversation and I explained to him as openly and as carefully as possible exactly what I saw the contours were and what I was—in fact, this should be regarded as a positive action on my part and not a punitive one. And after this conversation he said, “I think you’re right.” And, again, graciously.

But nonetheless, you can see how the seams are cracking. I vowed to myself I would never have an office at the Center after. Just walk away. And it’s kind of my working philosophy that once you’re finished with something you walk away. You don’t meddle; you don’t fool around with it. It’s other people’s business. I didn’t like to be meddled with and nobody else likes to be meddled with. If they’re running the show, they should run the show. That’s my whole view. But anyway, this was bad because later on, as Gardner fell ill, this was even after my—he lived through my directorship and three years later. But he fell ill. Had a long period of complete disability and I feel disappointed that I didn’t rejoin him because of this somewhat soured element that had gotten into our relationship. I don’t think I did what I should to a man who had been such a lifetime friend. So I carry this kind of burden of sadness and guilt about him because he was such a major figure in my own life.

35-00:16:02

Rubens:

Was he an intellectual presence at the Center? Did he attend the lunches?

35-00:16:06

Smelser:

Oh, yes. Gardner was a very—he was an outstanding psychologist. There was no question about that. And he was a member of the National Academy of Sciences. Even though his work—you would not call him an innovative creator of ideas in the field. Back to the *Encyclopedia*. I was probably more active than Paul just because it’s kind of my style to try to make order out of a lot of chaos. I really considered that to be a terrific intellectual achievement and to convince people to take assignments based on our definition of what the whole enterprise was about. So even though the editor of the encyclopedia is kind of in the category of non-original research—you’re an organizer, you’re a director and there’s some creativity associated with that, but that’s not the most salient part of it. I felt an intellectual excitement about the encyclopedia and its design and a certain gratitude to be orchestrating this. Such a great, complex enterprise and getting all these individuals, who were in fact individualistic in their own tastes and styles and so on, to collaborate in this huge enterprise. So, yes, I would have to say it had an intellectual excitement for me and I still feel that kind of glow.

35-00:21:14

Rubens:

Also in your introduction, I thought there may be a certain maybe defensive posture, particularly about issues of biology and culture. You say that there’s

an incredible amount of ferment at the boundaries of the biological and the behavioral and social sciences.

35-00:21:43

Smelser: Yes.

35-00:21:46

Rubens: I'm wondering if you felt that you really had to be negotiating those boundaries and were there critics in your mind that you were addressing?

35-00:21:54

Smelser: Well, I was, of course, very much aware of the—along with Paul, Paul Baltes, of course, who was more intimately acquainted with developments in neurosciences, and, for example, genetics than I. Even though I wasn't ignorant about these advances. And, of course, I was very familiar with fields like sociobiology, which came out of—truly interdisciplinary but biological in its origin. And I knew personally E.W. Wilson—he's the father of sociobiology—who was in the Society of Fellows with me and I even at that time was much interested in the kind of work he was doing with insects. He's an enormously famous man and also later turned out to be controversial because of some comments he made about race and inheritance and so on. Got into that business.

Anyway, I believe in the introduction that we acknowledged this great ferment. And then also said that there was resistance to it. And I said something I truly believe, that the greater resistance came on the side of the social scientists. Greater resistance to truly integrative thinking is that social scientists had not uniformly but dominantly taken a more defensive view, a more territorial view of this than the biological sciences, many of who were now making these creative formulations about the articulation of genetic and environmental forces, whereas in a way the social scientists were tending to insist more on their own turf. Now, I haven't followed that closely to say that I would still think that way. But that was the kind of perception we wanted to get in there. The keynote of this whole encyclopedia was breadth. So we were pushing on the boundaries, on the physical boundaries of society, on the biological boundaries of society and personality and so we felt inclined to stress this link with the biological.

35-00:24:11

Rubens: Sure. Well, I also wanted to just tie it into the two different, maybe more, projects, special projects at the Center. One was about the biology and genetics of crime and I think another was about educational capacity. And I wondered how you felt about the work of those.

35-00:24:30

Smelser: Yes. I didn't have any especially strong reaction to this. I had followed a number of the earlier projects on primate behavior, for example, and evolutionary psychology, which were earlier special projects when I was on

the board of trustees. I was much interested in those. I would say that, yes, I learned from them. They didn't shape my outlook decisively.

35-00:24:57

Rubens: Or did you think they were onto something or were they a little—

35-00:25:01

Smelser: Oh, yes, valuable, no question. I did not share this—what I described as attitude of defensiveness on the part of social scientists

35-00:25:09

Rubens: And just as a little tangent off but relative to this particular strain of discussion. I wonder if you knew Troy Duster here at the university.

35-00:25:21

Smelser: Oh, yes, he was a colleague in sociology.

35-00:25:22

Rubens: Of course. And so he had written a book called *The Backdoor to Eugenics*. He was someone who was really arguing that this—

35-00:25:32

Smelser: He also had a very strong program, intellectual program in his own mind on genetic counseling. That really was kind of the entry door. And its racial overtones. That was his contribution. Troy and I had a relationship above and beyond that intellectual issue, which I respected very much on his part. Troy was an extremely reasonable man, non-militant, even though he took on a subject and took a stand on it. I saw him as a kind of a catholic individual. He and I had a relationship in the department during my administrative years and you might say my political years as the middle person in the department. Troy was affiliated with the left group. That's the Kornhauser-Blauner group in the department and I considered him the most moderate. And when I was in the chair, when I wanted to communicate with that group, I communicated with Troy. He kind of had a leadership role and he was also a lot more reasonable than these other colleagues that I had trouble talking to just because they tended to be so shrill and intransigent, you might say.

35-00:26:56

Rubens: But you weren't necessarily persuaded that this really was—these kind of sociobiology genetic studies were—

35-00:27:03

Smelser: I didn't take a—I didn't have a strong view on that.

35-00:27:25

McIntosh: I had a question about the encyclopedia. Those major field defining projects, like the encyclopedia, are often lightning rods for nitpicking and criticism from people in the field who don't like a certain entry or who disagree with the way it's structured. How was it received among people in sociology at the time?

35-00:27:48

Smelser:

It didn't create a huge storm of the sort you suggest might be of the people who—what my handbook did—I described the, you might say, parochial reactions to that. I only saw a few reviews. I don't know how to review a project that size. Some reviews by psychologists are the ones that come to mind and they were enormously flattering. For example, this one woman, first of all, she described it as the Egyptian pyramids of the social sciences, which is very flattering. A wonder of the world. That's a little bit irresponsible, I think. But nonetheless, it was in that flavor that she was doing it and she said—I thought she took a critical look at the design, at Paul's and my design. What is left out and what's not left out? And she said to herself, "There are three or four topics I thought weren't going to be there because this outline didn't seem to suggest that they would show up. Then I went to the index and I saw these thirty entries on this one "empty thing" that didn't find its way into the title of a given section but nonetheless got covered." So she said she did all this detective work and uncovered all these entries, so all her misgivings or questions seemed to be answered because the index picked up what wasn't in—you can't cover everything in a given outline. It would be a mile long.

35-00:29:26

Rubens:

Indexes are so useful.

35-00:29:28

Smelser:

By and large, it was very favorably—I'd say favorably perceived. It didn't create any storms. I didn't have to answer huge letters of complaint that someone would write in a journal or anything.

35-00:29:44

Rubens:

I found it interesting that you said wrote that the fields in psychology had so blossomed that it was one of the only categories that had to have three sections. There are 150 entries.

35-00:29:56

Smelser:

Yes. I don't know that if I were editing it myself I would have done that. But Paul persuaded me. He said this field is so large. There was social psychology; social and political and personality and then experimental. It was highly represented and I got a little sense of inequity lingering in my mind. But I have to say that I became an honest convert to Paul's influence on that matter.

35-00:30:27

Rubens:

I noticed that Melanie Klein is under gender studies. When you're listing great thinkers, the biographies, I always thought of her as a psychologist/psychiatrist.

35-00:30:39

Smelser:

Might have been a mistake. I think it was probably a mistake.

- 35-00:30:43
Rubens: Well, she had a lot to do with role theory. It's not so important but indicates—
- 35-00:30:45
Smelser: Oh, there's no question about her theories of identity and so much—I guess in retrospect I might not have classified her, but, again, we had all this bargaining.
- 35-00:30:57
Rubens: Foucault is in psychology not history so I thought that was kind of—anyway, that's neither here nor there. So you didn't get particular critiques over who was in the great thinker's category.
- 35-00:31:14
Smelser: No, we didn't and I'd say that a lot of it had to do with the kind of dynamics of planning. In other words, we fought out some of those battles. In the planning and the organizational stages.
- 35-00:31:25
Rubens: You say that in the introduction.
- 35-00:31:28
Smelser: And while I as editor couldn't track all the individual assignments to categories, in a way, they disappeared because once you had all these people in categories—it disappeared because that listing of the topics was alphabetical. So you didn't know from simply reading the entry what larger editor had taken responsibility for it. I'm not sure how you discovered that Klein was in gender studies.
- 35-00:31:53
Rubens: I just read the list. Now what do you know about how people used it. You noted in your introduction that there was “a galloping industry” for encyclopedias. Most of them are specialized rather than interdisciplinary. Do you have any way of measuring how this was used? It must have cost a pretty penny to get into libraries.
- 35-00:32:37
Smelser: The retail price was \$10,000. That's a big hunk of money really. I'm not sure that everybody paid that amount. The publisher has all these deals and so on. They made a profit before a year was out. They were in the black already. It turned out to be a very successful enterprise. It was picked up, of course, by libraries and my friend John Reed bought a copy for his New York home and then he got word that he didn't have one for his Princeton home and so he got two copies. Anyway, individual purchases were probably not very significant. Mostly institutional.
- 35-00:33:23
Rubens: Is there anything more that you want to ask, Jess, particularly about the intellectual content?

35-00:33:30
McIntosh:

No, no. I think we've covered it. I'm more interested in the compensation aspects. I think you mentioned off camera that there was some issue of royalties.

35-00:33:39
Smelser:

Oh, yes. Okay. The encyclopedia gave Paul and me—nothing secretive about it—\$75,000 a year for three years. Payments in the first three years were advance royalties, right, and there would be royalties on top of that if the sales justified it. Well, it turned out that that amount for us was all we got. Ample pay for me for that enterprise. That was part of the deal. And we got ample administrative support for it and so on. After the encyclopedia came out, it was put on—it was a joint publication. Hard copy. Maybe in the next interview I'll show you and for the viewers of this interview one of the volumes. It was huge, really, and there were twenty-six of them. It was weighting down my shelves. It was going to collapse my study upstairs. But they put it online, as well. And they began selling online versions of it to universities. And then the universities would make it available online to their citizens, like a library book. You could get access to it if—

35-00:35:11
Rubens:

That's how I read it, through UC Berkeley's library. So it is Science Direct that digitizes it, whereas Elsevier was the—

35-00:35:29
Smelser:

Elsevier was the publisher. But they made this deal. Paul and I, in our negotiations with them, wanted royalties on every form of publication. They were charging several thousand dollars to each university or institute or research body that they allowed to have this online. Paul and I noticed this largely because they approached his institute in Berlin, trying to get them to subscribe online. So Paul saw physically that they were trying to make contracts with individual organizations. This raised in his own mind, and mine immediately, that we should have the royalties on this because the contract specified royalties of all descriptions. And they did not bother to include us in any royalties of this kind. Paul had tragically lost his first wife a number of years before and he had remarried and his second wife was a lawyer in Germany. This turned out to be a great asset as we had unpaid legal advice. Because here we were, two authors, two editors dealing with this monstrous international corporation. I felt this was hopeless. "What are we doing?"

And so we raised the question with them. What about these royalties that are not coming our way? And we began to get a little bit of a going around—"See, we can't calculate, you see, because these things are bought in packages." They gave us a lot of mumbo-jumbo about they really couldn't know about these royalties. And Paul and I still wouldn't accept no for an answer. We kept pushing them and pressing them. The only thing we had was that they were eager to have us edit the second edition. They were leaning on us in the meantime, during this whole time, "Won't you do the second

edition?”, which is going to be entirely online. Paul and I made a compact between ourselves, private contract between ourselves saying we didn’t want to do it and that one of us wouldn’t do it if the other wouldn’t which we kept completely secret from Elsevier because that was the only card we had in our hand. So there was this ongoing kind of nagging relationship that went on for about, what, six to eight months maybe and we just kept—wouldn’t give up and we kept getting advice from Paul’s wife, hinting about legal action and so on, that kind of thing. And finally, Elsevier said, “We’ll give you \$100,000 and let’s forget it.” And so Paul and I contacted each other and said, “Let’s take it and run.”

So that’s the way it worked out. Subsequently I saw in a conference in Egypt I’d like to mention the editor with whom I had the administrator in Elsevier with whom I’d had all the nasty correspondence and negotiation. He was completely normal, kind of like it never happened. I’m sure that’s happened to them a hundred times and they settle it in such ways. I don’t know. I was not in the business of getting into lawsuits over such matters and felt, “Okay, we’ll settle for it.”

35-00:39:12
Rubens:

Was there an issue of the textbook being digitized?

35-00:39:15
Smelser:

I don’t think it ever was. Maybe I’ll have another fight if it was. I think it remained in hardcover.

I have one more thing I’d like to say about the encyclopedia if I may. This has to do with the flood of entries that came my way at the end because Paul and I each had 2,000 plus. Between 2,000 and 2,500 entries that came in. I mentioned before that we succeeded in getting Elsevier to do all the nagging and the receiving. So I received all these by email, right, and got them reproduced and I read every one of them.

35-00:40:38
Rubens:

Did you? Wow.

35-00:40:40
Smelser:

Yes. I read every one and I sent back approximately ten percent to the section editors and said, “Look, this doesn’t look right. Would you go back to your author and pass on this request for revision.” And they were remarkably cooperative with me. And it varied by field. I’ve always regarded geography as being a relatively weak field and there were weaker articles in this area. Other fields that I knew, like economics, we had fewer. That’s just the way it’s unfolded. And I thought that was a necessary phase. We just didn’t want to take everything on faith. I wanted to go through these because ultimately I was responsible. I didn’t read Paul’s except on a selective basis. He felt it important that he send me any article that had to do with psychoanalysis, which were in his bailiwick because it’s psychology but he knew that I knew a

lot more about psychoanalysis. So we did some trading. And in some of the fields like biographies and methodology and so on that overlapped, we read both. Paul read it and I read it so we both had input to the section editor.

I had a couple crises I should mention. The question is one of outright rejection once you've contracted with an author to do it. We had almost none of these but I will give you a couple of examples. One was in urban studies. It was written by a Chinese scholar. We'd beat the bushes for scholars in other societies and this came in and it was quite obviously totally unacceptable. It was about one-fifth the length it should be, like scrapped together bits of something. It wasn't very organized. Really, it was like an outline of a term paper for some undergraduate, right. But I had to say that that was the way I read it. And it was in my bailiwick, urban studies. So I simply said, "Let's forget it." I just killed it because I had the impression that it wasn't going to be salvageable. So I just made an executive decision. Never heard anything. Guy just disappeared. He got his little money for writing it and that was it. Gone.

Another one was more controversial. It was in religious studies, which I was also reading. It was from a scholar at Cambridge University. It was in Muslim studies, which we included in the religious section. And it had to do with threads in the history of Islam. We had history of Christianity. The history of the main religions were entries in the encyclopedia. He wrote a flaming essay on how Arabic society would be ruined by the West. He was just totally polemic, a kind of Saddam Hussein talk about how every social problem in the Middle East was traceable to first colonialism and then US imperialism, and so on. It was a propaganda tract. So I consulted with Paul and then I wrote this guy back saying, "I'm very sorry, Professor, that I'm rejecting this article on two grounds. The first ground is it does not live up to the request that we made of you for subject matter. You have not fulfilled the obligation that you undertook by undertaking this field, to describe the historical aspects of Muslim religion. And secondly, the article is too intemperate, too polemic to be considered satisfactory for an encyclopedia." And then I went on and explained our idea about what our own view about the norms of an encyclopedia article are. So I shipped it off and I got back this threat of a lawsuit from this guy at Cambridge. He was a Muslim scholar. He had an appointment in a top university.

35-00:45:10

Rubens:

I assume he was paid, just as the Chinese scholar was.

35-00:45:13

Smelser:

Well, I have to say the pay is nothing. They got \$150 for writing an entry. That was it. Token payment. Money wasn't an issue. We didn't mind losing the money. We didn't even want to talk about the money. But it was a matter of principle and this guy was really sore. I wrote back and said, "I'm very

sorry. I think my position is tenable. If you wish to sue, I cannot prevent you.”
The conflict died there.

35-00:45:42

McIntosh: And his accusation was that your stance was just discriminatory?

35-00:45:49

Smelser: I was part of the problem. I was part of the problem he was describing in his—that this is yet another instance of, “Running this establishment publication, keeping me out,” and so on. It was very a very inflammatory letter and I just took a bet that he wasn’t going to carry it any further.

35-00:46:11

McIntosh: Well, maybe he shouldn’t have agreed to write for an establishment publication in the first place, if that’s his problem.

35-00:46:18

Smelser: Well, I don’t know. We had this big network of choices of authors and the guy’s credentials seemed okay.

35-00:46:27

Rubens: Did you ever encounter him again?

35-00:46:30

Smelser: No. The issue was gone. But anyway, we went through it all. I hired a research assistant to help me do proofreading on the thing. I didn’t get delivery of the copies until after I left the Center.

35-00:46:46

Rubens: You singled out Julia Delius as a science editor who was outstanding in her editing.

35-00:47:00

Smelser: Oh, this was one of Paul’s staff members.

35-00:47:05

Rubens: You mentioned a lot of people. Michelle Williams.

35-00:47:12

Smelser: Michelle Williams was my research assistant that I hired toward the end. A longstanding research assistant on many projects that I had with her. Regarding Julia Delius, Paul gave more executive responsibility to his assistant than I did to anybody. Julia Delius was a part of his staff at his center that he directed and he did a lot of delegation. She’s a super person, so it’s not a criticism when I say that. But that’s when we felt importunate to single her out as a special force in the encyclopedia.

35-00:47:47

Rubens: And I meant to ask you also if there was anyone from Berkeley particularly involved. You mentioned Nelson Polsby.

35-00:47:53

Smelser:

Yes. I got him to serve as political science editor. He and I had had a long and positive relationship here and unhappily I nearly lost him. He had a serious heart attack when he was in Oxford, right in the middle of the encyclopedia and I relied partially—he named Laura Stoker, one of his colleagues here in political science, as a person that I could work with, so I—she pinch hit for him for a lot in his section. But I was very pleased to get him. He's outstanding. Well, he was a close personal friend, too.

35-00:48:31

Rubens:

Anyone else from Berkeley that we should mention?

35-00:48:35

Smelser:

Well, let's see. Meg Conkey was the anthropologist. She was one of the anthropology editors. She was Berkeley. Oh, who's the other one? He will come to mind. It was another anthropologist. He was an archeologist.

35-00:49:06

Rubens:

I was curious why Bourdieu was not one of the biographies. He died in 2002, so that may have been after you were through with the encyclopedia.

35-00:49:24

Smelser:

No, we just didn't choose him. I had one instance that an American—we knew how controversial biographical entries were going to be. Everybody launches a campaign to be in it and even there were campaigns about the dead people. It's symbolic. It was all a symbolic representation of what part of fields are important, choosing these biographies and so on. We had very few of them and maybe eight or ten per field. A 150 in all, I believe, was the total. But I got approached by a friend of one American scholar who was very senior. Over eighty. Pleading for an exception to our non-dead rule. This person was so outstanding. And there was a claim to be made that this person was, in fact, so outstanding.

35-00:50:19

Rubens:

Do you want to say who it is or is that—

35-00:50:20

Smelser:

It was Robert Merton. Robert Merton was not dead by that time so he wasn't in the encyclopedia. I never communicated with him. It was all secondhand. And I just said, "Look, I have to speak to you as the editor. Is if we let in an eighty year old, we're going to get a stampede of other eighty year olds and also a sub-campaign of seventy year olds. There just is a floodgate here. I'm very sorry we have to enforce this rule. Literally, I regret it and there is no question about the deservingness of the person. But it's impossible." And that was accepted.

35-00:51:05

Rubens:

So no flak after the publication particularly about the inclusions. Well, there's a list of those who almost made it.

35-00:51:15

Smelser:

Well, these were all dead, too. So these were people who nearly made it but we felt inclined, because they were, in a way, semifinalists, to mention this to people.

35-00:51:28

Rubens:

This is an aside, but I meant to ask you at some point did you ever meet Bourdieu?

35-00:51:35

Smelser:

Yes. He came to Berkeley once, invited by someone else than I. But there was a little lunch of sociologists, very small group, maybe four met with him in the faculty club for lunch and we had a social lunch. That was about it.

35-00:51:49

Rubens:

Were you at all taken with his construct?

35-00:51:53

Smelser:

Well, I have to tell you, I must enlist my very favorite concept in all of the social sciences: ambivalence. He was a highly original man and some of his writing on cultural capital is positively brilliant. I really just admire that persistence and doggedness with which he wrote. I always had the feeling that his perceptions were skewed by his being French, that there was a peculiarly national feature of it that made him—he wrote it as though it were general laws of the world, this cultural capital and ways people guard social capital, the way they use it, the way they strive and the symbolism of social capital and so on. I felt he overpushed it and that it could not be regarded as a general theory, however brilliant he was in many of his insights. So I sort of had this feeling of being drawn to the man. He did not have a very pleasant personality. Very arrogant kind of person and people—Frenchmen wrote it off from the fact that he was, in fact, from modest background and he entered into this great establishment, elite establishment, and he never felt fully in it, felt snubbed. And so there was a lot of talk about his—some of his writings are a vendetta against academic things. But he himself was a kind of egomaniac. I talk to some of his students from time to time in the International Sociological Association and one of them told me this joke. Said the joke is that the only problem with Bourdieu is that he's forgotten the first half of his name. So that's the kind of knife that was stabbed into him. That's all the comment I have on him. I didn't know him well.

35-00:53:57

Rubens:

Let's change the tape now.

[Begin Audio File 36]

36-00:00:01

Rubens: I hope you don't mind me asking your opinion about some of these important thinkers. You've mentioned Clifford Geertz a few times, firstly because—

36-00:00:1608

Smelser: He was at Harvard with me and he was a student of Parsons.

36-00:00:19

Rubens: And then he was also here at Berkeley.

36-00:00:21

Smelser: Yes, he and I were colleagues at Berkeley for maybe four years. When I first came here, he had just arrived a year or two ago and I immediately established contact with him, along with several other anthropologists. Lloyd Fallers, David Schneider. These were people I knew by reputation and from Harvard. So I had a linkage with the anthropology department here. Cliff and I had quite clearly a lot of intellectual respect for each other. That was quite clear from all the interaction that we had. He and I, our style of our minds pulled in different directions. Despite the fact that I could not be described as a Durkheimian type of positivist, I always had an element—I still have an element in my own method and style of searching for regularities in social patterns and in searching for generalities and a continuous belief in the importance of measurable data as part of a repertoire of evidence that we bring to bear, trying to make things as systematic as possible.

Geertz really was quite a relativist. Coming out of anthropology, he had been much taken by the idea of relativism and how you just don't understand anything without getting into its cultural context and unique patterning, right. And so Geertz made a methodological principle out of this and everything he did was in this genre and everything he didn't like was people trying to make general theories, right. So there was this kind of tension between us nestled into this general sense of mutual respect that we had for each other.

So I don't think I could have collaborated with Cliff on anything because of these, you might say, philosophical differences. And he, of course, made it his whole life style and his whole message to the world during his whole career. He is said to have triggered off, to have been the darling of post-modernism that carried his relativism to a point way beyond anything he ever intended. I know personally that he was kind of disappointed by being put into this category, being a post-modernist before post-modernism.

I had a meeting with Cliff less than a year before his death. It was a dinner meeting at Princeton in which we happened to be sitting next to each other. And so it lasted on for a while. And he and I began talking about Parsons because we both had our own affection for him and we both moved away from Parsons, though, in quite different directions. We didn't converge, we

diverged. But nonetheless, we had this closeness and distance from Talcott and it was one of the most memorable kinds of conversations I've ever had. He opened up, I opened up. And we just could do it because our own personal histories intersected and there wasn't any fighting and there wasn't any enmity.

36-00:03:44

Rubens: So you kept track of his work?

36-00:03:46

Smelser: Oh, yes, and every time I saw him we would fall into conversation. There was no tension between us, although our style—but our styles were fundamentally different.

36-00:03:54

McIntosh: Were his impressions of Parsons similar to yours and were his experiences with him similar to yours?

36-00:04:01

Smelser: He was regarded as much more of a rebel than I. Certainly in the first ten, fifteen years after we were graduate students. I personally believe I went away from Parsons as much as he did, though in ways that were not as recognizable. So that would be the way I'd respond to that.

36-00:04:22

Rubens: All right. So should we move on to your role as president ASA? And particularly I want to get to your presidential address. But why don't we begin with a little background. You had been editor of the association's journal at a very early age.

36-00:04:39

Smelser: Well, the senior Bill Sewell said to me very early in my career, maybe I was no more than thirty-five years old, he said, "You are destined to be president of ASA." I think maybe he was president or had been president just very recently. And there was this kind of expectation that I would and some—it's not a huge issue but some people might ask, "Well, why was your election so late in your career?"

36-00:05:16

Rubens: That's my question.

36-00:05:17

Smelser: So I was sixty-five, sixty-six years old. I can't give you an answer for this but I can give you some reflections on the issue. I'm just going to say in advance this is—I don't have much emotional loading in this issue. It was nice to have been president. I don't feel as though I was cheated in any way or anything like that. But there was a period of almost fifteen years in which the kind of sociology for which Parsons was known was really vilified. Certainly all the way through the seventies and continuing beyond that. It became a kind of stereotype about Parsons' work, that he was a person of his time, that he was

out of date, that he was too general a theorist. General theory was out. That he was too conservative. He was an apologist for American society. All these things. And, of course, I got caught in so far as I was a Parsons student and identified as a functionalist. These things spilled off onto me and they also came out directly in the reactions to my *Theory of Collective Behavior*, which I got kind of assimilated to this reformist and revolutionary view of the field. And so this was a factor.

And some of the people who were getting elected to high office at the time, like Alfred McClung Lee, were outright maverick protesting sociologists. This is what the flavor was. This was the time when James Coleman was vilified as a sociologist because of the research that he did on the effects of integration, of racial integration which he said often aggravated racist situations and so on. And there was a small move to kick him out of the association, right. And he was delayed. He was elected president but he was delayed to when he was maybe sixty years old. So there was this internal politics, I think, that involved kind of the image of me in the field and so on. This sounds like saying I should have been elected but I'm not really saying that. I'm just giving you kind of an account of the dynamics of the field. Of course, the politics of the association reflected this era in that preoccupation with racial minorities and women were in the politics of the ASA. It's, in a way, a leader in this regard and sort of nothing goes on in the ASA without these voices being heard if they have any conceivable interest in the matter.

My election to the ASA was the third time around. The first time it was a very confused election and I, along with five others —this must have been about 1980. I'd been elected vice-president in 1973. In 1980, I was a write-in. It was all write-ins. There were five write-ins and I was one of them, and so nobody got any kind of thing like a majority but the plurality went to Herbert Gans of Columbia University. A worthy, deserving sociologist and I was somewhere in the pack.

Then in about 1992, I was nominated by the nominating committee of ASA to run against Amitai Etzioni, a guy who had been at Berkeley when I first arrived and for whom I had—I didn't have much of a positive relationship with him. I saw him as kind of an opportunist not true intellectual. We didn't like each other very much. He and I ran against each other. He ran a campaign, I didn't. I just sat there waiting for the election to happen. He began promising different groups he was going to advance their interest, like sociologists who were in the federal government. He made a special pitch that you people have been discriminated against by all these academics in the association. "I'm going to see to it that you get better treatment." Similar messages to women. And I was just sitting here out in my little office and waiting for the election. He won by about twenty votes in that election. Now, that was a trigger to the nomination committee that I was a serious candidate.

So the following year, the slate was almost all women, the year after the Etzioni election. It was either one or two years after. I can't remember. And a lot of people didn't like the fact they were kind of loading it up with women, right. So they're just saying there has to be a women president at this time. I didn't have anything against that. But several groups in the association said, "We would like to write you in." And I said, "Please forget it. I'm not going to get caught in this male coming in and pushing that movement to try to out-trump these women." One of them was my colleague here at Berkeley, Arlie Hochschild, colleague and student Arlie Hochschild. So I just said the hell with it, I'm not that eager for the office. I'm not going to get into this kind of infighting in the association.

However, I got nominated a couple of years later, that was in '95, and I won by a very big margin because I was pitted against a somewhat lesser known—much lesser known person and it was by a very big margin that I was elected. But there was this history. It's affected quite a few—Paul Lazarsfeld was not elected until his third try and he was furious. He was very furious about it and he sort of was muttering about anti-Semitism and all kinds of other things that weren't true, but nonetheless—it's a kind of strange dynamic as to who gets into this. And it's a completely popular vote. It's not a committee that—like in history, a committee that really decides—makes the final decision about—

36-00:12:04

Rubens:

And in the history associations, when you're a vice-president, you're already—

36-00:12:06

Smelser:

You go up. Yes. So I guess you can call the ASA a little bit more democratic or more participatory than many professional associations. But anyway, I was elected that year. And I was delighted. I said, "Wonderful. This is a nice thing to happen to me. It's one of my ambitions." In the early eighties I said I really couldn't conceive of anything more I wanted in my career by way of external recognition except the presidency of ASA and election into the National Academy. Those were the two things had hadn't happened to me yet I was eligible for them. And then, of course, they both happened in the early nineties. So I said, "Okay, I've arrived," right. And I knew the dimensions. I had been on the council myself for, I think, nine years, earlier in my career when I was editor of ASA and then I was elected to the council and I was on the council and I was vice-president. So I knew the whole ASA apparatus inside-and-out and it wasn't going to be a big surprise for me. I knew that the role of the president is likely to be a ticklish one because issues come up to the council, loaded issues come up to the council, and you have to super attend them when you're the chair of the ASA. And I knew that this was going to be an episode in which there were going to be some politics reentering my life because I was going to be head of the association for a year and a member of its council for three years. I accepted this as part of the job. Maybe I should

say just a couple of things about an issue or two that occurred during my presidency.

36-00:13:51

Rubens:

And you were on the council for a year, knowing you'll become president?

36-00:13:56

Smelser:

Yes. That was in the cards. And the council was embroiled in a controversy of the following sort that reveals its politics in their entirety. A number of groups in the association leveled an attack against the *American Sociological Review*, saying this is a biased publication, that it has these high falutin' scientific standards that it applies quantitative positivistic approaches and this, in effect, amounts to discrimination against other approaches. Qualitative approaches in particular—kind of fieldwork and so on. And, of course, this got assimilated to racial and gender issues. That these are the styles that these people work with and they're being discriminated by these high minded methodologically sophisticated quantitative types. And race and gender didn't always come up but it was always there. And the groups that were representing the most vocal position on the race and gender front entered this sort of thing and they wanted to start imposing new rules about different sociological styles in the journal, right.

Now, I was very much aware of this conflict, even when I was editor. Do you represent the journal as a banner of science or is it more kind of representative of different styles in the field? I tended to lean toward the heterogeneity but I wasn't doctrinaire about it. But I certainly was not for restricting editors, anything like a quota system of types of approaches or emphases or subterranean politics for the journal. So I had a very strong stand as a part editor that this was a wrong thing to be meddling, when I was going to be president. However, there was a strong and continuous move to impose these rules on editors and have editors, in a way, give their testimony when they were interviewed to be editors as to what their philosophy was about representativeness in the journal and so on. And I was very strongly against that. I said, "You choose editors because they're going to be good editors. You don't impose these systems on them, quotas that inhibit analysis." So I was very strong on that. And all this derived from my own editorial experience. I said, "The only thing you can give an editor is flexibility; you make them editor and you trust them." That's the way I felt about being an editor. There was never any meddling from any quarter. I got complaints about not publishing certain things and I even got a few accusations of bias. But, as I say, that's in the name of the game. But you don't build it into the system.

36-00:16:57

Rubens:

How many were on the council?

36-00:16:58

Smelser:

About twenty.

36-00:17:01

Rubens: And how often did the council meet?

36-00:17:02

Smelser:

Three times a year, I believe, was it. In this one, I don't want to go unduly into this issue but I said, "The council shouldn't really decide this. I think we ought to send this into the editorial committee," one of the major and most powerful committees of the association. Basically I staved off the showdown which came the next year but it was a hot issue. I, actually, as the president of the association, took a fairly strong stand on this issue because I had been editor and I felt strongly about preserving the integrity of the editorship. But as it turned out, informally the publications committee that interviews these editors, they give them a tough time and they ask them what they're going to do. And I think it's an unhealthy feature of the association. But I say it represents the whole politics. So every issue that comes up, this particular group has a feeling about it and wants to be sure it's not some indirect way of discriminating or carrying on some privilege or something of the sort. And that is just the hive of political activity in the whole association.

I never myself got into that. But when I was candidate, I would get these letters from—it was mostly women's group, women's sociology caucuses—asking me about where I stood on this issue and that issue and that issue, and so on, as though they were wanting—they wanted my testimony as a condition to whether implicitly they were going to support you or not. You see, that was the way in which politics in the ASA had evolved.

If you will, let's go back to this riddle of why I wasn't elected earlier in life. Part of it just had to do with that I just didn't fit into the evolving politics of the association. I was too old fashioned, in a way, for that particular network of political influences and the nomination committee was always dealing with these counter forces of who should be in, who should be out, who should be running against whom, what categories are these people in and so on.

36-00:19:19

Rubens: Did you ever serve on the nominating committee?

36-00:19:20

Smelser: No, I was never on the nominating committee.

36-00:19:23

McIntosh: May I interject with a question or two about this specific issue? Were there projects or publications in your past that you could point to when you were being interrogated by these groups as proof of your sensitivity to these issues?

36-00:19:40

Smelser: Oh, sure. Jeff Alexander once observed that I was talking feminist language twenty years before the feminists were in my doctoral dissertation when I introduced the whole problem of female labor and what happened in the industrial revolution and so on. That I was very explicitly bringing women's

issues right up to it and showing—and actually, though I didn't get inflamed about them, but showing injustices and so on that were being perpetrated in the industrial way of life. That was in my research, so I was always—and I did some early writing, a lot of early writing on the family, and raised issues of equality and so on. And here on the Berkeley campus, when I was in the chancellor's office, I made an initiative of trying to reform the definition of full-time student in such a way that it would favor veterans and people returning at a later age, who were mostly or many, women. Heavy population of women. So I took an initiative when I was in the chancellor's office to try to get these regulations that were imposed by the college, by the summer school, by everything that in fact I saw as being inequitable, even though they were not based on discriminatory attitudes. They had that effect. It was, you might say, systemic discrimination. So I made a heated effort to try to get some kind of flexibility into the definition of a full-time student when I was in the chancellor's office and on the Board of Educational Development. I failed because the deans brought me down. The deans just wouldn't respond to this kind of initiative. They had the rules, right, and they stuck by the rules, so I failed. But it was an initiative that I can point to. It didn't turn out to be a kind of flaming carrier of the sword for the women's movement but I was sensitive. So you asked the question. Yes, there were elements I didn't—that I wasn't just an old fashioned white—

36-00:21:39

Rubens:

Well, as chair you had really promoted hiring of women and minorities.

36-00:21:43

Smelser:

Yes. I sufficiently resented the questionnaire that the women's group sent to me that I didn't go into wild detail, defensive detail about what a great friend I was of the women's movement. But I did cite these.

36-00:22:27

McIntosh:

I was just wondering if the representatives from those groups were willing to look into your work and look over your CV and understand what were your stances on this issue, or whether they just sort of objected on principle.

36-00:22:42

Smelser:

To me it was more of an ordeal or ritual almost that they—and they were politically significant. The number of women in the field was growing rapidly and they were politically self-conscious. Other constituencies did not organize themselves.

36-00:23:00

Rubens:

The president of the ASA just before you is Maureen Hallinan.

36-00:23:04

Smelser:

Yes, yes. She was in that group of women who were nominated that I refused to get into the fray. It was Maureen Hallinan from Notre Dame, it was Cynthia Epstein from New York or Columbia, one of the New York universities, and Arlie Hochschild. These were the three women. And Maureen got it because

she was the Midwestern candidate that year. Regionalism also plays a role in the politics that I did not mention. But it's quite a heavy role with the South and Midwest and the coastal societies.

36-00:23:43

Rubens:

Was there anything that you had to lay out or that you felt you wanted to pursue?

36-00:23:50

Smelser:

I didn't have any organizational ambitions for the ASA when I went in as president. I was not going to carry the flag for public sociology or something. There was a thread of internationalization in the things I said about—in deciding on the theme I wanted to dwell upon in my presidential address, I was genuinely undecided in the beginning regarding what I would talk about. My mind was open. So I actually made a list. I thought maybe of talking about the vicissitudes of functionalism, which I was originally associated with and what it had gone through over the years and giving the kind of mature reflections on that. I had an idea of talking about history and sociology; I'd been in both fields. Maybe a half dozen topics. I won't recite the whole list. But they were topics, all of which fell under the category, you might say, statesmanlike reflections, right. And I was down at the Center at the time.

36-00:25:15

Rubens:

And you're working on the encyclopedia.

36-00:25:17

Smelser:

Oh, yes. It was right at the beginning of my encyclopedia commitments when I was elected. And to lecture about the encyclopedia, I didn't have any temptations. Anyway, I had this list of cosmic topics, all of which I had something, I think, to say about and something which would be appropriate for the tone of a presidential address. There's a general expectation, it's hardly specified, that you're going to be something of a looking over the scene, you're going to reflect.

One of the things I put on the list was ambivalence. I had already written a certain amount employing the concept. As I went back, I looked at my thesis as published. The first paragraph in the book said, "We are ambivalent about economic development." And I say the truth is that showed exactly what the contrary attitudes were. In my book on comparative methods, I say, "All humanity is ambivalent toward the alien," meaning the non—who are not us, who are the object of comparative studies. So I talked about the interplay between antagonism and envy there in the beginning of that book. I talked about ambivalence toward education in my book on British education, first paragraph. Completely without design. This was the way I started them all out and then led in to the substantive preoccupations of each of those works. I had only discovered that recently as I glanced at and republished this and that and so on. That I showed such consistency in my formulations.

Anyway, what triggered it was Christine Williams, my great student who had been a graduate student of mine in the 1980s, had written this book on women Marines and male nurses. That is to say, a minority agenda, minority in a field dominated by gender. And she was much influenced by Nancy Chodorow, by Arlie Hochschild and by me. I directed her dissertation. But it was psychoanalytically informed, her work, and she was one of my great students. As it turned out, as we talked, she was one of the ones who edited the *festschrift* for me, along with Gary Marx and Jeffrey Alexander. And she happened to be a fellow that year at the Center, coming from the University of Texas where she was now a professor. And so as I was there fussing around and she came in. We were chatting away about something to do with her work at the Center. Suddenly I said, “Chris, I’d like you to take a look at this, this list of my topics. Just what do you think about it?” And so I handed it to her. She looked at it. She said, “You have to talk about ambivalence.” She said, “These other things, they don’t count.” She really gave me a tremendously hard line on this idea. Said, “You’ll have something more original to say about this than you do anything else.” And so she gave me this real campaign and she convinced me. It was one of these times when I genuinely was floundering around. “What am I going to talk about? What would be the most important? What might be regarded as canned stuff?” Everything was going around my mind. So in a way she almost convinced—I even say in the footnote to the presidential address, gave her credit for being especially influential in my thinking about ambivalence.

And, of course, I knew I could say something. It had been in my mind. I had written this essay for the American Academy on the politics of ambivalence, which was my own commentary on multiculturalism, affirmative action and diversification of higher education. I had written this political essay called “The Politics of Ambivalence,” the theme of which was that every contender in this fight that we’re witnessing over the cultural turn is ambivalent about their own position. And if they’re ambivalent about their own position, we have a special breed of politics and I’m going to now try to lay out what the peculiarities of the politics of ambivalence are. So I had really gotten into it already before my election and that’s the reason it appeared on my list. And then in 1999 I wrote this essay on affirmative action and it was almost entirely in the category of ambivalence. The reason that affirmative action hadn’t settled into the polity the way almost all other innovations, like social security or other things, had settled into the polity was that we remained ambivalent toward it culturally, politically, economically, et cetera. So that thread was really alive in my mind at the time and so that’s why I made this decision to give it the most general theoretical treatment I could.

I also decided to use rational choice as a foil, and this was also a lifetime preoccupation, as we have talked about. And that rational choice theory is remarkable for its neglect of ambivalence. Things have utility, things have dis-utility. These are the references to values and liking and attraction or unattraction to objects of economic value. But no formulation at all that you

can both like and dislike at the same time. You can have positive and negative attitudes toward the same object without having it resolved. And you can hold these attitudes simultaneously with one another, and also the discomfort that ambivalence causes for individuals and how they're forever trying to resolve it into either positive or unequivocally negative attitudes. So all these threads that it—and I also argued that most of psychoanalysis had, in one way or another, been attacked or discredited, a lot of Freudian theories. But what remained absolutely central to the field was the idea of ambivalence. All these things sort of fed in. And so I decided to single this out using the rational choice as a kind of foil, if you will, and then point out those arenas of social life in which ambivalence was absolutely—played an absolutely central role in how we couldn't understand them without understanding the struggle with ambivalence on the part of people who held those attitudes. So I talked initially about the most obvious examples, ambivalence toward someone who's gone or died, and so well documented in the psychological literature. It was an easy case if you will. I then went into—

36-00:32:51

Rubens:

Separation. You called it death and separation.

36-00:32:53

Smelser:

Death and separation. And then I think I went into the true ambivalences toward economic objects and made the distinction between our more or less unambivalent attitudes toward completely instrumental things like hammers and nails and toothbrushes, but when it gets to status symbols, then everybody's ambivalent toward status symbols and you can't understand them without taking that into account and that we understood the vicissitudes. And so I developed a theoretical point of view that ambivalence is particularly charac— My master proposition of the presidential address, was that ambivalence is understandable when you're dependent on someone or something and can't escape. That is the field for ambivalence. So in most markets you're either, "Yes, I want that," or "I don't want it." Either one there. And you're not driven. If you're in an ambivalent situation, you're forever trying to resolve it and forever conflicted about it. So I took these situations of when people are dependent, when they're dependent because they're young, as children, dependent politically, dependent ideologically, dependent on a social movement, and tried to introduce a new understanding based on the mixed attitude toward objects and institutions and values that we can't understand if we take a univalent psychology. That was the essence of my presidential address.

36-00:34:23

McIntosh:

It does seem really to complicate the old theory of value and the science of ethics, where you take the decision and then read the value back into it, right? In this case, if I understand it correctly, a person can make a decision but still, after the decision, have very opposite feelings.

36-00:34:45

Smelser:

Well, yes. Well, actually, some of the minority of economists have developed this very interesting concept of the economics of regret, which is after you purchase something, you spend your time fussing about whether you should have done it. That's ambivalence. It's not a very central tenet in economic analysis, just a little side speculation on the part of a few imaginative economists. But no, it does. And it also says we're dealing with a process. We're not talking about a resolution. I had a lengthy commentary on public opinion polls, which are framed as univalent. How much do you favor this? How much do you not favor it? As though these were the alternatives. I said, "No, that's not the alternatives. You simultaneously love and hate."

36-00:35:37

Rubens:

As if they were definitive explanations of what people really think.

36-00:35:39

Smelser:

That's right. And so you favor, non-favor. This is a flaw, a structural flaw in measuring of attitudes. Mike Hout jumped on my neck when he read this.

36-00:35:51

Rubens:

Who's this?

36-00:35:52

Smelser:

He's a colleague in sociology who's a survey analyst. And he tried to get me to leave it out. And I responded to what he had to say. The idea is a lot of decisions are never resolved and that's kind of not the way that the vast majority of academics or businessmen or deciders regard it. So I saw it as a somewhat revolutionary statement.

36-00:36:22

McIntosh:

So in a model that is foregrounding the importance of ambivalence, where can a sociologist find value and are values still a relevant focal point for sociologists?

36-00:36:40

Smelser:

Oh, you mean values in the straight sort of social—

36-00:36:44

McIntosh:

Just the values of an actor.

36-00:36:48

Smelser:

Oh, of course. Well, the point is it doesn't do away with the idea of values, any valuation in attraction and being drawn to an alternative in a choice. This is the point. My point always was, is, that this is not a pure process. A value is not an unequivocal concept. You didn't want to totally go insane over this idea or declare a complete relativism but I just think you get a different dynamic if you understand that people are of mixed feelings about either one another or about values or systems or their own society, whatever.

36-00:37:25

McIntosh:

All right. Another important aspect of this to me seems to be that the making of a decision sets in motion another process, which is the reflecting upon the decision and the conflicts that you then try to resolve afterwards, whereas before, in kind of old school social science, it seems like once a decision was made, that decision was made. It's final, right?

36-00:37:50

Smelser:

One of the ideas that I'm going to develop in my Kerr lectures has the idea when you get a process of change going on in an academic setting, and it's especially true of academic settings which are highly moral in their fundamentals, is that you get a deification of the change. A brave new world, right, on the part of enthusiasts like computer technology or something like that. That's one utopia that gets invented in the conflicts over change. The deification of the future leads to a denigration of the past, a second utopia. A negative utopia. But at the same time you get a Cassandra response which says that this new innovation is leading us astray. It's undercutting our fundamental values, academic values in the cases I'm going to argue. And then that's a third utopia, negative utopia, that the new change is a disaster. And there's a fourth utopia of romanticization of some past that we're now losing because this is coming in. And you either really don't understand the dynamics of conflict in that academic setting unless you understand these absolutely contrary tugs that go along with any change. All stems from the uncertainty involved in some changes coming along and people take extreme reactions of this to the change. I'm going to develop this in connection with attitudes toward commercialization of the university, attitudes in the past toward the research emphasis and external research. I've got the evidence to show that these elements, these many utopias show up in all the controversies about change in the educational system. So that's the idea I'm going to develop.

36-00:39:38

McIntosh:

The image that appears in my mind when I hear you explain this is almost of a sort of perpetual motion machine of conflict, where each resolution of a conflict then sets in motion another—

36-00:39:51

Smelser:

Yes, creates a new—

36-00:39:52

Rubens:

A dialectic.

36-00:39:53

Smelser:

Yes. It sets and it establishes a new point of view, a new culture, which also can then regret when some new kind of change occurs. It's a kind of cyclical and dialectic. Dialectic not in full dress. But the logic is there.

36-00:40:12

Rubens:

You conclude your talk with ideas about conflicts in the fields of social science. “We social scientists, in our brief history, have become very divided and see things in very—

36-00:40:29

Smelser:

Yes, I think I gave examples of our attitudes toward deviants, attitudes toward community. On the one hand, the community is something we absolutely worship. At the same time, we see how it absolutely wrecks people. It’s constraining, alienating and so on. And these attitudes in our own field remain side by side and continue to form part of the great dialectic that we ourselves kind of split up and divide over, choosing one side of the— You see, a central theme was ambivalence is rather hard to live with, simultaneously holding strong attitudes of attraction and repugnance. So people are forever trying to resolve them in one way or the other, either into a positive—unequivocally positive frame or unequivocally negative frame. And that was the take off for pointing to these dialectical oppositions that we see in our own field about our very own subject matter. Development, community urbanization. Just name it.

36-00:41:22

Rubens:

Well, an implication also about the kind of contentiousness within the field over representation of particular groups or tropes or—

36-00:41:30

Smelser:

They tend to become interests. The resolutions of these ambivalences become interests, right, and form this sectarian, almost, quality of most academic fields.

36-00:41:42

Rubens:

Well, precisely what you were saying was exhibited in the contest over how was the editor going to be—

36-00:41:50

Smelser:

That’s it. Yes. All around us. And in this book that I’m coming out with Reed, I have two last chapters. One on demand for social science knowledge, meaning from business, governments, policymakers, et cetera, and the character of that demand and then I have a whole section on supply. And then I get into the whole internal dynamics of the academic world and there’s one section that deals with sectarianism. This will be coming out of UC Press hopefully.

36-00:42:27

McIntosh:

Now, I don’t want to harp on this or be too heavy-handed, but I do want to point out that over the last few interviews, Talcott Parsons has come up as somebody who avoids conflict in all aspects, both in his personal life and in his work and it’s something that we’ve joked about a little bit. And here we are with you and the model of the—the sort of ambivalent model where conflict is almost omnipresent.

- 36-00:42:54
Smelser: Endemic.
- 36-00:42:55
McIntosh: Yes. Or endemic is maybe a better word, both in the individual and then sort of more structural.
- 36-00:43:01
Smelser: Yes, no question. That's been a constant. I have, in fact, I believe in my career talked more about conflict than about stability.
- 36-00:43:12
McIntosh: Which would almost be the opposite of what your detractors would say, right?
- 36-00:43:18
Smelser: One absolutely has no control over what any detractors might say.
- 36-00:43:25
Rubens: The immediate response to your talk was that—
- 36-00:43:30
Smelser: Very enthusiastic. A cast of thousands showed up. It was really a wonderful experience in Toronto when I gave the talk. Like the whole profession turned out. It was unbelievable. Jam packed in the auditorium. Extremely warm reception. It wasn't exactly the most opportune setting to have this kind of reaction because the ASA had developed this whole system of prizes for people who did this, most relevant for this, general book prizes. There had been a proliferation of these prizes, of the section prizes, and they always preceded the presidential address. They go on, boy, and time was going on and I was getting antsy up there. As it turned out, I could give my whole address. But you just see these thousands of people just going through these things as people walked up and got their prizes and so on. They've since reformed it. They've really cut those parts of the ceremonies way down. But anyway, I gave the entire address. Immediately a lot of people flooded up to the podium afterwards and there was a party afterwards and it was all very—some person's narcissistic dream to have it accepted like this. And I got a lot of positive comments about it.
- 36-00:44:55
Rubens: Alexander calls it your most influential essay of your later career.
- 36-00:45:03
Smelser: I would accept that. Yes. There may be some contenders but I'd have to say it was. It's really widely noticed. And, of course, as Nancy Chodorow pointed out, it was the first presidential address that took psychoanalytic thinking seriously.

36-00:45:27
McIntosh:

So did this spawn a renewed interest in the psychoanalytic aspect of your work and were there any repercussions from that talk that led to putting out the *The Social Edges of Psychoanalysis*? Or are those separated?

36-00:45:41
Smelser:

Well, it was one of the essays that are included and it was probably the occasion more than anything else for me deciding to put my psychoanalytic essays into one single place. Maybe we could talk just a moment about that since we've just—in the meantime, and closely related, I had all of the sudden experienced the death of Erik Erikson, with whom I had this extremely close relationship and that was a big part of my psychoanalytic linkage, was right through Erikson and when we worked together. Robert Wallerstein, a long collaborator from San Francisco, set up a memorial kind of conference on Erik. His son was there, Kai, and a lot of quite eminent psychoanalysts showed up to contribute to the symposium. Bob Wallerstein asked me to contribute and so I decided I could best treat Erikson by analyzing him as a social scientist because he wrote about so many more macro issues in his career and history. So I decided to do that. That turned out to be what I regarded as a quite—I could say some original things that other people hadn't said about Erikson and so I delivered that. So I had this essay. It was published separately in the proceedings of this ceremony. But I felt it was something that gave more heft to my psychoanalytic work. That and the presidential address sort of tipped the balance. I had written maybe ten essays over my career and I had toyed with the idea of putting them together and these were kind of the critical mass of my contributions that said this seems like a thing that I could really, really, really do.

36-00:47:31
Rubens:

You had written a piece in '93, the *Psychoanalytic Mode of Inquiry in the Context of the Behavioral and Social Sciences*. With Prager and Rustin editors.

36-00:47:43
Smelser:

Yes. So that was, I think, a very, very important essay. It rose out of a joint course I gave in San Francisco with Bob Wallerstein, actually. And what I did was to take the idea that the knowledge generated by psychoanalysis was generally created in a curiously artificial situation, mainly the clinical encounter. And almost all the data that psychoanalytic theory has rested its case on has been clinical, in the case of treatment with those who are troubled, right. So you have this unique setting and unique process going on. At the same time, you have an intellectual framework of a general sort, a general theory of psychoanalysis that has risen out of this particular process. So you have a psychoanalysis of history. You have a psycho-history. A psychoanalytic interpretation of literature and works of art, psychoanalytic interpretations of organizations, psychoanalysis interpretations of behavior during disasters. It's all over the place.

So what I decided to do is to write an essay which was theoretical and methodological and substantive, saying what are the constraints on applying a theory based on clinical evidence to non-clinical settings. And here's where I picked up literature, I picked up historical interpretations, picked up political interpretations. Picked up work that had been done on bureaucracies and their depth, emotions and organizations and so on. So it was published obscurely in England by some publisher. And I never even received a copy of the book. I received a Xeroxed version of my chapter and it was a very fly-by-night publication. As far as I could tell, no one saw it, at least that I ever heard. So I felt it was really good to get it into a more visible place. And that was true, frankly, all of my writings, they were—and I decided the California essay should go in there because it was clearly about ambivalence. And then I included my "Politics of Ambivalence", I included my affirmative action essay on why—ambivalence was the core of four essays in the middle of that book. And I tentatively showed it to Jim Clark, saying, "You have any interest whatsoever?" I wasn't convinced that he would have. But he said, "Send it in," and so it came out in '98.

36-00:50:23

McIntosh:

He seems to be an unflagging supporter of yours.

36-00:50:28

Smelser:

I think I told you that Clark came to see me the first two weeks I was on the Berkeley faculty. Someone had said, "Here's a young hotshot," or something, and he was working for Wiley at the time. And he came into my office and formed a relationship with me. And he then went to Prentice Hall, where I then became series editor for a long period of time with him and Ed Stanford. Jim went on to Harcourt Brace and then ultimately was chosen the director of the UC Press. And when he came to UC Press, he and I immediately formed, re-formed, our relationship because it was so close. You don't do that with publishers very often. Generally there's a distance and sometimes a prejudice on the part of academics against publishers and their agents, as being part of the world of business and so on. But Jim and I didn't have that. We just had a very nice personal relationship and he relied on me tremendously at UC Press and was a constant supporter in terms of any publications that I wanted to have published by UC Press.

36-00:51:80

Rubens:

There was a spate of essays that come out in '94. You must have been writing them earlier. But listed on your CV it's pretty extraordinary. *The Sociology of Science, Humanism and Arts. Social Theories*. A preface to the Platt and Gordon *Self, Collective Behavior in Society: Essays Honoring Ralph Turner*.

36-00:52:08

Smelser:

Yes, I wrote the introduction to that. Well, we talked about some of these. There's that spate of things. Statesman-of-the-field articles. They came in—

36-00:52:19

Rubens: *Economic Reactionality as a Religious System.*

36-00:52:22

Smelser: Yes, that was at a conference on materialism at Princeton that another student of mine, Bob Wuthnow, sociologist of religion, organized.

36-00:52:31

Rubens: Figure that into your *Odyssey* book, as well, where you talk about religion.

36-00:52:37

Smelser: I talk about different aspects of religion. The big splash of the nineties was my book on English education which we talked about, but nonetheless, a lot of other things were going on.

36-00:52:52

Rubens: I wanted to ask you about one other piece, a memorial for Reinhard Bendix.

36-00:53:12

Smelser: Oh, yes, yes. That's a very interesting story. Bendix was, of course, one of the people who hired me. I went for my interview with him in his house at Eight Mosswood Road where I fell in love with the Berkeley view immediately. And Bendix became chairman the first year I came, replaced Herbert Blumer, who was the guy who was in the chair and officially hired me. But Bendix came in. It's so funny. My life was so tied up with this. Bendix had received compensation from the German government for the mistreatment of his parents, concentration camps. They did not get killed but they were really mistreated. They went to Israel after the war and then his mother came here. He lived at Eight Mosswood Road. With the money of compensation that he got, he bought number Ten Mosswood Road, which is cheek by jowl with number eight, right at the top of the same steps. He rented that house to me so I was—six months after I had come Berkeley. I had lived down near the old co-op for six months near Sacramento Street. He brought me into his house. He was chairman of the department. So I lived there for several years.

When my first marriage broke up about five or six years after I joined the faculty, I moved out and lived in an apartment and then Jane Bendix said, "Do you realize that number Six Mosswood Road, which was at the bottom of those steps, has come free. Would you like to rent it?" This was my landlord. A faculty member owned it. So I moved back to number six. I was around Bendix. And then when I remarried, Bendix moved across the street to Orchard Lane and sold me number eight. So I was moving around this tight little house. I once joked with Bendix, he wrote it down, said, "Look, you're the chairman of my department. You hired me; you're my landlord. Do you think about opening a company store down the road that I have to shop at?" because they were much in my life. He and I had a very close personal relationship. He didn't like Parsons very much and so he was constantly—he was kind of bringing up these things. So there was an element of—he got caught in that thing. I didn't particularly feel like fighting him. He was more

of a historicist, you might say. He wrote about political history mostly. And collaborated a lot with Marty Lipset.

So I'm pretty close to Bendix. I can't say I was personally very close. He had a remoteness that—I don't know what to attribute it to but there was always a little bit of a barrier. Not dislike but stiffness, I suppose, you would say in our relationship with each other. Though he asked me to be a commentator on his retirement, one of several. And then I knew that he wanted me to do this and other people asked me to write the memorial after he died. So it was one of these very interesting things. Naturally, in the memorial I expressed mostly admiration for him. That's the occasion to do that. And it was genuinely felt admiration. But then, once again, there was a bit of intellectual style difference. He brought it up more than I did. Not that he thought my work was—he thought my work was excellent. He pushed me all the time. When I got these outside offers when I first came to Berkeley, he was the guy who had to represent me to the administration and he was completely supportive on all occasions. So we had a kind of very interesting mixture of closeness and distance in our relation.

36-00:57:24

Rubens:

What's our timeline?

36-00:57:25

McIntosh:

Well, we have about three minutes left, so this might actually be a good place to stop.

36-00:57:30

Smelser:

Gosh, we never cover all we plan to. But it's your fault this time. You had many questions.

36-00:57:34

Rubens:

Yes, perhaps I had too many de-railing questions—and we're not through with what you wrote about for his memorial. Now Bendix was president of the ASA. I was just looking for Berkeley people. Goffman, Lipset, Blumer. Etzioni who had been here a short while. Kingsley Davis.

36-00:58:00

Smelser:

Kingsley Davis was president before I came here.

36-00:58:04

Rubens:

Anybody you thought who should have been president who was from Berkeley?

36-00:58:06

Smelser:

Well, I always thought Philip Selznick deserved the presidency and he was never—he shunned the politics of—he had shunned participating in the association by choice. But in looking at his career, I believe he fully deserved it.

36-00:58:25

Rubens:

Okay. So I guess we're running out of tape. We'll pick this up next time. .

Interview #19 August 29, 2011

[Begin Audio File 37]

37-00:00:00

Rubens: Hi, Neil.

37-00:00:02

Smelser: Good morning.

37-00:00:03

Rubens: It's our nineteenth interview. We ended so abruptly last time, talking about the remarks you gave for Erik Ericson's memorial, and the influence he had on you. We also talked about a few other people who influenced you or with whom you may have disagreed, but nevertheless admired as thinkers. At one point I think you had said that the German philosophers, the sociologists, had influenced you more, but you named a couple of French that you were drawn to.

37-00:01:30

Smelser: Yes. I wouldn't say that—or meant to say—that the Germans influenced me more. I interacted with the Germans a lot more, largely because they have a more open kind of intellectual system with respect to sociology than the French do. Generally, the French is a much more self-regarding tradition. But in any event, the main French sociologists that I came to know and were influenced by in some respects were Michel Crozier, who was a writer mainly on French society and French organizational theory and practice. He had a more positive and open orientation to American sociology than most. He wrote a very famous book on bureaucracy. I used it. I quoted it, I think, on a number of occasions. Then I made a point of contacting Crozier, I believe at International Sociological Association meetings, then subsequently had occasion to interact with him. He recognized my work. I'd have to say we formed a social but not a really terribly deep, personal relationship.

Alain Touraine is another story. He's a sociologist, but a French intellectual more than anything else. Of course, Alain Touraine jumped on any situation that was coming up in France and sort of proclaimed it as cataclysmic or world revolutionary or whatever. That was his style. That didn't appeal to me, of course, but nonetheless I always appreciated Touraine's insights on things. The student movement, for example, which he wrote extensively on. I didn't agree with him on its world significance and so on. We had a different diagnosis, but nonetheless we kept in contact with each other. When the Brazilian leading sociologist, Cardoso, whom I also had a good relationship with through the International Sociological Association—when Cardoso was here, Touraine paid a visit to Berkeley and I had them over in this very room for dinner one night with their wives. We had a really good, open, you might say free, intellectual session that evening, as I recall, going from topic to topic, entertaining our views on things. No disagreements. We were in

different parts of the world and different parts of the intellectual world, but this was a kind of a very, you might say, collegial sort of time. I see Touraine. He was very active in the International Sociological Association. I was the first recipient of this lifetime achievement award in the International Sociological Association. He followed me in receipt of the second award of that. We kind of see one another as a little bit of brothers, even though we never try to pretend that we had a common intellectual outlook.

Raymond Boudon, I got to know really more—he was also a guy who'd engaged himself in American sociology, particularly Columbia sociologies, Merton and Lazarsfeld. As a matter of fact, his writing could have been written in American theory. He was very much a product of the World War Two hegemony of American sociology in the Western world, or in the world in general. He was methodologically very savvy. That was Lazarsfeld's influence. He wrote as much as anything on methodology. I decided to choose Boudon as my editor for sociology for the encyclopedia. It was a kind of strategic choice for me. I felt I would have some trouble choosing an American for it, because if you do, you just cast a vote for a certain type or a certain person. Since we were in the business of making it international anyway, I thought it'd be a very good idea to ask Boudon. I never knew him personally very much. I met him only a couple of times, but we had, once again, an intellectual rapport with one another. He agreed.

He had been kind of out of touch with the field of sociology, however. He's older. You always lose touch with the younger people coming up. I don't know whether I mentioned this or not, but when he submitted his list of topics, which I worked with him closely on because I thought he had some omissions—left out certain fields like criminology—I made suggestions. He accepted them all. Also, he was out of touch with people. I think maybe I mentioned that a certain percentage of his candidates for writing essays were dead, had passed him by. So I tried to bring more of a youth movement into the sociology section. Actually, he and I were more coeditors on the sociology list. It never showed up in the encyclopedia, but being a sociologist, I worked closely with him. He couldn't travel. His health was already weakening, so he didn't come to the orientation meeting for all editors. All by correspondence with him. I tried to look him up in Paris on one trip that I made during the encyclopedia period, but he was out of town, so we never personally met. Nonetheless, I felt him kind of brother under the skin with me.

37-00:07:23

McIntosh:

Just for the record, other than a focus on methods, what are the defining traits of the Columbia school of sociology?

37-00:07:41

Smelser:

I answer in simplified form by referring to the essay by Merton, in which he referred to himself as a middle range theorist, or advertised middle range theory as the most productive line of inquiry in the field. He was also carrying

on a dialogue with Parsons, and he was also carrying on a dialogue with his own colleague, Lazarsfeld, with whom he was of course very close. That argument was that grand theory—he was very diplomatic—of the Parsonian sort is okay, but it really doesn't link you too much to engagement, either with empirical issues or with social problems and social policies. Merton argued that a middle range theory, which was certainly general and analytic and had to observe the canons of theoretical, reasoning, was much more relevant. He placed himself in that level. It was the first of many, and the gentlest, of many attacks on Parson's level of theorizing. Of course, Parsons got a broadside from C. Wright Mills, who thought that this was useless stuff. Theology, ecclesiasticism, whatever—useless. Mills didn't like Merton's compromise either. Mills was much more engaged in the evils of the world and the problems that are there, and how to get at them, and political activity. In Mills' book on American sociological theory, he dismissed Merton in one sentence, calling him “the middle man,” and that was it. That kind of closer touch with empirical reality, closer touch with the current and important institutions, such as voting, such as mass communication, such as the media and so on. They got into many developments in medical sociology. All closer to the ground, you might say. I'd say that would be the distinguishing characteristic of the Columbia school that was dominant in the period from the fifties and sixties.

37-00:10:07

McIntosh:

That particular period is what influenced Boudon?

37-00:10:10

Smelser:

Yes, and he was a graduate student, I think, at Columbia himself. If I'm not mistaken, he actually studied at Columbia and got a Ph.D. there. That took on him. I almost went to graduate school at Columbia. We covered that.

After I had my Rhodes scholarship and was coming back, I had been turned down by the Society of Fellows and I came back as a graduate student because I had linked up with Parsons again in England. I applied to Columbia just as, you might say, a backup or an alternate. I had some reservations about continuing with the same people I had studied with as an undergraduate, so there was a little ambivalence about going back to Harvard, though I still loved it. Anyway, Columbia really wanted me to come. I got a handwritten note from Merton, really begging me to come. They offered me a very big fellowship to come to Columbia. I thought about it kind of seriously. That would have been, of course, a very big difference in my own career. But Parsons had already engaged me in this “Economy and Society” project. I basically didn't have much choice at that point.

37-00:11:45

Rubens:

Off-camera, when we started, I asked you about the classical sociologists who had the most influence on you. Is now a good time to sum that up?

37-00:12:01
Smelser:

Yes. The two who have influenced me the most and that I've written most about are Max Weber and Emile Durkheim. I was inspired originally with their work through Parsons, because they were the two biggest influences in his life. His first great monograph picked out Weber, Durkheim, T.H. Marshall, the English economist, and Pareto. The last two were not really as central to the development of American theory. I read all of Weber and Durkheim in graduate school. Through Parsons, I came to appreciate their work as well.

I also, largely through my writing on the British Industrial Revolution, had to come into contact with Marx, because the largest part of the first volume of *Capital* is on the very subject matter that I was dealing with, namely the history of the working classes in the nineteenth century. So I went all through *Capital* and actually made a curious observation that I was reading the actual same blue books that Marx was reading and interpreting this. I mentioned Weber and Durkheim coming through Parsons, in a way, to me. Marx came through another way in Parsons, but Parsons so hated Marx. He just rejected him. He's just one of the people he thought shouldn't have happened in the history of thought. He was open about it. Parsons was very diplomatic about almost everything he talked about, but with Marx and with Thorstein Veblen and with C. Wright Mills, Parsons was very vociferous and very negative toward all three of those.

Now, I did not absorb Parsons' negativity about Marx. I certainly did not share his views of revolution or his total diagnosis of capitalist society, but I came to appreciate the theoretical framework which he was using, some of the insights, and so on. I wrote a considered critique of Marx in the next-to-last chapter of my doctoral dissertation when it was published. Later, I actually edited for the University of Chicago one of the books in the history of sociology. I edited the Marx book, and wrote kind of a synthetic, more accommodating type of essay on Marxian theory and its relation to sociology, and included those things which I thought were his most valuable contributions to the study of social structure and to the study of social change. The influence of Marx on me was very different. In other words, I adopted a kind of middle of the road, from the standpoint of either accept or reject Marx, which is so much the story about Marx's role in the history of thought. I had a much more contingent kind of relationship with the works of Marx.

Now, there were other figures as well, like Tocqueville that played an important role in my thinking, especially about American society. Others, like Simmel, not so much. I think it wouldn't serve very much to point out those theorists that didn't have much influence on me.

37-00:15:47
Rubens:

So I think we're at a transition back to Berkeley.

37-00:15:52

Smelser:

Well, there are a few things I'd like to talk about, if I may, on the National Academies and the Social Science Research Council and on the German-American Academic Council. Those three things and the Guggenheim Foundation.

37-00:16:07

Rubens:

Yes, you must. I just wanted to set the stage a bit. This is why I asked about your return to Berkeley.

37-00:16:12

Smelser:

I moved back to Berkeley in 2001, September 1, 2001. Settled in this very house. We never sold it. We had bought it in 1980, and we decided that we knew we were coming back. My heart was always in Berkeley. I still remained a loyal Cal fan in basketball when we were down there. We even kept our season tickets, and punished ourselves by driving up the Nimitz to see these games on Friday or whenever. The traffic was absolutely horrible. In any event, came back here and settled. As I say, it was a fateful date, because eleven days later, 9/11 came and kind of changed my life. I think we'll get into that.

37-00:17:01

McIntosh:

How exactly were you involved in the National Academy during this time?

37-00:17:08

Smelser:

I was elected in 1993. I didn't play a terrifically active role until mid-1990s. I was asked to be a member of a thing called CSAC, Committee on International Security and Arms Control. which was the National Research Council's body on foreign policy and security. I joined it, but I was asked after one year to rejoin the Committee on Behavioral and Social Sciences, which is one of the divisions of the National Academy, and to chair that committee. That committee had been the parent committee of my work in the 1980s on the social sciences and their past, their utility, and their future. So they had superintended the work of that committee on basic research. Now I was asked to join the parent committee.

37-00:18:24

Rubens:

Who asks you? The parent committee?

37-00:18:27

Smelser:

The academy officials who were responsible for staffing these committees. I was a good choice. I had been elected to the academy by this time. All my previous work had been prior to my election, and now I was a full-scale member of the academy, so they asked me to join it. I said to John Holdren, who was head of the CSAC that I loved working on that committee but I felt it was closer to my capacity to contribute to things by joining the Committee on Behavioral and Social Sciences in general. That committee superintends all the special panels and all the special projects and reports that that division undertakes. We read them, we approve them, and we help staff them, so on.

It's the parent guiding committee for all the work that's done by that division. It doesn't have too much power in that they don't take terribly much initiative, except occasionally when they see the opportunity to initiate projects and panels and working papers and so on, but they are clearly a guiding and monitoring service.

37-00:19:36

Rubens: How many are on that committee, about?

37-00:19:38

Smelser: I would say it's about maybe twenty. They're outstanding social scientists, all over—

37-00:19:44

Rubens: All academy members?

37-00:19:46

Smelser: No. I remember Nelson Polsby was a member. He was not an academy member. It's mixed, but a majority of academy members are on it. Meets several times a year, does its work. Is the imprimatur organization for all the work that's done. Then, after I'd been on there for a year, they asked me to be chair. The guy who had been chair stepped down. I'd had a lot experience over a long time of chairing committees. I'm not sure what the dynamics were in choosing me, but he said, "Will you chair it?" So I said okay. Then, after one year, they changed its status from being a committee to—it was called CBASSE, the Committee on Behavioral and Social Sciences and Education. They changed it to DBASSE, Division of—so on. I always teased them about it—next it was going to be EBASSE, when we went completely electronic.

37-00:20:51

McIntosh: Was there any actual significance in that change of name?

37-00:20:54

Smelser: Yes, there was. When that status of my group changed, I became a member of the central governing council of the National Academy. I would go to the governing body meetings. Previously, they hadn't involved them. Then they began paying a significant stipend to the head of this committee at the same time. They added a salary of \$50,000 a year to my income for being on that. I served as chair of DBASSE for, I think, six years, up until 2004 or something like that. The exact dates are available. I was fairly aggressive in this. I was in the core of the academy by this time. It's quite interesting. I was very active in the American Academy of Arts and Sciences as a young man. At this stage, I was extremely active with the National Academy. Up to now, the last five years, I've been very active in the American Philosophical Society. So all three of these, I've had a long period of membership, but a selected period of maybe five years in each one of them of being very active in its governance and its policymaking and so on.

This set me up as a central member of the National Academy of Sciences in the social sciences, because I was in these leadership positions. Of course, that set me up as a visible target when the academy subsequently became involved in its activities on understanding and working toward the prevention or control of terrorism. My term with DBASSE overlapped with my work on terrorism in the National Academy, in 2001. Those two went together. I was really thick with the academy for a period of about eight years.

37-00:23:03

Rubens:

You said you were fairly aggressive with this. Does this mean as a member of the central governing council?

37-00:23:14

Smelser:

As chair of DBASSE, I was something more than a kind of referee who said, what's your point, what's your point, what's your point? I really entered in aggressive, meaning that I wanted to influence the policy of the committee's panels. I'll just give you two examples. A congressman got extremely agitated about the shootings at Columbine after they occurred. He put in a bill, with some money attached, to commission the National Academy to do research and come up with scientific findings on school shootings. It was a very definitive and direct charge policy. Figure these things out and tell us what we can do about them. That was a slightly hysterical tone that this legislation had. I said, we should not accept this charge on its own language and on its own assumptions. We do not have the know-how to come up with general principles about what causes these events, nor do we have any fool-proof ways of preventing them. We're going to have to insist that if we undertake this assignment that Congress has asked us to undertake, we're going to have to bill it as a series of case studies from which we can gather limited insights and hints, and hopefully some ideas that might be useful to administrators of schools, but we're not going to pretend to follow the tone or the spirit of the recommendations. That's an example of how I intervened in the meaning of these things.

There was also a commission for DBASSE to support a group on compulsive gambling. This, once again, came out of some kind of concern, I believe, of some Congress people. The original charge came to us to deal with compulsive gambling as a psychopathology. The psychological aspects of it. The sort of Dostoevsky approach to the gambler. I, once again, as the leader of DBASSE, said, look, we can't leave the social and economic side of this out. There were already interests being expressed in this panel's work by the gambling industry in Nevada. They, of course, liked the idea that it was a psychological problem and not something systemic or that could be dealt with at higher levels. So I made an insistence of kind of modifying the charge to get more into the economic, and to some degree, the political side of it, and try to influence the panel's work along those lines. Those are examples of how I made an effort to insert my own commitments and views and sensibilities into the work of the specific panels.

37-00:26:39

Rubens:

What about the central governing council? How big was that, about?

37-00:26:45

Smelser:

That was a different kind of voice. They were interested on a larger scale. The central council involved the National Institute of Medicine and the National Academy of Engineering. Those had merged into what's called the National Academies in the early 1990s, I believe. It was much more comprehensive. A lot of it had to do with jurisdictional disputes among the three academies, and general policies involving issuing statements. Later on, they got very much interested in the whole issue of the immigration and recruitment of foreign scientists and students. This was a post-9/11 issue. Still is an issue. Are we discouraging our own benefit from foreign scholars and scientists by having such ridiculously demanding and punitive immigration and residence rules? I'd have to say, in the larger central body, I was more, I'd have to say, kind of responsible contributor to the discussion. "Aggressive" wouldn't be the word. I was just a committee member, but one who actively participated.

37-00:28:12

McIntosh:

The Columbine example speaks to my question a little bit already, but I wanted to just get your thoughts on what you saw the National Academy as really doing. What its functions are or were at the time when you were so actively involved.

37-00:28:28

Smelser:

I'll leave aside the purely honorific side of the National Academy, which of course was why it was born, to honor the nation's leading scientists. Gradually, it moved away from the math and science complex to include the social and behavioral sciences. Anthropology and psychology came in first, and then the others. Its tone is definitely honorific. At the same time, when the National Research Council was formed, and I cannot tell you exactly when that was, it took up the role of being more actively involved in policy issues and social problems, scientific problems, of the nation. The National Research Council, you might say, is the research wing of the National Academy. Now, it took its place as an agency interested in social problems along with two other major contributors to the understanding of social problems. One was the universities themselves, who do a tremendous amount of research relevant to society's problems. They're forever being called in as experts and for testimonies. Their works are cited, but they're not designated. The academies are designated to follow their own noses. Many of them become practical, many don't. Many of them think it's the wrong idea to be practical, and so on.

That's one source of knowledge for social policy and the understanding of social problems. The other is the think tanks of the nation, which are private, usually. It's often for-profit organizations that are presumably meant to supply, on demand and on contract, relevant knowledge for social policy. Those, in the past couple of decades, have become increasingly partisan. There are think tanks associated with the Republican Party, there are think

tanks associated with the Democratic Party. They thereby compromise their, you might say, distance from the policy issues that they are commissioned to study. They tend to be more likely to say what the politicians or the agencies want them to say. I think they've become contaminated by their own partisanship. They're not think tanks. They are suppliers of political position statements.

That is not the kind of understanding of social problems and social issues that the academy provides. It's a model. It takes commissioned projects from outside—usually Congress, sometimes from foundations, sometimes from agencies—and receives money. The National Research Council survives on income from agencies who are interested in certain social problems. At the same time, once the problem is handed to the academy, they are in charge of choosing those who are going to constitute the panel. They're responsible for interpreting the charge as it's given. There is a system, an implicit system, that those who commission the panel are going to stay out of its business. They do not communicate with it. That is basically adhered to, that these panels do their work in isolation, on their own, choosing experts from around the nation—some members of the academy, some not members of the academy—and they issue these panel reports, which are really intensively reviewed. I'll get to that when I talk about my terrorism involvements—how heavily they are reviewed by the academy staff and special review panels themselves. They cannot be accepted until they are approved by the reviewers. Then they are published under the auspices of the National Academy, which has been able to maintain this political distance and unique role in the understanding of the empirical and policy bases of social legislation and social policy. I talk about these alternative sources of usable knowledge in my book that's forthcoming. I have, in a way, summarized some of the kinds of views I took and presented in that book.

37-00:33:08

McIntosh:

It sounds like the National Academy is above the fray of ideology at a certain point, or at least has more checks against that than other think tanks or even universities.

37-00:33:21

Smelser:

Yes. It gets into them. I didn't want to describe it in perfect terms. For example, a panel came out with a recommendation, maybe fifteen, twenty years ago, that actually pointed out, on balance, the fact that the legalization of marijuana would cause fewer problems than we now have with the regulation of marijuana. The high officials of the National Academy would not endorse that report, largely because they felt the political pressure from drug enforcement agencies, as well as what they saw as public opinion.

37-00:34:08

Rubens:

Were you privy to that, being a member of the governing council?

37-00:34:11

Smelser:

No, it was before my time. That particular episode was before my time. They've gotten very controversial engagements on issues like global warming. A lot of their reports, which by and large are from scientists who generally verify the general hypotheses and directions and threats of the warming process, and of course, once they come out, they're the subject of bitter partisan attacks on these scientists if you read about these things. So they're not above politics in terms of what they do. Sometimes, they're tainted as these scientists are interested, so on and so forth. But by and large, compared with all other forms of producing relevant or usable knowledge, it seems to me to be the most insulated, and to my mind, the most objective of all of them.

37-00:35:05

McIntosh:

It was understandable, then, when 9/11 occurred that—I don't know who commissioned the work that you did on terrorism, but that the National Academy would be seen as one institution to turn to for understanding what happened and how to prevent—

37-00:35:25

Smelser:

I don't want to get too ahead of myself, but I'll respond to that by saying that when 9/11 occurred, within two weeks, a letter went to President Bush, signed by the three heads of the three academies, pledging the cooperation and the service of the academies to the current crisis. They took the initiative, saying, we are going to do what we can as scientists to intervene, to help, in this national crisis. It was in the wake of that, not by any requests from the government, that the National Academy undertook these several important panels on terrorism and its implications. This maybe covers what I would like to say about the National Academy involvement.

37-00:36:24

Rubens:

Until we get to the terrorism, okay. Of course, we want to acknowledge that this was taking place during your tenure as director of the Center.

37-00:36:32

Smelser:

Yes, mostly. I stayed on as the head of DBASSE for two or three years after my directorship, but most of it was during.

37-00:36:44

Rubens:

Do you want to talk about your role with the Guggenheim?

37-00:36:48

Smelser:

Yes. Guggenheim Fellowships are probably generally familiar, so I don't have to summarize them. They're one of the honorary sources of scholarship in the country. They give out something like—it's been variable over time, but something like 180 scholarships a year, to faculty members almost exclusively. It has an international presence as well as a national one. It's highly honorific. People list it as though they were members of some kind of academy if they've gotten one, but it's very interesting in that it gives very

small stipends compared with other research grants. The annual stipend now, I think, is maybe \$45,000 a year, which will help supplement sabbatical leave, but it's certainly not a year's support for most people. Nonetheless, it still continues to have huge numbers of applicants. People like not only the freedom that it applies for their own research during that year, but also to have the label "Guggenheim" tacked onto their own careers.

I received a Guggenheim Fellowship in 1973. It was to supplement my sabbatical for that year abroad, in which I did the work on comparative methods in the social sciences and economic sociology. So I was already a kind of bona fide alumnus at a fairly young age. Then, in the middle eighties, largely, I think, under the influence of Robert Merton and his wife, or wife-to-be, Harriet Zuckerman, my longstanding Columbia, loyal, devoted friends and colleagues, they had been involved in the selection procedure of the Guggenheim Foundation themselves for many, many years. They recommended that I become the sociology reader. There are some eighty categories of grants that the Guggenheim gives. Every academic field, and then a lot of them in poetry and literature and other arts and performance areas. They have referees to review all the applications in one field. Usually two referees, to whom are sent all the applications. They asked me to be one of the two sociology referees. It was sometime in the eighties. I agreed to do that. Each year, I would get some thirty-five to fifty applicants from sociologists who were trying to get a Guggenheim. I would read them, I would rank them, and I would send in my evaluations. They would then go into the larger central administrative decision making of the foundation on actually who's going to get the fellowships. I served in this capacity for, I guess, maybe—I can't know how many years—seven to ten, maybe.

Then, at about the time I was entering into the directorship of the Center, I got a request from Guggenheim to join the central committee. It's called Committee of Selection. It's the one that processes all the input from the readers. It evaluates the proposals themselves and makes the final decisions on the grants. I was asked, 1994, 1995, I believe, to become a member of that. I think I replaced Harriet Zuckerman, who had been in it for a long, long time. I thought that was good. I would do it. After one year, they asked me to chair that. The other chair, who was a Berkeley physicist, had fallen ill and actually died in his last year. Then they had one acting person who they didn't think they wanted to continue. In the meantime, I had been on there for a year. Joel Connor, who was the administrative head of the Guggenheim Foundation, announced to his board of trustees that, when I walked into the room to my first meeting, it became apparent that I needed to chair this group. He sort of made a joke out of it. After one year, he asked me to step in as chair, a role in which I served for fifteen years. My last year as chair is going to be this coming February. I'm stepping down. Not exactly that I feel like stepping down, but at a certain time, it's enough time, and I'm not young. So I'll be stepping down and there will be a new member from the social sciences.

Now, my role. I was responsible for seven fields myself. Of coordinating and evaluating the evaluators and their teams. These fields were sociology, political science, economics, law, education, planning, psychology. I think those are the ones. I would inherit all these, which were about 200 or 250 applications, which would be sent to me —intended that I was to read them all. What I did was I would do this. I would read them all and I would rank every field, even those in which were not my own, before reading what the evaluators have to say. I did not wish to poison my mind with the outside evaluators for each of these fields, so I would prepare the total rank order of all fields in my own mind before I even consulted these outsiders. Then they would come in and I would modify what I'd said and take into account all the reasoning and so on, and come up with a list of people that I would recommend. Meantime, there were six other members of this committee, all of whom had many fields to cover, and all of whom came in with their own recommendations. We had a collective meeting of a day and a half, every year, late February, in which we would go over the entire—hundreds and hundreds of applications for Guggenheims. We would end up coming up with about 180 names of people who we judged to be final. Our judgments were the final judgments. They were officially approved by the board of trustees, but they never changed anything. It was one of those situations. Then, as part of that, I was chair of this meeting. They're all extremely eminent scholars.

37-00:43:46

Rubens:

How many people, about?

37-00:43:47

Smelser:

Seven total. Seven on the committee. One of them had charge of all of history, one had charge of anthropology and folklore, and philosophy and religion, one had charge of all the physical sciences, one had charge of the life sciences. That was the way we divided them up and divided up the applications. One was in charge of poetry and literature and fiction and so on. We would have to mix it up. These were, as I say, very intelligent people. The meetings were themselves unbelievably gratifying intellectually. I just sat back. I was half entertained by this, as well as chairing it. They're very talkative. There's a high degree of culture and mutual affection, mutual respect, in that committee. It's one of the best I've ever served on. But nonetheless, they talk a lot and they have a lot of opinions, and some disagreements come up. I had to be responsible for moving it along and getting consensus, making decisions at the last hour or two of the meeting on those borderline cases that we didn't know what to do about. Often the opinions on those were stronger than the consensus opinions on people that we just said, okay, let's give it to them, and so on.

I had to take a pretty active role with these active people. Fortunately, their own views about the process helped me. They're very catholic, with one or two exceptions. I had one member who—it happened to be a she—was a representative of her fields and she wanted to make sure her fields got

represented enough. That caused a little bit of a problem in the general deliberation of the committee, which was not to work with specific quotas. We wanted representation, but we didn't want to be detailed about it. We wanted it to vary from year to year, according to strengths and so on and so forth. I kind of developed a style of this with this group. I guess I'd have to call it mock authoritarianism. I would make these—"We're not going to finish. Keep moving." I had this phrase that I was teased about all the time, which I would say after a certain amount of discussion had been going and there had obviously not been a decision—I would either say, "Well, it looks to me we're not quite ready on this. Let's wait a little while," or, "I think we really have said everything we can on this case. I believe we better bring it toward a vote." Then I would always say, "I'm just the timekeeper. I'm not really trying to railroad anything through." I really had to use these various techniques. I became the object of a lot of affectionate teasing about my role. Everybody liked it. Everybody, I guess, could get very many strokes over my leadership style in that organization, because, in fact, we've never failed to come up with a definitive list of awards each year, and feeling comfortable about what we've done.

We have a dinner with the trustees of the foundation the night before we begin our work, so we all get ready for the work, and then we have a collective lunch together on Saturday afternoon, after we finish our work. It's all in very good spirit. I've become close friends with everybody on that committee. There's a certain camaraderie that it developed. Very positive experience in my life. I really will miss not being in those meetings.

37-00:47:44

Rubens:

I imagine keeping up with the applicants, too, is rewarding, enlightening.

37-00:47:47

Smelser:

It's educational. You get to know what's going on in these fields and what are the leading issues, what are the controversies.

37-00:47:54

Rubens:

So this is during your period of doing the encyclopedia as well?

37-00:47:57

Smelser:

It fed into everything. Everything fed into the other, particularly the Center, what I learned by knowing who was at the Center and the fields of representation all over the vast areas in which they were studying, my work on the encyclopedia, the Guggenheim. They all fed each other, really, in fields which I had no business knowing about otherwise, because I would have been sticking closer to my own tasks. So it was all a very fortuitous series of involvements at the time. I believe you're suggesting that, and it really was. I wanted to say just one more thing about a couple of the tough cases that we had. In particular, this historian, she had a particular period of history in mind, and a particular area of the world that she was especially interested in, and a particular slant on gender studies, and so she was very active in this. I didn't

say she was unfair, but she certainly had kind of a different outlook than the general catholic one that prevailed. In particular, she engaged another member of the committee in constant battle. They had a lot of disagreements. I guess I'd say that was my biggest—

37-00:49:21

Rubens: Would it be improper to say her name?

37-00:49:24

Smelser: Her name was Natalie Davis. She was a French historian.

37-00:49:30

Rubens: She used to be at Berkeley. We have an oral history with her.

37-00:49:32

Smelser: She was here for many years, and I was a good friend of hers. She was then at Toronto. She left. Had to do with her husband. He was in the law school or something, I believe.

37-00:49:41

Rubens: And Princeton.

37-00:49:43

Smelser: Yes, that's right. She went to Princeton, but then she commuted a lot to Toronto, because that's where he was, and then when she retired from Princeton, she went to Toronto. I guess I'm not getting her into trouble by saying this. She was very strong, and obviously one of the most intelligent scholars that I've ever dealt with. That wasn't the whole story about her, but she tended to be the one who really took a more representative role, and it created a special problem for the chair.

37-00:50:18

Rubens: As I understand it's a fairly elaborate application, and you have to get recommendations.

37-00:50:28

Smelser: As applications go, if you take applications to things like the National Science Foundation and to federal agencies, it's relatively simplified. You have to list a narrative of your own career, of several pages. You have to list the project that you're intending to work on if you get a fellowship. You have to send in a vitae, and then you have to identify three or four referees who are then contacted by the foundation to write—I did a lot of writing.

37-00:50:59

Rubens: I was going to say, you must have had—

37-00:51:01

Smelser: I did a lot of writing as a younger scholar, but then we developed a rule of thumb that members of the committee could not recommend people, nor could we write letters of recommendation on their behalf. Sheer conflict of interest

consideration. We honored that. I got a lot of requests to write from people after I became active in the central committee, but I always could explain to them that I couldn't do it.

37-00:51:31

McIntosh: Is it a funding source that was typically designated for more accomplished and more advanced scholars?

37-00:51:39

Smelser: No. The criteria that we would review to ourselves each time and became the kind of culture of the group was that we were interested, above all, in rewarding accomplishment and promise. That's older and younger. Of course, there was the project in front of us, too. What was the project worth or worthy of? We always had the idea that when the chips were down, we would give accomplishment and promise more weight than the project itself. We never totally lived up to that. When we get to discussing a candidate, for example, we would just refer to everything, so in practice, you took all those things into account. I think you were asking whether or not there's a senior bias in it. We tried not to do that, but people who had not published more than a couple of articles, we said it's too early. You can always reapply. There was no penalty for reapplying, and we had some people who applied three, four, five—one guy applied thirty-six successive years and never got it, which I thought was kind of—

37-00:53:06

Rubens: Would you ever write critiques of their rejection? "You could have emphasized this"?

37-00:53:13

Smelser: We wrote critiques for our own—I took reams of notes on these people. We of course had critiques, and occasionally, to a person we gave a fellowship to, we would occasionally append some opinion that either reviewers or we had that we thought would improve the study. We didn't go into a long critique of those people who were rejected. We basically couldn't. We made the decisions. The staff of the foundation carried them out by informing. I never got in any trouble. Nobody ever came to me, thinking I had some enmity with someone in the field and I was going to punish them or I had some kind of power. This kind of phenomenon develops if you're head of an agency that does something like this. Often, it gets personalized that your own views might get involved. I really was very fortunate in staying clear of any kind of after-the-fact criticism from any scholars or fellow. Any informal communications we got, I tended to field them and say, "Bring them up with the staff." The staff was very willing to handle any conflictual episodes that arose out of the granting process. Happily sheltered from the uglier, or more political aspects of the process.

37-00:54:47
McIntosh:

A more general question. How essential do you see foundations like the Guggenheim being for the production of knowledge in the second half of the century?

37-00:54:59
Smelser:

It's a differential. The smaller foundations have been totally overshadowed by the government in some areas. For example, I was a member at an early stage of my career, thanks to Gardner Lindzey, with a small foundation called the Foundations Fund for Research in Psychiatry. It was a small endowed fund that met in New York and gave out money to psychiatrists and to other psychologists and other researchers, roughly in the psychiatric area. It got totally overwhelmed by the National Institutes of Health. There began to be falling off of applications because our stipends were smaller. It finally went out of business. They went out of business and gave their money to the Center. The Guggenheim doesn't fall in that category. It's a big foundation. It's fairly rich. It gives a lot. It fortunately has one of these reputations that carries on no matter what, even though its stipends could be larger and so on.

I have to give you a qualified answer, because during my time at the Guggenheim Foundation, I began to get more and more convinced that we ought not to be in the business of giving grants to certain categories, like physical scientists, for example, or medical researchers, for the same reason. They applied in certain numbers. Not huge. They could get much, much more money from every other place, NIH particularly, and NSF. The Guggenheim, they give you time off to write, or time off to do this. It didn't exactly apply to these huge labs that everybody was running. I actually raised the question openly with my committee, and talked informally to some of the trustees at the dinners. I said, why not make this more selective? This organization does such value to humanists and poets and writers and so on, and we give a lot of them to these—and the performing arts, which aren't supported in these ways. Why not shift our emphasis to where we do the most good? I even suggested, well, maybe the economists don't need us, because they have so many other sources of support. I had a pretty articulate view on this, but it never got listened to. The historical commitment to covering the field was so kind of unconscious and strong that I didn't make any headway.

A more general answer to your question is they do a lot of good. I believe that the presence of these private funding organizations that support scholarship outside the federal government are extremely important and healthy. They tend to be less bureaucratic. They have, in a way, more freedom than the large funders. I would be very positive about the role of Guggenheim.

37-00:57:56
McIntosh:

Especially in terms of the humanities, it seems.

37-00:57:58

Smelser:

Yes. The first part of my response had to do with areas of the world which it does the most good, and that's both because of its own support and because of the impoverishment from other sources. In psychology, which I was responsible for, we got many, many more applications from people who couldn't get money elsewhere. We didn't get many applications from the experimentalists, who would go to the Institutes of Health and other more bountiful organizations. We tended to get the ones in clinical psychology. Maybe the social psychologists would be over-represented in our lists of psychologists, because they had fewer outside alternatives.

37-00:58:49

Rubens:

Would it be an ambition for you to be a trustee?

37-00:58:55

Smelser:

A trustee of Guggenheim? I don't think at this stage of life. It still could happen. A former member of my group, Jean Strauss, who wrote this very excellent biography of J.P. Morgan—she's one of my favorite people in the world—she was a member of my central group for many years. She did literature. She was responsible for the fiction and nonfiction. A wonderful woman. She was a freelance worker in the whole world. She had to resign from the committee, largely because she was spending too much time on it and she had to earn money elsewhere. Then she got named director of the residential scholar group at the New York Public Library. It was kind of equivalent of my Center leadership. They have a modest residential program at the New York Library. She's rewarded. She was then put on the board of trustees. It's conceivable. I have very little interest in being on a board.

[Begin Audio File 38]

38-00:00:00

Rubens:

Let's move to your work on the SSRC.

38-00:00:17

Smelser:

I was invited, in 1997, I believe, about then, after I had begun work on the encyclopedia, to rejoin the council of the Social Science Research Council. That's the governing board. I had started work on the encyclopedia with Paul Baltes. Paul was the chair of the Social Science Research Council. He persuaded Craig Calhoun to put me on. He said he's just an obvious candidate. As you know, I had a long ago past history, including chairing that council for a couple of years, in the late sixties and seventies, but I hadn't been active other than giving them advice on candidates or projects to support and so on. I hadn't been active in the SSRC in the interim. They re-invited me to become a member of the central governing council, which, of course, was responsible in much the same way as my board for the fiscal affairs of the SSRC.

Craig Calhoun was the head of it at the time. I'd had an earlier relationship with Calhoun. He had written on nineteenth century British history, and he had written a critical review of my doctoral dissertation book. Then when we were together on that German-American theory group, I asked Calhoun to come to one of the meetings. He told me later, he said he was scared to death, because he had been this enfant terrible, attacking this great historical work, and he was afraid I was just going to squash him like some bug. Well, what the hell? That's that. He and I then developed a new and different relationship in that experience with the German-American theory group. He was perfectly comfortable to have me come back on the board. We developed a good, collegial relationship that has existed ever since.

I don't want to spend too much time on it. There was nothing particularly historically critical or noteworthy that I would want to report on. I had been on the search committees for Social Science Research Council leadership in the interim, in the eighties, a couple of times, and been active in the choice of Frederic Wakeman. He was chair of SSRC for a few years. He was there for three years.

38-00:03:15

Rubens:

He was in the Berkeley history department.

38-00:03:16

Smelser:

Berkeley historian, Chinese historian.

38-00:03:19

Rubens:

Who died, tragically.

38-00:03:20

Smelser:

He died. It was tragic. I liked him a lot. He was in another seminar with me on the Berkeley campus for years and years. A wonderful man. Brilliant historian. SSRC was in a big transition. It was one of these organizations that had gotten itself into this situation of having not very much money, but being a longstanding—it's the oldest social science body for research in the country. It predates all the federal and foundation activity in the social sciences. It has a very noble past of initiating extremely important areas of research. The SSRC got involved in this dilemma of what to do now that so many other sources were giving out money in the social sciences. They developed this area studies program, in which they gave out fellowships to people studying in different parts of the world. That was one of the directions they took. Even that began to be a competitor with the defense department fellowships, and language studies and so on. They were in the process of a new, longer readjustment when I came onto the board, and that was internationalizing their activities rather than being yet another supporter of research in the United States. We sort of superintended that increasing involvement in international meetings and collaborating with other international bodies and financing research in their own countries and so on and so forth. Interesting episode, and of course you always—

38-00:05:09

Rubens: How long did you serve?

38-00:05:11

Smelser: Two three-year terms, and then left.

I was also brought in to the German-American Academic Council in 1995 or six. I think my directorship of the Center was probably at the heart of it. This was an organization that was a brainchild of Chancellor Kohl of Germany. He persuaded Bill Clinton to set up a collaborative German-American—there was some feeling that after the Cold War, after the unity between Germany and the United States, all during the Cold War period there was a danger of a falling off of these close relationships, and this initiative was one of the initiatives by Kohl to keep the friendship with the United States going. So we had the German-American Academic Council. Clinton was extremely excited about it and pledged to co-support it, but Clinton managed to end his involvement at that point and produce no money to match the German funds. Kohl went ahead anyway, he felt so strongly about this. So this was a German-American collaborative group, financed entirely by the Germans. It created a few problems that we were not really fully in it, but nonetheless there were about half American academics, half German academics.

38-00:06:32

Rubens: How many on the council, about?

38-00:06:34

Smelser: Twenty-some-odd, I think. All fields. We would meet two or three times a year. There was some money that they gave out, modest amounts of money, and they supported conferences and they supported little programs. I was part of a special conference on higher education in both these countries that got carried on for a couple of years. It did some good. It had a lot of very eminent people. Gerhard Casper said, “You people ought to be doing more important things, because you’re wasting the time of all these brilliant scholars who you’ve got on this committee.” He might have been right. I’m not sure. Nonetheless, it was a source of personal gratification for me because I got a little money out of them, actually, for the Center at one point, for a couple of special projects at the Center. It was, in a way, a kind of conflict of interest on my part, but somehow or other these continental scholars didn’t see it that way. I even suggested that I step out of the room while they made a decision on this grant. Good American conflict of interest guy. They said, “What are you talking about?” This was the gentlemanly club. It ended up being killed by the German government because of excesses in spending. They would do things that were out of keeping with German law. They took us all to the opera in Munich.

38-00:08:01

Rubens: They’d obviously pay your way over.

38-00:08:03

Smelser:

Everything was paid. I didn't get a stipend. But nonetheless, they entertain you very well. It came afoul of the German foundation and legal system, so instead of punishing the wrongdoers, they killed the organization. That lasted about maybe four years. A nice little episode on my part.

38-00:08:21

McIntosh:

Were there other members, either German or American, with whom you'd had previous relationships?

38-00:08:27

Smelser:

Yes. I'll name one. I developed some new relations during it, but the one that I had already was Fritz Stern, the German historian from Columbia. He had been kind of an office—not mate, but an office neighbor in my year at the Russell Sage Foundation. We hit it off very well indeed. That was the year of the collapse of the Wall and the end of the Cold War. Fritz, of course, was being called on by every magazine in the world to comment on this historically. He and I developed a mutual respect and interest during my year there at the Russell Sage Foundation. He was a kind of soul mate of mine on the German-American Academic Council.

38-00:09:12

McIntosh:

Do you recall any of the other members, particularly Americans?

38-00:09:20

Smelser:

There were a couple of members whose names I cannot recall, who were, in effect, representatives of the National Research Council. That was an organization that was honored there. A couple members of German Parliament were members. I will not be able to remember their names. A first-class humanist with a Polish name with about sixty letters in it was in it that I cannot remember either. Nonetheless, it was an intellectually congenial and civilized group.

38-00:10:05

McIntosh:

And as you said, certainly an honor to be appointed to.

38-00:10:12

Smelser:

I hosted the whole group at the Center once during its existence when it was meeting at Stanford. I had them all to lunch at the Center, and introduced and told them about the Center as an organization. I believe it was a nice interlude. Once again, piled upon the millions of things I did while I was director of the Center.

I believe you wanted me to talk about a few general reflections on being director of the Center, and I can spend just a couple of minutes on it. I believe I'd like to say that I've always been ambivalent toward the nuts and bolts of administration in the academic world. This probably explains the fact that except for the Center directorship, my only directly administrative staff relationship was chairman of the department. I turned down deanships, many,

and some presidencies, as we've reviewed. I always felt that it was going to wreck my scholarly career if I got into this kind of full-time administrative responsibility with a lot of boring aspects and a lot of conflictual aspects in it. So, in the end, my career left that out. I actively wanted to be director of the Center, however. Here was an administrative job that had full administrative responsibility. It was like a smalltime CEO. I had a budget responsibility. I had a staff I had to hire and fire. I had a board of trustees I had to be responsible to. I had to shepherd all these fellows in one way or another, dealing with incidental problems that arose in their lives, and making sure that the Center was a running, going concern. So I was a full administrator, and I turned out to like it. I never developed a full, libidinal relationship with fundraising, but I liked it when I got the money. That was very gratifying.

Furthermore, it was so infused with ongoing intellectual relationships that I always would tell people I didn't miss teaching at all, because I was forever in the world of ideas. I was evaluating people in the world of ideas, being with them on a day-by-day basis, collaborating in some cases as a fellow myself. So I was involved, and able to keep up a stream of scholarly work. After all, books were published and articles were written and the encyclopedia was edited during that period, so I did not lose touch with my first love in the world, which was my scholarly life. I was involved in it in many, many different ways, and didn't find the strictly administrative aspects alienating, the way I had spent my whole life dreading.

38-00:13:04
Rubens:

I wanted to ask you if there are any particular special projects or people that came through the center that you've kept up with, besides Jeffrey Alexander.

38-00:13:15
Smelser:

Alexander. I already had relationships with Gary Marx and Christine Williams, the editors of my *festschrift*. Both of them were fellows during my time there as well. There were a few more people who I knew at Berkeley and so on. Let me see. It's a hard question. I don't know that I can really begin to make the kind of distinctions about very special relationships. I had a lot of relationships with fellows that turned into having a personal side. We would go out to movies together and we would make a point of seeing one another at lunch and so on.

38-00:14:18
Rubens:

We'll talk about the *festschrift* later. We'll turn to the work on terrorism, then.

38-00:14:29
Smelser:

We came home. The board, in a gesture of generosity—my retirement date was September 1. They said, forget it, go home the end of July. You've done your job. Go home. So we moved here the first few days of August.

38-00:14:47
Rubens:

Did you have a hand in your successor at all?

38-00:14:51
Smelser:

Basically, no. When they were down to the finalists, I was asked to comment on the last two or three finalists. They had a strict structural separation, which I liked very much, between the involvement of the past director and choice of the new one, which is a healthy way to do it. Anyway, no. This was Doug McAdam. He was a sociologist. He was a Stanford faculty member. He was my successor. He came in. As I say, I was put on the board for six more years as a courtesy appointment. I went to all the board meetings for six more years.

We moved back into this house and were resettling. I didn't have a moment-to-moment agenda. The chair of the sociology department had asked me to teach one seminar for the first semester, which was a special seminar to graduate students who were in the writing stages of their dissertations, and I was to be kind of a guide, a writing guide, to dissertation students. I had about eight students. We met in this house, very informal, but it was a course. He asked me to do it, and I said, that's a nice transition back into this community, so I decided to do it. It was enjoyable to be working with these students. I kept contact with one or two of them.

But here we were, getting ready to go on a trip, and September 11 came. We were just about to get on a plane to go to Oslo. I had been asked to be a lecturer at the University of Oslo for a few days, and we had planned an additional week of traveling around the Norwegian countryside, going to look up the little town where Sharin's ancestors were born, which we did, but only later, because on September 11 you could not get on a plane. Our plane was to have departed on September 12. Everybody in Norway who was going to be my host, two or three people there, oh, they were so concerned. "Please, just name the time. You can come back later." I said I didn't want to come in the winter, so we rescheduled for the following June, and we did take that trip as planned.

However, here we were. The TV was blaring. People were going crazy. Phone calls. The usual thing after 9/11. Here we were, scheduled to be away for ten days. So what do we do? I said to Sharin, I do not want to sit here and get the CNN syndrome, as they called it, of watching all the news and repeated news and fake news and invented news that comes on after such a national crisis. These empty ten days, which I had emptied out to go to Norway, I said, let's get in the camper. Let's just skip town. We went up to Seattle and visited our son, who was working there in the Seattle Rep Theater at the time. We spent three days with him. Then we just kind of wandered around in the camper. I felt this interesting compulsion to go to Mount St. Helens. Symbolism is pretty obvious. Violence in the world. Which we did, which in fact turned out to be a really quite moving sort of thing to see the effects of that devastation. We listened to the news all the time, of course, in the camper. I write this up in the odyssey book. I said this was an enforced little odyssey that we put on to ourselves.

I mentioned that the National Academies swung into action, more or less immediately. They then decided, well, what should we do if we're going to take some kind of helping or constructive role in this national crisis? They decided to do what they could do best: to set up, on their own initiative, one of these kinds of panels that I talked about as being usually the product of the divisions of the NRC. They decided to do this either with their own money, or they got some money out of the office of the president—something. They decide to have a really major panel to prepare a report, which mainly dealt with defending against terrorism. The project was called “Making the Nation Safer.” It did not have much to do with where terrorism was coming from or how we might understand it from the standpoint of its origins or the social conditions of the parts of the world in which it might be emanating. It was more on the defensive side.

Of course, it was academy-wide membership. Most of the members were scientists, engineers, people from the National Institute of Medicine, because there's a public health aspect, obviously, to defending against terrorism, and some members of the federal government who had been in either the state department or in some agency that had been kind of responsible for some kind of security aspect. There were not more than three or four of those, but they were another significant element on the committee.

There were two social scientists. Tom Schelling, the economist, who was basically the father of deterrence theory, was an obvious choice. He's a social scientist in a somewhat narrow sense of the term. A technical economist. He had been responsible for the development of models and theories of deterrence. He was sort of the parent, really, of that, and he subsequently won the Nobel Prize, primarily for this formal work in the theory of terrorism. I had known Schelling a little bit before. I was a little bit afraid of him. He was this kind of formal intellect. It's quite clear that he considers himself extremely brilliant, and he doesn't suffer fools gladly. Didn't exactly frighten me, and we developed a good relationship, but nonetheless. He was the only other one than I. His views were much more technical than mine. However, I was an obvious choice. I was centrally involved in the academy anyway by this time. This was already in my years after a long period on the council and directing DBASSE. I was visible. I was at all the meetings. Furthermore, my past involvement in movements, collective behavior, mass behavior, which I was still known for, even though my original contributions were now approaching forty, fifty years ago. Still, this was one of my fields, so I got chosen to be on this master committee.

The master committee spun out subcommittees, sub-panels, to deal with different facets. They had a public health subcommittee. They had a subcommittee on cyber terrorism. They had a sub-panel on protecting physical infrastructure of the society, and so on. They had one on understanding terrorism from a behavioral and social science point of view. They immediately appointed me chair of that, with Schelling as not co-chair but

member, but then we chose additional panel members for the special subcommittee. I was chair of that. On top of that, the defense department, DARPA, our organization within the defense department, decided it wanted to have some work done. They gave money to the academy to spin off another subcommittee on terrorism to study the relevance of deterrence theory for understanding terrorism, or deterring terrorism really. They made me chair of that, and it was quite funny because they made Schelling a member only.

38-00:23:49
McIntosh:

Why was that?

38-00:23:50
Smelser:

Don't know. I have no idea. Schelling was a member of equally good standing in the parent committee. He could have chaired that himself. Maybe they figured that the number one expert shouldn't do it because he'd be too involved. Who knows? I never got privy to this. That committee also spun off, and I became chair of that. So I was on three committees, all dealing with some central aspects of terrorism.

38-00:24:24
Rubens:

The master and these two—

38-00:24:25
Smelser:

Master and the two subcommittees. I was a member of the master, chaired by a physicist, Lewis Branscomb. I had developed a pretty good relationship with that man. He was at the Harvard School of Public Policy.

38-00:24:45
Rubens:

Where was Schelling?

38-00:24:54
Smelser:

Schelling was retired, but now at the University of Maryland. Still active, teaching at the University of Maryland. He retired from Harvard.

38-00:25:05
McIntosh:

Am I recalling correctly that you had done some work with DARPA before? Wasn't there a paper that you wrote on nuclear—

38-00:25:17
Smelser:

Yes. I don't know whether that was DARPA at the time. It came out of the defense department. I was dealing with a man who was responsible for commissioning quite a few different working papers on issues of social change and security. Yes. I had had this relationship, and I believe we talked about it. That, I don't think, figured into this current one.

38-00:25:46
Rubens:

Also had the experience with the national labs, and a clearance.

38-00:25:48
Smelser:

The labs. I, of course, was closely associated with the labs, and that was clearly relevant to it as well. So there was logic in my being so involved in all of these things. Well, where to begin? The first thing to say is that I had the idea that being in the parent committee was going to be—I sort of had the feeling that I was a token, along with Schelling, and Schelling really was there because he was something other than a general social scientist. I didn't expect to have any particular say, because most of the interests in defending against terrorism have tended to be with technical scientific gadgets. Dogs sniffing, devices, radar. prevention. All kinds of things like that. Security on planes. How to keep electrical grids protected and so on and so forth. That was the dominant theme of this whole committee. The social aspects, I thought, were just going to get very short shrift. I was wrong. I think I've talked about the whole culture of the National Academy as scientific. Well, okay. Make my voice heard. It turned out I was really wrong. The diversity of people who were on the committee, with a couple of exceptions that I'll mention later, were very sympathetic to the social aspects, particularly responding to terrorism, because that was the basis of this committee, was responding rather than understanding where it's coming from. I really felt kind of surprisingly welcomed in that committee. What I said was really quite listened to. Immediately, I was assigned the job of drafting the entire chapter on human responses to terrorist attack. That was my understanding, and this was going to be a parallel chapter with maybe ten or so others in the book, and that was it. Furthermore, even in the kind of technical sides, I would contribute my own ideas. I really developed a feeling of full acceptance as a committee member, which I had my doubts about because of the fact that it was taking place within the National Academy of Sciences.

We had a very interesting episode having to do with another aspect that never got covered. Some of us thought we should at least have a few words of introduction about where terrorism was coming from in the late twentieth and early twenty-first century. This meant getting into special international terrorism and its roots. It was generally thought to be a decent idea by almost everybody. I was requested to draft an introductory section for an introductory chapter on where terrorism was coming from, and I borrowed from my subcommittee, my sub-panel. We were considering this full-scale in my subgroup, so I profited from those discussions, plus my own ideas about the social conditions which might breed this kind of thing. Issues that came up in my terrorism book full-scale later. So I wrote a little draft, an objective draft, I thought. I handed it into the chair, Lewis Branscomb, for draft material for the first chapter, introductory chapter, orienting chapter. Two of the members of the committee, both of whom had had service in the government, took violent objection to that material, saying it was an excuse for terrorism. Flabby social science excuse for terrorism. They were very militant about it. They were so militant that they drafted an alternate chapter, which treated terrorism as basically immoral, fascist. It was a complete evaluative blast of terrorism as criminal. All evaluative. That was their view of where it was coming from.

They went to the chair of the committee, Branscomb, and told him that if my draft went into that report, they were going to not only resign from the committee, but make a public stink about it in defense circles.

38-00:31:10

Rubens:

These were not academics? These were—

38-00:31:11

Smelser:

These were former—the man who made the most opposition to this was named Jerry [Lewis Paul] Bremer. He turned out to be the czar of Iraq after the invasion later. His view was that this was going to damage our attacks. To treat it as a problem to be understood was a way of apologizing for it. That's the thread in the literature, of course. It's kind of an anti-social science thread in the literature. Well, the chair was kind of in a box there, having these two versions of what to do, so he and his co-chair did the only thing they could do. They killed them both. However, they left our subcommittee alone. A part of the understanding of this spinning off of sub-panels was that the sub-panels made their own reports and they were not going to review them even. They were independent. We, in our own sub-panel, which came out to have a small book published out of our work, along with a major book, called *Making the Nation Safer*—we had a smaller volume that I was coeditor with a staff person on. Developed all we could say about the nature of terrorist activity in the contemporary world. So I got my way, but I got my way in another publication. This was a bit of a crisis in that committee, that because of the heat of the politics at the time, really couldn't—I have several biographical boxes in my book on terrorism, and I write up this episode, without naming Bremer as my chief adversary.

38-00:33:03

McIntosh:

Could you summarize what you were saying in that explanation?

38-00:33:09

Smelser:

Yes, I'll give you a couple of points. I was saying that I developed the idea of the necessary condition of some level of disaffection in the societies in which terrorism came to be the choice of behavior. Dissatisfaction or alienation or disenfranchisement. I took the view this wasn't necessarily a simple function of poverty. The fact that rapid social changes were going on in these societies and there were multiple cultural expectations being generated, partly imported from the West, partly internally generated, and partly reactive to those changes, that created a kind of cultural confusion, and in some sense, cultural conflicts in these societies. That there were certain groups that felt disenfranchised, often based on separate ethnic or national aspirations or identifications within the larger nation. Then I also laid out an analysis of what alternatives to violence were available in the contemporary world, with the superpower competitions finished, with basically one superpower, against which nobody in the world could wage war. The CIA itself said if the whole world went to war against us, we would win. That was the CIA's view of the

situation at the time, because we were the sole commanding superpower at the time, end of the twentieth century, early twenty-first.

I started talking about alternatives to terrorism and how, systematically, each one of these is kind of ruled out by the contemporary international situation. I used a phrase that I later quoted, that terrorism is, in a way, a protest of the weak against the strong, that they do not have the capacity to wage wars. Armies are, in some sense, irrelevant. So this was the line of analysis that I was taking, written in very brief scope in this analysis, which I developed into a really pretty large theoretical diagnosis in my own book. Half of my book was dedicated to where terrorism comes from. Not just international terrorism, but terrorism in general. The second half of the book was dedicated to responses to terrorism. It was a kind of understanding, as you see from the tone of what I'm saying, it was an effort to understand from an objective view, if possible. The mentality was such at the time that understanding got pretty close to apologizing. So that was the basis of the big fight.

38-00:36:10

Rubens: How soon after September 11 were you meeting with this master committee?

38-00:36:11

Smelser: Oh, we were in business within two or three months. We had a self-imposed deadline of one year. The report was published in one year's time, 2002. *Making the Nation Safer*. It was really on fast track.

38-00:36:26

Rubens: How often were you meeting?

38-00:36:28

Smelser: We would meet every six weeks to two months. Then I would try to combine my meetings with my panel with those meetings so I wouldn't be traveling all the time. A very funny thing happened in connection with my deterrence committee. It was defense department, DARPA. They, of course, wanted advice. At our first meeting, which was not too long after it was formed, the head of DARPA came for the first meeting. He knew I was chair. We had a private conversation before the meeting, and he laid out a few expectations and so on about what he expected from the meeting. He said, "I would like a report of one and a half pages. Not longer." This was on the whole idea of how relevant and effective the idea of deterrence is in dealing with terrorism. I told him, I said, "I'm not sure we can do that." Then I said, "Won't you please stay for the whole meeting today? How much of the meeting can you stay for? See what we're thinking about and where we might be going." I invited him to stay.

The first meeting turned out to be so rich, so intellectually interesting, so many interesting corners explored, and obviously the complexity of deterrence in light of what kind of ideologies were generating within the terrorist movement, how much they even trusted the enemies—which is a certain

element in deterrence theory, is that you have credibility, you believe what the enemies are going to do. Schelling gave a beautiful characterization of traditional deterrence theory, and he wasn't convinced it was relevant to terrorism in all its regards either. The head of the department, his name is Tony [Anthony] Tether, the head of DARPA, got really engaged and was really learning something, and I was delighted that he was learning something about what the substance was. Well, he had to leave about 2:30. We were going to meet until about five. He had to leave about 2:30, so he excused himself and came around the table, left, paused, spoke to me quietly in my ear as he was leaving. He said, "Page and a half." It didn't penetrate him at all. As it turned out, we wrote a whole lengthy report.

38-00:39:17
Rubens:

Did you tape these meetings? Was there someone taking notes?

38-00:39:26
Smelser:

They may have been taped. I never referred back to any taped record. I took my own notes, assimilated things in my own mind, and so on. I drafted a full report of both those committees, and we had a final meeting in which the final text was reviewed and approved. As with almost all the panels and committees I've been on, I was the author. I listed Faith Mitchell as co-editor. She was a staff person who was assigned out of the National Research Councils to meet with the committee, to arrange its logistics, so on and so forth. She was engaged in it. It's a custom to list the staff member as coeditor of the report. I was responsible for the drafting. Put it that way.

38-00:40:27
Rubens:

So the one, "Discouraging Terrorism: Some Implications," that's the—

38-00:40:35
Smelser:

That's the social and behavioral sciences sub-panel, and the other one is called—

38-00:40:40
Rubens:

"Perspectives from the Behavioral"—

38-00:40:41
Smelser:

Oh, no, that's the general subcommittee, the "Perspectives." "Discouraging" is the deterrence one. I fought for the term "discouraging" because it was a much more open-ended multiple strategy kind of a term than "preventing," "deterring," whatever.

38-00:41:00
Rubens:

Were there political scientists involved? You said there were only two social scientists.

38-00:41:07
Smelser:

Yes, on my subcommittee we had a political scientist of Arab countries. Her first name escapes me. She was a very helpful member on it. We had a man by the name of Ira Lapidus, Middle Eastern historian.

38-00:41:27

Rubens: From Berkeley, too.

38-00:41:29

Smelser: Berkeley. He had been on a lot of things with me here on the Berkeley campus. He was on both the deterrence and the social science subcommittee.

38-00:41:45

Rubens: You picked him?

38-00:41:46

Smelser: Yes, I picked him. Gene Hammel was another one from Berkeley, an anthropologist. A demographer anthropologist. A very, very brilliant guy. There was a man from the University of Maryland, Arlie Kruglanski, a psychologist, who'd gotten interested in terrorism. Anyway, Lapidus, I asked him to serve on the deterrence committee as well. He got very nervous. He didn't want to be on something that DARPA was sponsoring. Comes out of the left. Didn't like the defense department. Didn't like working for the Department of Defense. He wanted to resign from everything. So I took him aside. We'd had a long, friendly relationship here at Berkeley. I said, "Ira, why don't you just quietly resign from the DARPA committee and not the other?" He didn't have any objections to the other. It was just an NRC committee. So he was willing to accept that. Rather than make a big stink about DARPA shouldn't be involved in this kind of business, et cetera, et cetera, he just quietly didn't come to those other meetings and was not listed as a member.

38-00:43:02

Rubens: I was trying to think of one other name of a political scientist –Chalmers Johnson. I think he ended up at George Mason. He coined the term "blowback." He had been a proponent of the war in Vietnam, and then he recanted that position later in his life –somewhat like Robert McNamara. It sounds like your analysis in the introduction accounted for blowback, accounted for reaction to neo-colonialism.

38-00:43:43

Smelser: Oh, yes, very much so. I made a big point of the principles and how colonialism had left a residue of states whose boundaries more or less coincided with the colonies, and that turned out to be a disaster because they had no social-psychological base for national identification at all. They just were boundaries of the colonial powers, and of course Iraq is one of them, one of the prime examples.

38-00:44:11

McIntosh: Can we talk a little bit about your visions about human responses to terrorism? We talked about the part of the report that got omitted, but what about your contributions that were included?

38-00:44:31

Smelser:

In the report, we talk more or less about short-term psychological responses, because that was what was going on at the time. I began to develop the ideas that came into full flower as I worked on my terrorism book. These had to do with identifying some of those conditions which make terrorism terrorizing. I laid out certain kind of features of terrorism which tend to generate the highest levels of anxiety. These had to do with events which were unpredictable, rare, and lethal. You have to have all three of these to get the full-fledged kind of terrorist apprehension about terrorism. I appealed, of course, to the social-psychological and personality literature in making this particular set of choices, and tried to indicate as to why these particular characteristics contributed to the limitlessness of the fear. That there were few reality checks involved in this. It was itself, in a way, a fantastic invention on the part of the terrorist to combine these ingredients into one package that gave rise to an especially high level of apprehension. This was all in the area of threat, the ambiguities that are involved, and the fact that you can't really predict when events are happening. I likened this to more like an earthquake than a hurricane. There's absolutely no advance preparation, and that's what the terrorists want, is not to have any advance preparation, to maximize the impact of terrorist activities.

Then I borrowed a lot from the known literature on disasters as to how people initially behave in the short-term. What the immediate adaptive responses are psychologically, in terms of what people want to know and insist upon knowing, and if they don't know it, what they're likely to do. The importance of where their immediate family and friends are at the time something happens. What's called convergence behavior, where, at a spot of attack, there tend to be people who converge together, both out of curiosity and out of altruism and out of less noble motives, such as looting. We covered a lot of those aspects of responses to it. I got very much into the business of patterns of scapegoating after disasters, particularly when some responsibility might be assigned for who didn't do what to prevent it, or who didn't do what in immediately responding to it, or how are we going to prevent it next time. It's an analysis of the general social, psychological, political, spilling over somewhat even into the economic implications of 9/11, for sure, which were quite clear. There was enormous damage to the travel industry. Of course there was damage to buying patterns, to economic apprehension, so on and so forth. These were some of the lines in my book. I undertook longer-range considerations, such as the degree to which the target powers might want to intervene, in what ways, into the origins of terrorism, if at all, and what the larger foreign policy implications of American position in world society were. We didn't talk about that in this short-term report. It was more geared to the short run.

38-00:48:33

Rubens:

When did the idea that you would write a book—

38-00:48:42
Smelser:

I didn't intend to write a book from the beginning. I thought these reports got my views in there as much as I wanted. It was really members of the staff of the NRC, the DBASSE staff, who told me that it would be a help to the world if I were to gather my most advanced ideas and elaborate them into book-length form, to make them available to scholars and the public alike. At first, I said no, I didn't want to do it. I was antsy to get back to the odyssey book. I don't know. I said, well, I've written a lot of the things I'm thinking about. So I resisted. Then I just kind of floated along. Didn't do anything. Then the National Academies Press decided to initiate a series called "Essentials of Science"—or "Science Essentials" was the title of it. These were short books, written mostly by natural scientists, on topics to be expressed in lay, available terms in their own areas. There was one on genetics. There was neuroscience. There were others that were going to be in that series, all of which were rooted in the physical and life sciences. I was on actually a committee with the Academies Press to help them decide on topics. They asked me to come in as a member of the National Research Council, in any event, to advise them on this series. It was out of those conversations in which I was advising them on topics such as demography, practical applications and understanding of demographic processes, and other things in the social sciences that I knew about. The subject of terrorism came up as one of the things on which the social and behavioral sciences might have something to say. The publishers themselves began agitating with me to write a book in this "Science Essentials" series. That, plus the cumulated advice from the time before, more or less turned me—okay, I'll do it. I didn't exactly force my way into writing this. It was going to come out published by the "Science Essentials" series.

So I went to work on it. It took me about a year and a half in additional work. I had to consult a lot of literature. I discovered how little sociological literature there is on the subject. I tried to make some sense out of that. Most contributions had come from political scientists and psychologists and historians of the topic. I decided to cover something other than contemporary international terrorism, so I had sections in there on terrorism during the sixties in the United States and seventies in Germany and Italy. I went back into some historical episodes of terrorism to try to make this applicability wider, and didn't want to limit it entirely just to the international terrorism that had been invented in the late sixties by Arab countries, mainly anti-Israel, to get the attention of the West, as its origin, and then developed up to 9/11. I really wanted to make it a more general treatment, and so it required a lot of additional reading in the literature on my part.

So I wrote it. I sent it in at "Science Essentials." In the meantime, "Science Essentials," as a list, was having a horrible time. They weren't selling. It was a strange enterprise that just proved to be commercially unviable. The academies were not a profit-making organization, and the National Academies Press was subsidized, but this was too much. So they decided to discontinue it. Here I was. Have a full manuscript in their hands. What to do? I was

obviously an author. I was terribly frustrated, as you can imagine, by having this thing held up. This timely book that I had entered into ambivalently in the first place. It was now not going to be published. I started making noise and I said, “How can I withdraw this and turn it over to another publisher?” They said, “Please wait, please wait. We’re trying to sell the list to a publisher. Don’t withdraw it yet. Give us a little time.” So they began marketing the book to some publisher who might take the books, the whole series, “Science Essential” series. They had contacts with Berkeley. They had contacts, I think, with Columbia Press. They had contacts with Princeton. So I went along with their request to wait a little bit, but was always threatening to withdraw it. I knew UC Press would take it in a minute if I approached them as an author. It wasn’t my personal panic that this was not going to get published, it was just impatience over all the delays.

In the meantime, the academies really went cracking on this, not because of me but because of their own interest. They sold it to Princeton almost right away, and Princeton took over the list. Immediately, I did have it reviewed by Princeton. They had to do their own procedures, and I had it reviewed by two outsiders, both of whom liked it and asked for few revisions and corrected a few mistakes that I’d made, but Princeton then picked it up and published it, more or less on the same schedule as the Academies would have.

38-00:54:30

Rubens:

2007 is the date of publication

38-00:54:32

Smelser:

Yes. I finished writing it in, I think, late 2005. So there was a year, year and a half delay, but university presses, they don’t set speed records anyway.

38-00:54:45

McIntosh:

I’d like to get your take on what you see sociological analysis as offering in terms of understanding terrorism. You mentioned that the literature in history, political science, and psychology had addressed terrorism, but what did sociology bring to the table that those disciplines have not?

38-00:55:10

Smelser:

I’ll answer your question not literally, by saying, well, here’s what sociologists can say. Obviously, the whole study of revolutionary movements and social movements was so closely relevant to this, and sociologists have really taken the lead in this, even though there is an interest in political movements within political science, and the social psychology of collective action in psychology. Sociologists have made a genuine and independent contribution to this, and I think I summarized a few of the kinds of issues which sociologists had developed—relative deprivation and theories of revolution. So there was something definitely to be contributed, but I advertised this book as not a disciplinary enterprise. I made comments on why sociologists hadn’t attended to it, and I can perhaps say a word about that. I did say that it was very essential to get the human dimension in. That we were

so preoccupied with momentary policies and the technical side of terrorists that that human dimension was really being systematically ignored in research, and that was giving us a very partial understanding of what the dynamics of the terrorist phenomenon is if we don't bring the social and human sciences in, full-scale.

So it was meant to be a corrective to what I saw as a kind of national basis, an instrumental basis. We can handle this problem the way we handled the Manhattan Project. It's a full-scale technical innovation. Turn it over to the engineers. We'll do it. We'll get the right formulae. That's not the story. What I did, I just borrowed, totally independently, and the title said *Social and Psychological Dimensions*. I didn't want to get territorial about our own social sciences, so I deliberately put it in, which was very comfortable to my own style as a social science enterprise.

38-00:57:26

McIntosh:

Great. We're, I think, close to the end of this tape, too.

38-00:57:30

Rubens:

What was the response to the book?

38-00:57:39

Smelser:

The reviews that I got were generally of a positive sort. There was a lot of commentary on a lot of the original insights that I brought to bear. I don't regard any wholesale assaults on the book. I didn't get called into Congress to testify. However, the National Academy continued activity on terrorism itself, and I became a member of a National Academy panel that organized various public panels and open symposia in different communities that developed scenarios if there were a terrorist attack in that community. We involved the press. We involved the local law enforcement officials in writing scenarios. For example, a dirty bomb being dropped not far from or near the Chicago Trade Center. Port damage in a different city. I was involved in designing these for different cities and went to a couple of them.

38-00:58:51

Rubens:

Did it sell well enough?

38-00:00:08

Smelser:

The book? I don't know. I don't know the circulation figures.

38-00:58:59

McIntosh:

Just curious—do you feel as though the nation is safer today than it was back in 2001, 2002?

38-00:59:08

Smelser:

Yes. This is a relative statement, not an absolute statement. You don't plug every leak. But the fact that the nation engaged in a great deal of upgrading of its defensive apparatus, preventive apparatus, border checking, planes and so on, while it can be very imperfect—I mean, really imperfect—and committed

a number of undesirable political effects in doing it, if you look at the dimension of safety alone, I would have to say that the country is blessed by the unknown—it's a judgment of faith on my part—it's the unknown extent of the degree to which the danger has gone down. Plus the fact that we have, relative to European countries, relatively few homegrown terrorist activities. A lot of invasion of some civil rights in keeping track of suspected groups for sure, and I don't excuse that at all, but to answer your question, I have to be affirmative in my response.

38-01:00:16

McIntosh: That might be a good place to end.

38-01:00:17

Rubens: Good, good. I was going to ask you about the Patriot Act and invasion, but you just answered that.

38-01:00:22

Smelser: I had my little say on the Patriot Act when I wrote "Surprises at Berkeley." I said if they ever decide to enforce that, this place is going to blow. Getting in and getting the records of what scholars have checked out and what students have checked out, oh god. That was permissible in the law. They just didn't happen to do it. I think they maybe recognized the political volatility.

Interview #20: September 14, 2011

[Begin Audio File 39]

39-00:00:00

Rubens: Hi, Neil.

39-00:00:02

Smelser: Good morning.

39-00:00:03

Rubens: Today is the 14 of September, 2011, and this is our twentieth interview. Let's start today talking about the response to *The Faces of Terrorism*.

39-00:00:34

Smelser: I don't have the practice of chasing down reviews. It was probably reviewed maybe twenty times. All I really saw, maybe, was four or five reviews. They were generally positive. A lot of people stressed the originality of the insights, the new points that were made, and so on. However, there's a deep division in the world of terror analysts, and that is people who take a somewhat more analytic, distant approach from it and try to understand it as a natural phenomenon. Most of these people are historians of terror, who—terrorism goes way, way back. They keep chasing it back. They find evidences of it in the Bible and in Greek historiography and so on and so forth. There's a whole literature on the history of terrorism, and that tends to be more objective and distant, and takes it as a subject matter to be explored. Many political scientists use the same approach. Terrorism is very under-studied in sociology, as I remarked in a previous interview. I have a reason for why. But this was reviewed mostly by political scientists and sociologists. The three or four reviews I saw were favorable.

Then, of course, it had impact in the Department of Homeland Security, I remember. It was circulated quite widely in Homeland Security. But the tension that I'm referring to is between the broader analytic understanding, and my book is about that, and what do we do next? What did Bin Laden say four days ago, and can we do this, and what's the next strategy? It's all much more state department thinking, or defense department thinking, of the next day and so on. Very much instrumental. Of course, the dominant culture about terrorism has to do with what kind of technology do we use to fend it off. A polemic in this book is technology is okay, and any time we can use it, fine, but the human side of terrorism, including not only its genesis but also reactions to it, are so fundamental that that's what we have to know more about.

39-00:03:05

Rubens: You also talked about it emerging out of an extreme form of social movements.

39-00:03:12

Smelser:

Yes. Yes, usually it comes out of some kind of—most frequently now, fundamentalism, but terrorism associated with both left-wing and right-wing ideologies of all sorts. Most of our homegrown terrorism is from the right. Has usually been. The Ku Klux Klan forward is more of a right-wing tinge. Though the sixties are an exception.

39-00:03:42

Rubens:

You said about sociology not paying attention—you said you had some ideas about that.

39-00:03:49

Smelser:

Oh, yes. Here's my take on that. If you take a look at the origins of sociology as a field, in the United States, at any rate, it was a field that came along after economics and psychology, and had those as images of when it was joining into university faculties and so on. These were the two main reference groups. Of course, it had the motive to set itself aside from these two fields, for sure, as well as history. Sociology carved itself out an image in its early history of being, A, scientific, and B, dedicated to social reform of those features that were the negative fallout of the Industrial Revolution. Urban poverty, crime, prostitution. Inequality was a very big one—poverty.

39-00:04:56

Rubens:

Child welfare.

39-00:04:58

Smelser:

Yes. This reformist ideology is something that still infuses the field. The second way it legitimized itself, of course, that it was being scientific rather than ideological in its approach to—so these didn't go exactly together, but these were the two legitimizing frameworks to which the field oriented itself. It still goes on as a living tension in the field. But the reformist emphasis, and this is where I start my observation, usually has to do with dealing with problems that you can do something about. That there is, in fact, some legislation or some social policy that is relevant, and you can achieve something by it.

There's an analogy. There was once a survey of psychiatrists around the whole country on what their patients talked about. Particularly, did they talk about the nuclear threat in the Cold War, and did this inform their affective states of anxiety, depression, whatever? The surprising results of this were that they almost never talked about it. Patients almost never talked about that aspect of internationalism. I concluded from this that people do not worry too much about things they can't do anything about. Here it's just beyond you. It seems to me that the mysteries and origins of contemporary terrorism are such that, in a way, they're almost beyond forces. Sociologists have had—they didn't talk about nuclear war either during that period. They couldn't even develop it as a theme for one of their annual meetings. Sociologists have been weaker on environmentalist, except to understand environmentalist

movements, than either economists or natural scientists, geographers, and so on. So I think the field is dominated by the idea that, in order for us to be interested in it, we have to be able to reform it. Right? Terrorism doesn't lend itself, really, to that kind of mastery, you might say. That's my take on why the literature in sociology is sparse by comparison with political science and history and psychology.

39-00:07:26

Rubens:

You mention in the book that you advised the FBI on the Unabomber. I guess that's in '96 or '97. We didn't talk about that, and I think that's worth a mention.

39-00:07:50

Smelser:

Yes, I can talk a little bit about that. I was at the Center, director of the Center, and I got a call. Three or four people from the United States Postal Service and the FBI wanted to come see me. It was about a year before the Unabomber was arrested. They had, in a kind of desperation, I think, decided to go around to a number of academics in the country to show them—his manifesto had come out. They gave me a copy of the manifesto, and they gave me a copy of all the letters he'd written in connection with the bombings that he had released some of his own—he wrote explanations. They said to me, please read this and tell us if you've got any clues as to where this guy might have gotten the intellectual influences in his life, where he might have been as a younger person in his formative years. What can you tell us about institutions he might have been affiliated with? And so on.

39-00:09:08

Rubens:

Wasn't there some Berkeley connection?

39-00:09:11

Smelser:

He was a teacher of mathematics.

39-00:09:14

Rubens:

We're talking about Theodore Kaczynski.

39-00:09:16

Smelser:

That's right. I don't know why they came after me, but I think maybe Bob Merton mentioned my name. One of their conversations seemed to indicate that he had said, "You have to talk to Neil Smelser." So what I did was I read all this stuff carefully.

39-00:09:36

Rubens:

Amongst working on the encyclopedia and everything else?

39-00:09:40

Smelser:

It was going on. Well, I actually got quite fascinated by it. I wrote them a report saying that I really had some things to say. First thing I said was his style is absolutely deadeningly pedantic. Furthermore, he doesn't understand the intellectual background that he himself cites. He has a kind of wooden style. It puts you to sleep. I said I really had trouble. I went on and I said, I

think this man is a failed academic in the social sciences, maybe. He's had some exposure to the social sciences, but he doesn't have any sophistication. He's kind of wooden. Well, I was a little bit wrong, but he was a mathematician. He was outside the field. I mentioned the institutions where he might have been connected, and two of the institutions that I mentioned were, in fact, correct ones. Chicago and Berkeley. I mentioned Brandeis, I mentioned Wisconsin. The flavor. I also identified the major intellectual influences that informed his manifesto. Herbert Marcuse and Habermas were the main intellectual influences. So I said the times for these people were exactly around 1970. It seems to me that if we take the idea that a person's intellectual influences will be between twenty and thirty years of age, I predict—they wanted to know how old this guy was, too. So I predicted, within a couple of years, how old he was, correctly, because of this cluster of influences that I identified. I also said that he probably came from a liberal family that he got disillusioned with, which turned out to be correct, too. His parents were these forward-looking, quasi-socialist types. Because he attacks both right and left in this, and you just dissect all those themes.

I sent this into the FBI people. Of course, it contained almost no clues as to where you might find him, or his whereabouts or whatever, though I was a little bit proud of myself for making some of these inferences that were close, if not on target, as to what this guy's background and mode was. Of course, what did him in was the fact that his younger brother recognized the text. Recognized the mentality. For the Unabomber, the birth of his younger brother, of course, was a traumatic event in his life. He really never was the same after that brother was born, as it turned out. His brother actually said, well, it's my brother. I sort of commented on the Dostoevsky-like tragedy of the whole thing, of this unwanted brother actually being his undoing in the last analysis. My analysis was totally informal, and I think it was an act of desperation on the part of the feds to turn to the academics, because academics were not going to provide smoking guns or direct evidence, but they were looking for anything they could to narrow down their heretofore, totally unsuccessful efforts to locate the man.

39-00:13:32

Rubens:

You said as deadening as the reading was, you were still fascinated by the whole mystery of it.

39-00:13:37

Smelser:

Well, yes, and I was driven, of course, by being given an assignment to produce my best ideas.

39-00:13:45

Rubens:

Well you've had many assignments over your career to testify in a variety of matters—the black market term papers, numerous social science councils and commissions, some requests for information when you were a UC administrator. Do you recall any other encounters with the FBI or CIA or anything like that? I think you mentioned being asked about students.

39-00:14:16

Smelser:

I used to get frequent—not frequent—periodic visits from FBI, most having to do with former students who were going into a government job or they were looking into them and so on. I myself had a kind of resentment about these visits that I was being asked to rat on people if I knew something. So I developed a phrase that I used maybe 2,000 times, and that was “Not to my knowledge.” I never lied. Most of the things were not to my knowledge. I decided I was going to be a bland informant in this regard. It was just largely because I didn’t go along with a lot of the clearance procedures that were being used at different times during that history. Of course, I was—

39-00:15:19

Rubens:

And didn’t go along with them for a real political, as well as moral point of view?

39-00:15:28

Smelser:

Yes, a mix of those considerations. I objected in principle, and I also felt that a teacher ought not to be asked for incriminating evidence about his students. They assumed a kind of loyalty, a kind of a limit beyond which I didn’t think I wanted to go. It wasn’t unpatriotic. I’m not an unpatriotic person. I just didn’t want to get involved in this line of activity. Of course, I had a lot of contact with the FBI when I was cleared for their labs, and then re-cleared five years later. Maybe I talked about this.

39-00:16:08

Rubens:

I think a little bit, yeah.

39-00:16:12

Smelser:

That was alienating, too, to me, because they got so many things wrong about my career, or else they were baiting me with misinformation, seeing what I would say. They totally misunderstood my role in the sixties, for example. I was cleared, but it took a long time. They went to every neighbor. I went along with it. I could have said, any time, for the labs, “I want this procedure to stop,” and they would have stopped it, but they would have kept me out of classified meetings at the labs. So I let it go through, and then I was re-cleared five years later. I don’t know how much money they spent on me, but nonetheless. Those are kind of the extent of my linkage with security agencies.

39-00:17:13

Rubens:

We talked just a little bit about you kept being called on as an expert after being part of the commission on terror, and then after your book.

39-00:17:24

Smelser:

Mostly by the press.

39-00:17:27

Rubens:

As opposed to the Homeland Security?

39-00:17:29

Smelser:

Yes. I didn't get called in directly. I went to the meetings in the National Academy of Sciences, where they were determining about what kind of research centers to set up on terrorism, which they did. There are now about six of them in the country. I was an advisor on that topic. I went out and spoke to the security people at the Livermore labs. They got a lot of money from Homeland Security to work on mostly gadgets. I went out to talk about the social dimensions of terrorism out there, was invited. So there were scattered involvements and a couple of conferences I went to over time. What happens is, when you do something like this, you write a book on any topic, it disappears out into the world, and you just don't know what happens to it, mostly. Ninety-five percent of what happens is beyond your knowledge. It's a bit frustrating.

39-00:18:37

Rubens:

Just parenthetically, you were talking about these centers being set up. Security studies seems to be a growing field. I just recently heard an interesting interview on NPR with Bruce Hoffman, who a professor at Georgetown who specializes in security.

39-00:19:03

Smelser:

Well, now that probably is not a spin-off of Homeland Security. Georgetown would be a natural place where they'd have formed such a study unit, because they're in the center of political life in Washington, D.C. American University does it, George Washington University does it. All those in the area. The University of Maryland is heavily involved in it. This wouldn't necessarily be a Homeland Security project. They do have one at Maryland, in the social and psychological aspects of terrorism, which I argued for. Most of them, as you might expect, were technical.

They did establish one on the economics of terrorism, I believe at USC. So around the country. I actually argued for the social sciences to be represented in their own interest and research. I didn't cause it, but there are a couple of centers that are definitely in the social sciences.

39-00:20:08

Rubens:

Did you speak at any of them, or were you consulted by them?

39-00:20:13

Smelser:

There was a meeting of the Academy, in which a lot of Homeland Security people were present. A number of us—I remember Charles Vest, the former MIT president, was there as one of the consultants. A number of people around. I was just one of the consultants that came in to talk at this joint meeting between the National Research Council of the Academies and Homeland Security people. One man there from Homeland Security did tell me that my book was widely circulated in the different offices in Homeland Security, but he didn't go beyond that.

39-00:20:50

Rubens:

I wanted to ask you one more thing about the book. You have these boxes, these really personal reflections or personal comments or stories. One is about professor and the Unabomber. It's your style. How do you design that? What makes you stick that into an analytic work?

39-00:21:20

Smelser:

Well, first of all, it was a unique experience for a social scientist to be called in to the kind of inquiry that the National Academy of Sciences, which is a scientific organization, undertook. It is not a longstanding style of mine. When I did my text, I would introduce each chapter with some kind of vignette. On some occasions, those were personal. For example, I introduced the chapter on—either it was personal interaction or stratification—about my own life at Oxford, in which I had a servant named Henry. He polished my shoes. He brought me my rations at the time. Some foods were still being limited in distribution, like butter and sugar and that kind of thing. I described my discomfort at having a servant. That was a personal anecdote. Other stories, I occasionally introduced in there. It wasn't a big part of that text, but it seemed to be an effective mode of introducing the topic. That's the way I used it in that book. This one, I felt I had some stories to tell. They all related to the subject matter of the book. That's where I developed the idea of the informative biographical box that would break up an otherwise relatively dry exposition of an academic sort.

This new book, on usable social science, with John Reed, I persuaded John to write up eight or nine decision points in his own life, including the merger between Citibank and Travelers Insurance. I said, "John, give me some failures as well as some successes, and tell me if you can think of any area of the social sciences with which you're familiar would have been relevant in these decision settings." Then I took them, his stories that he wrote—he's not a terribly good writer, so I rewrote them in livelier prose. Then I myself pulled in findings and research traditions in the social sciences that would have been relevant to his decision. They're going in as boxes.

39-00:24:09

Rubens:

Biographical experiences are important in *Reflections*, and central to *Odyssey*.

39-00:24:17

Smelser:

The Odyssey is full of it, yeah. I guess I've drifted in a biographical direction over time. Maybe associated with getting older.

39-00:24:28

Rubens:

We've talked at times over the course of these interviews about your facility with and attention to writing, to style. And I think I asked you this question before, but did you continue to read novels and plays throughout and did that form of literature influence you?

39-00:24:57

Smelser:

I read lots of plays and lots of novels. My older brother had a big influence on me in this regard. When he was in the service, in the military, and then he worked for TWA as a weather forecaster for a year after World War Two, and of course he had a lot of time on his hands. He was an intelligent and engaged with the intellectual world himself. He would give me all these things about what to read. Somerset Maugham. In high school, I read a lot of Tolstoy, and of course a lot of drama, because my parents were forever giving me plays. In high school, my teachers always would comment on the clarity of my writing. So I had some flair, I guess. I would say that my experience in journalism was probably the most important. I was heavily involved in all the journalism projects in high school. I was sports editor, then editor of the high school newspaper. I worked for *The Arizona Times*, and then a reporter for *Arizona Republic*. Clarity and directness of expression is a high premium there, and I was often writing under pressure because of a news story that I was covering or something.

39-00:26:27

Rubens:

Years of giving lectures. Of course, being book review editor. But reading novels, is that a regular part of your life?

39-00:25:65

Smelser:

I read, and read, some serious novels, but I'm a bit of a fan of detective and thriller novels as well. Like Follet, for example. I've read virtually everything he ever wrote. I do this mostly traveling. Take them on planes and so on. Of course, I get my kind of continuing education through my Guggenheim contacts. Outside my own field, I would say that my loves, kind of in order, would be drama, music, then literature.

39-00:27:46

Rubens:

With thrillers, you must love the whole rational structuring.

39-00:27:53

Smelser:

Figuring out.

39-00:27:59

Rubens:

I'd like to ask you about your book, *America Becoming: Racial Tends and Their Consequences*. This is something you edited with William Julius Wilson and Faith Mitchell. Do you want to talk a little bit about that?

39-00:29:19

Smelser:

It was a project of the National Research Council and the Academies. Of course, I was—

39-00:29:24

Rubens:

We're talking about '97, I think.

39-00:29:27

Smelser:

'97. I think I rejoined already as a head of DBASS in the Academy, so I was around. The Academy was kind of interested in what was perceived as a

combination of Clinton's interest and Clinton's disinterest in race as public policy and as a campaign. Wilson was an advisor to Bill Clinton in the area of race. Bill would talk to me a lot. Clinton was somewhat cautious in that area, because he saw it as a political decision on his part. That he would go in for other themes that he thought were more relevant to his own political standing and political success.

39-00:30:26

Rubens:

Clinton did establish a commission of some sort on race, with John Hope Franklin as the head. I can't remember the name of it.

39-00:30:33

Smelser:

This was a kind of response to the politics of the time, this *America Becoming*. The issue came up as what can the Academy do best in the study of race. The Academy can do best by understanding exactly the nature of racism, residual institutions in this country that have to do with its perpetuation, or its easing of injustices in the country. Christopher Edley, he was at Harvard at that time, was very active in getting this conference kicked off. They chose, basically, Bill Wilson and me to be the two organizers.

39-00:31:24

Rubens:

So you literally organized the conference?

39-00:31:25

Smelser:

Yes, we did, with the help of one of the DBASS committees, of which Faith was the executive officer. She was my executive officer in the two little books on terrorism. I co-edited those two books with her. That was the custom. It's sort of a custom for the head of the committee and the executive officer to be the two editors. I had been working with her through the NRC for a long time on different projects. I worked very well with her. She's a very sophisticated, knowledgeable woman.

39-00:32:09

Rubens:

Is she African American?

39-00:32:10

Smelser:

Yes. She's an anthropologist, trained here at Berkeley, but went into government work rather than academic work. We had a lot of stories to compare. We had a lot of people we knew together in the anthropology department. We immediately had a kind of kinship, and we developed a really close working relationship and personal friendship with one another. I saw her when I went to Washington a couple of times, well after all these episodes. I made a point of looking her up. We had lunch together and reviewed old times and the new times.

The story is told by the book. Bill Wilson, I can say a few words about. He was a longstanding friend of mine. We had mutual respect. I tried to persuade the department to go after him at the time that Harry Edwards was appointed. There was an open position. We had committed ourselves—this was about

1970, I think—to an African American appointment. I immediately responded that Bill Wilson, to my mind, was the best in the country. We actually felt him out. He, at that time, I believe, said he'd prefer to stay at Wisconsin. He later went to Harvard, of course. We couldn't get him. He declined our feeler. That's where I first knew him, and that's where I got to know his work. I was already a fan. We saw each other at ASA meetings, and we began to form a relationship. He, in fact, was the one who pushed my candidacy for membership in the National Academy of Sciences. He wrote the definitive letter of recommendation for me. You have to have one from an Academy member to nominate you. He was my nominator. He mobilized others to support my candidacy, so I had a personal indebtedness and gratitude to him for that. Then he and I had a way of thinking that meshed pretty well. He's the most sensible thinker about race that I know. He's unpopular with the far left. The very title of his major book, called *The Diminishing Significance of Race in American Society*, already would inflame the self-conscious, militant racial protestors, because he's basically saying race doesn't matter as much as it once did in society, and that is not good news to people fighting for racial justice.

39-00:35:05

Rubens:

Just for my clarity, does he argue, ultimately, that class is a more determinant—

39-00:35:10

Smelser:

Yes, that with the growth of a much larger black middle-class and the diffusion through the status order, racial identification gets diffused with class identification, and for that reason, the racial impulse gets less. It's still there. He didn't mean to say it's disappeared. He called it diminishing significance of race. A very controversial book of course. He's also very much interested in the plight of the really poor black population. It's not that he's Tom-ish. He sees the plight and writes very effectively and well and convincingly on the really poor, disinherited poor in the black population. At the same time, he doesn't turn it into a religion. He maintains his analytic sense. That's one of the bases on which I could resonate with him. He kept his objectivity about his approach to the world.

39-00:36:26

Rubens:

Was this a pretty intense time, putting on the conference and editing the book.

39-00:36:32

Smelser:

Well, it was one of those editorial jobs that was considered less demanding than many because of the staff. We had Faith. Faith is terribly efficient, so I didn't have to worry about deadlines or hustling authors. The usual thing you get when you're editing a book, I didn't have to do any of that. It was intellectually gratifying because the qualities of the articles in that book are very high. We got a very, very good and informed group of people at that conference.

- 39-00:37:12
Rubens: Yes. Edley writes the forward to the volume. And working style with Wilson?
- 39-00:37:27
Smelser: That was very positive. It was a good thing. We were naturals to come together.
- 39-00:37:34
Rubens: Were you excited by the conference?
- 39-00:37:37
Smelser: Yeah, I was engaged. I was very much engaged in the conference. There wasn't a moment of boredom for me.
- 39-00:37:44
Rubens: I want to just pick up some of the themes. You had already paid attention to race. It had been a—
- 39-00:37:49
Smelser: Theme that came in and out of my work. I couldn't be called a scholar of race relations, but it just came in and out a lot.
- 39-00:37:57
Rubens: Of course you had a commitment to diversify the sociology department at Berkeley, which shows up in *The Changing Academic Market* and in these interviews.
- 39-00:38:03
Smelser: *The Changing Academic Market* has an element there. Then, of course, racial disturbances played a big role early in my studies of collective behavior, because I wrote a lot about riots, and included race riots as a major topic in that book. At one time, I initiated a research project, as a very young scholar, on trying to figure out, historically, the differentials in patterns of lynching in the late nineteenth, early twentieth century in the South. I was interested in county-by-county differences, and tied up with sociological variables such as migration rates, unemployment, other things of that sort. Turned out to be kind of a dead end. I didn't really come across any thrilling findings, so I never published anything in the area.
- 39-00:38:55
Rubens: What got you interested in that, specifically?
- 39-00:38:57
Smelser: It was my interest in collective behavior. Of course, I saw that as a genre of race riots and racial violence, and I was hoping that I could contribute something original to the understanding. I made up my mind I couldn't.
- 39-00:39:17
Rubens: That leads me also to ask you about your essay, that of course you published later in *Reflections*, on affirmative action. This was in the wake of the regents effectively dismantling preferences for admission based on race. Your essay is

tremendously edifying by looking at the preconditions, the whole social ferment that's going on around the period of mid-1990s. '96 was when the regents passed SP1 and 2.

39-00:39:59

Smelser:

Well, I have to give you a little background of that. A few years before, the American Academy of Arts and Sciences, along with support from a number of foundations, sponsored a conference in Boston. I think it might have been called *The University and Its Discontents*, or something like that. It had to do with the major problems facing research universities, really. Jonathan Cole of Columbia, and Eleanor Barber of Columbia, both old friends, were the people who put it together, with support from the Mellon Foundation. They wanted to get me involved in that conference, and so they wrote me a letter saying, could you give us a general, overarching essay on the missions of the university? I said okay, I'll do it. I did so with a heavy heart, because this literature had always been kind of boring to me, in that it didn't seem to ever say anything new. Rhetorical, very rhetorical, and very un-analytical. I said okay, I'll take it up. I hope I can give you an interesting twist on how the missions of the university have evolved, and tensions among the different missions. I was going to try to write an analytic rather than an exhortation about universities and their value to society.

39-00:41:51

Rubens:

This is just before you go to the Center?

39-00:41:53

Smelser:

Just before.

39-00:41:54

Rubens:

This is when you're in the office of the president?

39-00:41:58

Smelser:

Yes, I think so, in 1993. That's about the right time. I either was asked at that time or I wrote it during that time.

39-00:42:05

Rubens:

'94, it comes out.

39-00:42:06

Smelser:

Yes. '93. So I was over at the president's office when I did this. I was just getting ready to put this thing together and start my work, and they called me back. They said, "We hope you haven't started this thing, because we'd like you to write another essay instead. We'd like you to write about diversification, cultural conflict, and multiculturalism." The whole eighties and nineties story. Boy, was I happy that I had gotten off the hook for writing what I considered would have to be something of a dull contribution to something that was very exciting. The reason they asked me was they had gone around and got refusals from a lot of people, or they had decided that some people weren't the right ones, because they'd already had their say and

they felt they were going to just say it again. Some people said no, they didn't want to touch it, just because it's controversial. I got very excited about writing that essay, so I did. I called it "The Politics of Ambivalence."

39-00:43:11

Rubens:

The first foray into using that concept of ambivalence? Well, you had done it with a psychological piece, but this was before your ASA speech.

39-00:43:20

Smelser:

It was before my ASA speech. I went back and looked at the introductions to my doctoral dissertation when it was published, the introduction to *Theory of Collective Behavior*, and the introduction to my book on comparative sociology, and my book on British primary education. Every one of them began by the proclamation that we were ambivalent towards the subject, and this was completely unconscious on my part. I didn't realize that there was this continuity in my own thinking. I only discovered it later. So no, I had been preoccupied. It was one of the concepts in psychoanalysis that grabbed me most, and has proved to be one of the most powerful in that tradition. There was a kind of build up.

I got the idea that in these very complex cultural and racial and gender fights that had been going on, that they had peculiar characteristics. Almost all the advocates from all sides were ambivalent toward what they wanted. I said, how far can I push this idea, and what are the implications of the fact that when you're fighting for a cause, you have some reservations about whether or not you really believe in it? There's an ambivalence. I said most of the pushers for the causes, as well as relevant audiences and administrators and faculty and students, all themselves, have greeted this movement with ambivalence, tied in with their own cultural pretensions, their own cultural ideals, and so on. I tried to get as much mileage out of it as I could. The paper was really enthusiastically received at the conference at the American Academy. I felt almost embarrassed because they didn't get any fighting about it at all and everybody who read it from different angles thought it was wonderful.

39-00:45:22

Rubens:

You were also trying to articulate how much does a research institution and an elite institution become a microcosm or a mirror of what's going on in society, or does it retain a certain remove from the society.

39-00:45:36

Smelser:

It always gives it its own flavor. I tried to impart some idea as to how these conflicts worked out in a specifically university setting, which is different than they worked out in the marketplace, and different than they worked out in the contracting—

39-00:45:52

Rubens:

The mission of the university had incorporated—

39-00:45:55

Smelser:

The university had taken it up as a more or less official mission, but also in an ambivalent way.

39-00:46:03

Rubens:

Where did you come down on that? I suppose we're projecting ahead on this also. That it has—maybe “weakened” is the wrong word, but it has had an impact on the nature of—

39-00:46:18

Smelser:

That's the main argument I developed in the essay on affirmative action. The reason I give you the background is because when I got this invitation to give a paper at the 250th anniversary of Princeton celebration—that's where it was delivered—they asked me to write on affirmative action. It was still big in California, so that's why I called it “A View from California.” However, the organizing argument of that essay on ambivalence, which you obviously picked up because I said it, was, why hasn't affirmative action, as a public policy, taken firm hold in the country and its politics? I said, most reforms have a very controversial beginning, but pretty soon, they get settled into the political and social order and become part of the ongoing political life of the country. Social Security. It was just practically defeated and torn apart, and people saw it as socialism and everything, all in the early stages. Then it began to get into the fabric of the society, and it's now kind of routinized, institutionalized. Affirmative action never had that experience. Why? Why didn't it go the route of most reforms and become just an institutionalized part of how we do things? It's always flaring up again and again and again, and backlashes and frontlashes. All kinds of things were continuing to happen. I said, why? That was the intellectual starting point for my analysis. I went through cultural and political and economic aspects of these policies and tried to explain, especially on these cultural and psychological sides, why this continued ambivalence and conflict hounded it. That, of course, was the lead-up to this kind of rollback in California politics, which was the last part of the essay.

Now, once again, you can see from what I'm saying, this was an analysis. Stepping back from the rights and wrongs of affirmative action, which I was basically in favor of, but also appreciated some of the excesses and unhappy turns and twists that the history of that movement took. But I wanted to be analytic. That was my distinctive intent, rather than to simply argue who's trying to defeat it and so on. Here's something that's a cultural phenomenon, or a political phenomenon, that we deal with. It's had a peculiar and a unique history, and how can we understand it? That was the intellectual impetus to this article. As I think I explained in the prefatory remarks, the people at Princeton didn't like it too much. You do not take for granted what we take for granted. That it's just a good thing. The main thing to understand is who's trying to defeat it and how we can get it pushed.

39-00:49:46

Rubens: You were talking about the complication of, for instance, an ethnic studies department being ultimately marginalized.

39-00:49:52

Smelser: Yes. I just sort of wrote what I saw and tried to incorporate it under an organizing framework of cultural and psychological and political ambivalence. There was a direct continuity between those two articles. Your reaction, which you recently confessed to me, very positive. I sent a copy of it to John Cummins, and he sent me back a note saying, "Well, we have all the answers."

39-00:50:22

Rubens: What made you send it to Cummins? You had known Cummins from your early experience in administration life –and we'll talk about the Committee on Surprises later.

39-00:50:31

Smelser: Cummins and I we're blood cousins. From my experience in 1965, when I was in the chancellor's office dealing with student conflicts, ten years later, Mike Heyman regularized that appointment. Really made an official cabinet or administrative position out of it. He was in the ranks. Cummins knew what I did. Cummins would ask me to some of the planning meetings for his own purposes. We liked each other. We had a golfing relationship together. He would always ask me what new jokes I'd heard. I would ask him what new jokes he'd heard. As a matter of fact, Cummins was the guy who made a big push for me to have an oral history. He was one of the big movers in urging this.

39-00:51:32

Rubens: He's always valued oral histories. He's used them, many of them, on the history of Berkeley and the whole system.

39-00:51:39

Smelser: Yes. Well, he'd had his own. John was a fan of mine. John thought that even though I wasn't in one of these positions that would automatically qualify, like having been a chancellor or a publicly visible administrative leader, that I was someone called a non-positional leader in the university, merited an oral history.

39-00:52:09

Rubens: So you sent this essay to him?

39-00:52:11

Smelser: I sent it to him.

39-00:52:12

Rubens: Systemwide, and especially at Berkeley, diversity seems to be a fundamental tenet. There's a new administrative program, with a dean, that is focused on

equity and inclusion. Does that mean that affirmative action has taken hold, even if it requires advocacy and monitoring by a separate office?

39-00:53:07

Smelser:

I believe that the institution of higher education responded, basically, well and positively to the massive external forces that were working on it. I'm not just referring to the civil rights movement, the affirmative action movement, the social movement, feminist movement, all the things that went into making it a political force. But the government joined that movement. That's a very important factor and role.

39-00:53:43

Rubens:

National and state and local.

39-00:53:45

Smelser:

Absolutely. They picked it up and embraced it, and in fact, the university administrators were kind of—no one opposed it. There was just a kind of uniform idea of its value as an instrument of social justice. Martin Trow, who was a critic of affirmative action, once went to a meeting of the American Association of Universities, full of chancellors, and he baited them. He said, don't you consider it odd that in this room of several hundred people there isn't a single dissident voice? Can you think of a political issue that doesn't have same variation of opinion? You're all alike. He really was baiting them. Neil Rudenstine blew up and walked out of the room when Trow was talking. He was president of Harvard at the time. He was just attacked, Martin was.

Anyway, that's kind of incidental. Universities threw themselves behind—It became part of the arena of political correctness, and still ambivalently so. The import on the university, most of all, was that they came under institutional pressure, mostly from research-granting agencies, to pursue affirmative action aggressively. HEW [Health, Education and Welfare, at that time] was a very big agency, and Department of Labor. Anybody who gave money to the university. There's always the kicker that the university has to obey federal laws and federal policies, right? These officers would come out from these granting agencies and make their inquiries into how the university was doing on this. I actually was in a meeting at one time when the HEW people were pushing the university people, almost to the kind of quota-type logic. Because I was head of the senate at the time, I was called in. It was a kind of ambivalent set of relations.

However, they've gone for it. They've institutionalized things. But those institutions, like the issue itself, are always kind of hanging around the edges of the university. There is kind of a ghost, that it's an obligation hanging over the university more than a matter of complete and full commitment, because the university is, of course, the seat of radical egalitarianism. Are we compromising our own fundamental values by this? I think my story continues. Even though affirmative action itself, in its literal form, has been, to some degree, dismantled. It's taken a new cloak, it's this outreach, it's this

diversification and gotten assimilated with other kinds of diversification. Nonetheless, I said in the end of that article that the rollback of affirmative action in California is not the end of the story. That we're still in the same mental trap that we've been in all along.

[Begin Audio File 40]

40-00:00:20

Rubens: *America Becoming* is a powerful book. I can hear your voice in the introduction, it's so clearly you.

40-00:00:34

Smelser: I'm very proud of that book, but because it was such a big collective enterprise, you can't get your own personal view wholly into it. But I actually was a prime author of that introduction. Bill wrote parts, but I rewrote the whole thing.

40-00:01:01

Rubens: This question addresses the second essay. What is it about the historical forces in the eighties and nineties that just drives this political correctness regarding multiculturalism and the role of race? There is ambivalence; there is Wilson's argument about factors other than race. Then you point to postmodernism. You say that that has had an effect. I wonder if, intellectually, you could just speak to that.

40-00:01:43

Smelser: Oh, okay. The context in which I brought up the postmodernism is the following. That is to say, in the first decades of affirmative action, the main import was improvement in economic access and in university and college access. These were, you might say, economic, career-related, occupational-related arenas. The idea was the economic and social advancement of certain categories of people, including also the end of discrimination, as in the case of gay rights and so on. Gay rights was different from the women's and the racial and ethnic minorities, in that it didn't focus so much on occupational issues. They were more interested in stigmatization of homosexuals, so it had a different flavor. Generally speaking, the early history, from the Johnson administration for the next fifteen to twenty years, was advancement. Big increases in enrollment in universities. Pressure on contractors and pressure on employers to advance the aims by recruiting, under different formulae, minority and women. That was the main story of affirmative action, economic and educational. A lot of advancements were made.

40-00:03:30

Rubens: In terms of the staff—

40-00:03:32

Smelser: Staff was easier to deal with, in a way, because, in a way, this preoccupation with meritocracy wasn't quite as predominant as it was in admissions of students and recruitment of faculty. Meritocracy turned out to be a

counterforce, in a way, to affirmative action. However, then I went back to Tocqueville, and I said, okay, when people get a certain advancement, and get access to the system, it's almost always partial. Because they're advancing in one arena, the fact that they're not advancing in others becomes all the more painful. I said it was kind of natural that they should take a cultural turn. That the demand for cultural respect followed on this greater economic and occupational access. This became more troubling, because in fact they had undergone some advances, but they didn't seem to be—the curriculum was still offensive, still dominated by the establishment, et cetera, et cetera. That was one big part of the cultural turn, meaning that we are culturally, still, disadvantaged, because of these archaic curricula dominated by a white male, dead people, and where are we? That's, of course, the origin of these reforms, for curricula to get new kinds of—that cultural studies is the rewriting of Western civilization courses at Stanford, and so on and so forth. Began to press on the new and cultural and, in a way, more disturbing aspects of your life, because they're getting at the heart and soul of what the faculty is teaching, and what the university is offering, and what kind of institution is represented, and what value biases does it have from a cultural point of view.

Of course, postmodernism fits right into this because of its relativism. It sort of says we can't really get at the truth. That's one of the features of postmodernism, is everything is constructed. It's arbitrarily constructed. Furthermore, it's constructed in the interest of the people who control. A lot of people have treated postmodernism as an echo of political radicalism of two decades before, without the political. In other words, it turned in a cultural direction. It had this idea of knowledge as oppression. The new spokesmen were Foucault and Derrida and others, and how they were adapted became knowledge as power, culture as power. It sort of fit into the idea that these old dominating groups, white male Euro-centric types, were culturally dominating the system, but we want that to end. We want to have our curriculum. We want to have equity in what's offered. We want to have our groups represented in who's contributed to American history or literature, so on and so on and so on, to a cultural push on top of the, you might say, economic and social mobility and educational push that had preceded it. I interpreted it as the inner logic of the social movement. Once it gained some successes, other issues become running sores for it.

I also have subsequently developed the idea that postmodernism was, in fact, more a voice of the humanist rather than the other academic areas of the world. English, history, language studies. History of science, communication, so on, were the hotbeds. It crept very selectively into sociology. Not much at all. Economists never heard of it. Very little in psychology. Anthropology, yes, but that's one of the humanistic social sciences. Somehow or other, I saw this as, in a way, a class protest on the part of these disciplines for the accumulating advantage they have had with the tremendous influx of scientific research and the increased perks and salaries. Evolution of the social class system among academics that has come about. Postmodernism, which

had an antiscientific impulse as well, was almost a kind of social protest movement as well as an intellectual movement. I'm going to develop this theme in the Kerr lectures.

I have not, myself, entered in here to get the differences of constructionism, post-constructionism, postmodernism. All of them are part of a larger movement of cultural protest. I hesitate to get into drawing the fine lines among them. You wouldn't have gotten it from my essay, because I didn't have it either.

40-00:09:32

Rubens:

Well, you mentioned it, I thought. There's a way in which you string together several of these phenomena.

40-00:09:40

Smelser:

Once again, it's my style of taking a look at a phenomenon and not exactly treating it completely in its own terms, but stepping back and saying, well, what is this likely to be all about? Why did it appear at this time? What's the real import of it in the larger picture? I'd have to say that those kinds of questions are a consistent intellectual style of my own.

40-00:10:15

Rubens:

You always have an historical perspective. Regarding Sp1 and Sp2, I hadn't quite paid attention to that the regents had turned over dramatically; that they have twelve-year year appoints, and by '94, '95, all the Jerry Brown appointments, arguably liberals, turned over and now there were the Republican appointments.

40-00:10:54

Smelser:

Twist in their politics. I actually sort of observed this in action when I was on the regents. You could just see it. They still had Vilma Martinez. They still had this gay regent, Andelson. These were all Brown's somewhat quirky—he went out of his way to be unorthodox when he was first governor. They were still there, but then they disappeared in the early nineties.

40-00:11:22

Rubens:

Were you called in to talk to the president or to Jud King or to any of these people who were developing their response to SP1 and two?

40-00:11:37

Smelser:

No, no. I had gone to the Center. I was out of the university scene. I might have been involved in this. Insofar as it was flittering around in admissions policy and so on during the time I was in Peltason's office, we talked a lot about these things. But subsequent, when the real action began, '95, I was already long gone from the university context and was not called in.

40-00:12:09

Rubens:

We began today talking about the Clinton administration seeking you out, and through the Research Council, having you do, ultimately, *America Becoming*.

40-00:12:19

Smelser:

Yes. There could have been some sense, but anybody who goes to Stanford has sort of stepped off the end of the world as far as they're concerned. I came back from time to time. I was on the search committee for UC Press, for example. Jud King called me in on that. I was at Stanford at the time. The review of the Education Abroad Program—they brought me up for that. Highly selective.

40-00:12:50

Rubens:

Shall we talk about your joining and participating in the Robert Wood Johnson Foundation?

40-00:13:12

Smelser:

Yes, yes. Just before I left for Stanford, I was approached by Richard Scheffler of the School of Public Health. He's a trained economist, but he's working in economics of medicine. The Robert Wood Johnson Foundation initiated a program of sponsoring social science research, and of health care delivery and health policy, in the early nineties. It was going to take the form of giving postdoctoral grants to economists, political scientists, and sociologists who committed to study some aspect of health care and health care delivery. It was Robert Wood Johnson's way of getting into this side of health research. The postdoctoral program, they wanted to have them at the top universities in the country, and they invited each university to present a way that they would organize a postdoctoral program if they were given a sizeable grant from the Robert Wood Johnson Foundation. I get ahead of my story to say that Berkeley was one of them that was chosen, along with Yale and Michigan. The Yale one didn't seem to work out the first few years, and they switched to Harvard, but those were the three. Scheffler brought me in as an advisor to him in submitting Berkeley's proposal.

40-00:14:57

Rubens:

Scheffler was the—

40-00:14:58

Smelser:

He was the entrepreneur, and he was the one who got it administered through the School of Public Health. That's where it is. I had some paternity rights in this program, but of course I wasn't affiliated with it, because the minute it was established and Berkeley got going in it, I went to Stanford. The first seven years of its working, it just went on. The way it works is that the four postdocs would come to the campus, and they would basically be free, as postdocs are, to follow out the research projects they'd submitted. At the same time, they were given some instruction. In particular, an introductory seminar given by three faculty members to four postdocs on how their own disciplines approached issues of health and health care and health care policy, and the most important aspects of the whole arena. That seminar has, from the beginning, been a part of the postdoc program.

When I came back from Stanford, the sociologist had not worked out as well as they hoped, and they were interested in getting a replacement. So Scheffler came back to me and said, would you join the executive committee of this, and would you be responsible with the economist and the political scientist for offering the introductory seminar in the first semester that they're here? It was something I wasn't anticipating, and I wasn't quite sure I would take it. However, I was quite certain I didn't want to return to become a teacher part-time in sociology. I felt my severance with the department was such that it was kind of permanent, and that I had certainly paid my dues to the department, and that there were many ungratifying elements of being involved in the department's politics and routine administration. They had room for an emeritus, and they did get me to teach a course the first return, but I kind of said to myself, this RWS project is just the right kind of thing for me. It's a little bit of income. It's a chance to continue interdisciplinary work. I knew that the postdocs were bright as anything, because they paid them so much money. They recruited the very best people out of the very best universities. It was more like a collective seminar than it was instruction, because these people were so mature and so smart.

So I have taught that every year, up to the present. I'm teaching it right now, this very minute. I had a class yesterday, I have a class tomorrow. I've found it continuously gratifying. I've developed mentor relationships with the sociologists. We divide up our labor. They would have two post-docsociologists, one political scientist, and one economist one year. Then they'd emphasize economists, and then political science, then go back, because the four wouldn't fit three. Now they've cut them down to three a few years ago, in the great financial bust of 2008. The Robert Wood Johnson Foundation suffered like they all did, so we now have three postdocs, but the program is doing well.

40-00:18:42

Rubens:

Do you all meet together, the political scientists and the economists, or are you—

40-00:18:45

Smelser:

Everybody goes to all the seminars. It's a little seminar of six. It's absolutely a kind of sacred obligation that you show up, even for those seminars that you're not responsible for organizing. I organize the seminars on doctor-patient relations and professionalism on one hand, and inequalities in health care delivery on the other. Those have been the two most interesting aspects of sociology research into the medical area. Then I get a guest to come in and talk more about the demographic aspects of it.

40-00:19:29

Rubens:

Do you talk about psychology?

40-00:19:31

Smelser:

I assign the classic work, which happens to have been by my mentor, Talcott Parsons, on the doctor-patient relationship. It's fraught with psychological analysis. In fact, it was informed a lot by psychoanalytic thinking—Parsons was. I include that, yes. It's been the perfect solution to my lingering attachment to teaching.

40-00:20:05

Rubens:

Great. Good. Okay, so you wanted to talk about that in relation to the Foundation. You also then taught at the University of West Virginia and Illinois.

40-00:20:13

Smelser:

And I've taught abroad a few times. Vienna —

Let's talk about Virginia and Illinois. I will confess, straightforwardly and straight-facedly, that my teaching at the American universities was completely a function of our attachment to our daughter, Sarah. She is an artist. She got an MFA at Iowa. She taught at a small college in Wisconsin, and then she taught at Pasadena Community College. It's a tough market for artist teachers, and then she got a ladder offer at the University of West Virginia. She joined as an assistant professor about 1998. Here's what happened. One of her students in one of her art classes was a sociology major. He had read some of my work, the student, so he said to Sarah, "Are you a relation?"

40-00:21:31

Rubens:

She uses the name Smelser?

40-00:21:33

Smelser:

Yes, she goes by Smelser. Sarah Smelser. He says, "Are you a relation to Neil Smelser?" She said, "Of course, he's my father." This guy then runs that news back to the sociology department, that Neil Smelser's daughter is teaching in the art department. A person, also a former graduate student at Harvard, who was chair of the department at West Virginia, approached me and said, "We have this visiting professorship. Would you like to come?" I probably, I have to say, without being a cultural snob, would not have chosen West Virginia as a place to go. West Virginia as a state has fallen into poverty. It's kind of our third world, almost. Its universities have been starved. But that opportunity to be living in the same community as Sarah, who was just having her first child, I snapped it up. Just after 9/11, spring semester 2002, I went to teach there. I taught a course in sociological research. My old methods course, brought completely up-to-date. I taught that.

40-00:23:02

Rubens:

The whole semester?

40-00:23:03

Smelser:

Yes, whole semester. It was four months. I also taught a graduate seminar on different aspects of my career. It was more a biographical seminar that I gave

to graduate students and numbers of faculty who came to it. It was not an overwhelming load.

40-00:23:29

Rubens: The students must have been very different than—

40-00:23:33

Smelser: No, no, they weren't. This happened at Illinois State, too, but it happened at West Virginia. This undergraduate course I taught, the faculty sort of scared mediocre students away. I was a big shot, from California. I was going to flunk you out if you're not doing well—so I had a small class. There were only about fifteen people in that undergraduate class, and they were the pick of the crop. They were the best students they had. To me, it wasn't like saying, I've got to treat these like uneducated brutes, or anything like that. I gave my regular course and they got engaged in it. Graduate students were not nearly the level of anything I'd ever had in my life, and most of them were locals and they were interested in local issues. I learned a lot about West Virginia in teaching them, but by and large—

40-00:24:33

Rubens: What's the town? Where is it?

40-00:24:34

Smelser: Morgantown. It is looked upon as a cultural center of the state. It is dominated by partying, by football. I would sit on this commuter train that leads around to all the buildings and the offices, and I'd listen to undergraduates talk. It was the usual thing, parties and football. The girls would talk about boys. It was that thing, so I was really back in the mainstream.

40-00:25:06

Rubens: It's that kind of odyssey, right?

40-00:25:08

Smelser: It was a very interesting thing. Here was the irony. After I'd accepted, Sarah got her offer at Illinois State, and she left a month before I was arriving. Sharin went crazy. She said, "Can't you reconsider?" I said, "I've signed the contract." She said, "Can't you squeeze it in to six weeks, maybe?" I said, "No, no, no. I've made this agreement. I'm going to do it." What we did was I negotiated that all my teaching should be Wednesday and Thursday. We took the camper. That is so beautiful, that part of the country. We toured around Virginia. We went to Kentucky. We went to Tennessee. We went to Maryland. Until it got too cold, we would go camping. We went to New England in the fall. We made the best of it.

40-00:26:13

Rubens: I was wondering if you visited Washington as well, as a kind of cultural hub?

40-00:26:19

Smelser: Did we visit Washington during that time? Maybe one weekend. We wanted the country. The Blue Ridge Mountains. We made the best of it. Sarah wasn't

there, of course, and the baby wasn't there. They expected me to withdraw from the visitorship. They confessed to me that they thought I was going to withdraw when Sarah left, and that there was some psychological wisdom to that judgment, but I played it professionally. I had agreed to do it. I said I would do it.

Now, the visiting teaching at Illinois State was also because Sarah was there. She now had two children. So I also swindled an appointment in the sociology department, winter 2005-2006, the same year I went to the Library of Congress. So we were away eight months during that year. So I taught a course in economic sociology to the undergraduates. They asked me to teach that course, because my handbook had just come out. The second edition was out. They thought it was one of the more rigorous courses they could offer, and they asked me to offer it. Once again, they discouraged their mediocre students from coming, so I had a good group of students there. They do give an MA at Illinois State, so I did a graduate student seminar as well, mostly on my own work.

40-00:27:58

Rubens:

Where is Illinois State?

40-00:27:59

Smelser:

It's in Bloomington. Don't mix it up with Bloomington, Indiana. This is Bloomington, Illinois. Near Normal, Illinois, from the old Normal school. It's in the center. It's just a two-hour drive south of Chicago. Not far from Springfield. It was pretty much in rural Illinois, though State Farm has its offices there. The university is big. It's a big state university. Twenty-five thousand students. She teaches in the art department there.

40-00:28:30

Rubens:

What does she teach, by the way?

40-00:28:32

Smelser:

She's a printmaker. A lot of her pictures are around this room. She's quite an accomplished and successful printmaker, and she teaches printmaking mostly at Illinois. She's tenured there. A couple of years after she got there, she was tenured. She didn't care for it too much at West Virginia, and so she welcomed the opportunity to move. The funny outcome—they were in the transition from a chairman, who chaired that sociology department—he was the one that organized my invitation. He was a student at San Diego, and he had come to hear me speak when he was a graduate student. I was on his radar screen. They also knew that Sarah was there. So they asked me. Then the dean of the college called; he wanted to recruit a new chair. They have a policy, sort of, when in doubt, recruit from outside. Bring in someone to the department as the new chair. My friend from San Diego had been chair for ten years. It was time for him to go. He was beyond the term. They wanted my advice about searching and on fields of interest that might be worth cultivating in the department. I became a kind of advisor as well as a teacher in the

department. I talked to the dean one time, and then he called me in a second time for further advice. He said, "Would you be the chair?" Here I was, retired already for several years. He was working on my tie to Sarah, obviously. That was his leverage, that we were so connected with this family that I'd be tempted to do it. Of course, I was not. This was totally out of my range of interest at the time. I told him how flattered I was to be approached to be chairman of a department at age seventy-five. I had retired already twice. I didn't want to retire a third time. So it was okay. Whenever we visit there, I occasionally talk to the sociology people.

40-00:31:05

Rubens: Did you connect with anybody at the University of Chicago or Northwestern?

40-00:31:14

Smelser: I gave a public lecture at Notre Dame. I didn't link up with any other colleges at that time.

40-00:31:21

Rubens: William Julius Wilson was at Chicago before he went to Harvard.

40-00:31:41

Smelser: Wilson was at Chicago for years and years and years. As a matter of fact, he tried to recruit me at Chicago when I was at the Education Abroad Program. For when we came back.

Anyway, it was a happy episode at Illinois State. I continued to see a few people, and of course in my daughter's academic community, when we go back there. We go back there at least once a year. Then they come out here every summer.

40-00:32:26

Rubens: Does your family all get together? Do all the children—

40-00:32:31

Smelser: Oh, yes. When they're out here, the other kids from San Francisco come over a lot.

40-00:32:37

Rubens: I was just thinking about your Christmases. You mentioned bringing your older children to Europe a couple of times.

40-00:32:42

Smelser: Then for a long time, we had the big dinner and so on, but now we've taken to doing something special just for ourselves. We went down to Death Valley one year. Now we've been going for a couple of years to see the grandchildren at Christmas.

40-00:33:00

Rubens: Is now a good time to talk about the Committee on Surprises?

40-00:33:11
Smelser:

Yes, why don't we talk about the Committee on Surprises. Shortly after I came back from Palo Alto, Robert Berdahl was still chancellor. Berdahl knew me, I think probably mainly through John Cummins. They were close. Cummins was his chief of staff. He was there during Berdahl's entire career, and he worked several years for Birgeneau, but now John is not in the administration anymore.

In 2002, we had the SARS epidemic. The university went into a big fit over that, and in particular they had a couple of seminars that they scheduled for the summer—this was to be a summer educational program for students from Hong Kong and China. They canceled them. This was a public health measure. SARS was an unknown syndrome for which there was no vaccine. It had sometimes deadly effects. It was a serious epidemic. Berkeley panicked. They didn't have the public health facilities to handle anything approaching that. They were certainly aware of the health dangers of bringing people from that part of the world into this part of the world, and they were also aware of their own limited capacities, and they were also aware of the criticism they'd get if they brought over these people and SARS appeared in the Berkeley community. You can just see that they had to do something, so they canceled the seminars. This was a personal wound to the Asians. A matter of face. That we'd insulted them. We didn't show respect. There are all kinds of the usual cultural accusations that would come from that part of the world, that we had not lived up to our obligations, so on and so forth. The thing that happened that was unanticipated was that a number of Asian American alumni groups got in on it. These, of course, are donors. The university had to do a tremendous amount of repair work, diplomatically, after this sort of thing.

They were taken completely by surprise. It was out of that that either Berdahl or Cummins got the idea of forming a special committee that would sit to it would try to understand the nature of surprises—political surprises, usually, political crises—and also keep an eye open as to what were looming on the horizon as issues that might come up or explode in our face—become surprises. They brought in a few administrators. Cummins was on it. Berdahl himself attended the meetings. They were once-a-month dinner meetings. They brought in some other seasoned veterans and faculty. Tom La Porte was a member of it, from public administration and political science, and a couple of other administrators.

40-00:36:43
Rubens:

Whose name was this, Committee on Surprises?

40-00:36:46
Smelser:

It was called Committee X at the beginning. I don't know. We named it Committee on Surprises once it started meeting. I thought it was colorful and better than Committee X. I thought it was very colorful. I was an active contributor to it. On my own initiative, I went to Cummins one day. I said, "John, I think maybe I would like to contribute something to this group that

wasn't in the cards when you asked me to join it. I think that I might be able to write an analytic essay on exactly what is a surprise." What are its consequences, what are the typical patterns of adaptation to it when it happens, what can we learn, and how can we better establish a mechanism for anticipating and involving relevant parts of the community, et cetera, in it. I said, "I think I could write an essay based on a history of Berkeley over the past forty years, and it might be helpful to this committee." Cummins ate up the idea. He liked it. Cummins is my fan. He was glad I volunteered. I said, "I'd like a couple of research assistants." He said, "Done." We found the money, and so I hired two sociology students to go through the historical archives, with which I was somewhat familiar. But I didn't follow every little item. There were a couple of big surprises when I was down in Palo Alto, because I hadn't really been involved in, like the course in Palestinian studies. It was anti-Israeli or something. It caused a great big blowup. So he said, do it. I had enough spare time. It was the reason I volunteered.

The surprises thing, it's my bag. I wrote this essay. It appears in the book, *Reflections*, and it is really an analytic essay of what makes a surprise, and what happens, typically. So I analyzed the dynamics of responses to surprises, both the positive and the negative possibilities when a crisis or a surprise hits a campus. What are the limitations of responses? Then I developed a whole section on what kind of machinery, beyond Cummins, do we have, or should we have, to deal with anticipated surprises. So I wrote the essay. The essay was the subject of the last meeting of the group. Berdahl didn't continue it, and his successor didn't continue it. It was an ad hoc committee. But I wrote this essay, with which I was very surprised at how well I was able to put it together. I documented it extensively, based on my own memory and work of the research assistants, so that it was an empirically informed piece of work, and also policy relevant. I don't know that it had any influence on anybody in terms of subsequent adoption of measures or precautions. I made the point in the essay that surprises, in the academic world, are things that people like to forget. That they like to assume we're back to normal. Consequently, you assume they're not going to happen again. Very unrealistic, of course.

That's where I began to develop a political thread in my thinking about universities, about exactly what the role of constituencies in the university is. The ways in which we are dependent on constituencies, how they've multiplied, how they have become, in effect, shareholders or stakeholders or owners of the university, and how integral a part they are. Almost all surprises had to do with some constituency yelling at the university for something it did, or a group of constituencies, or constituencies yelling at each other, both putting pressure on the university. So I analyzed crisis in terms of its relational aspects. Not something within the university, but something that almost always involves some kind of external forces—and that's what makes it a crisis, of course, because you're scissored in by external forces. That, of course, is going to be a big theme in my Kerr lectures, to push the

implications of a university becoming more complex and more involved in the larger society for its history. That theme will reappear in my lectures.

40-00:42:06

Rubens: This wasn't for public consumption, really, until—

40-00:42:10

Smelser: It was for the group.

40-00:42:11

Rubens: —you published publish it in *Reflections*.

40-00:42:16

Smelser: Like many of the things in *Reflections*, I just let the thing sit. I didn't experience the need to have it published. I don't know whether this is unusual or not. Erving Goffman once told me, in a kind of joking bit of advice, he said, "Don't get into anything unless you can get an article out of it." That was his somewhat cynical advice. But I didn't feel that.

40-00:43:24

Rubens: You then got this stroke of genius to put these all together. Maybe we should mention that now, even though we're jumping ahead.

40-00:43:35

Smelser: I put it together after writing "Surprises," even though I waited six years after that essay. It began to bubble around in my mind that maybe I had written enough about the University of California and related subjects. This was like the book I put together on my psychoanalytic essays that came from very diverse places. I also had some new things to write. In particular, the writing up of my time in the Meyerson administration, about which I had never even written a word. A lot of people had advised me to write that up over time. Mac Laetsch was forever after me. "Write that up! You're the only person who can write that period up," and so on. Gradually, it grew on me. I didn't know whether it could be an integral enough series of essays to become a book. I floated the idea to Lynne Withey at UC Press. Did she think this might be a decent idea? She waited ten seconds and said, "Do it." I think we talked about her role in insisting on having the first essay be auto-biographical. I came up with the idea of "reflections." The University of California Press made up the rest, *From the Free Speech Movement to the Global University*. My original title was *The University of California: Reflections of a Battle-Scarred Veteran*. They didn't care for that. They moved the "reflections" up to the first part of the title: *Reflections on the University of California*." This was their idea, this was going to be a cosmic thing from which all historians of higher education could learn.

40-00:45:34

Rubens: I think it serves that purpose.

40-00:45:35

Smelser:

It was okay. I thought it was a little flashy, and I didn't really write about the global university. So I had a little ambivalence about the subtitle, but I went along with it. Well, we're on that book already, but maybe in the time left why don't I just talk briefly about my trips abroad. I can do that in five minutes.

40-00:46:36

Rubens:

We didn't talk about some other teaching gigs.

40-00:46:46

Smelser:

Teaching. I'll say just a few words, because it's not highly dramatic. I was asked to go to Trento, which is an Italian university, which is not one of the leading ones, and it's not in the metropolitan areas of Italy. It's a small town in the Alps. Smallish. A hundred thousand in the Alps, and has a university, however, with considerable strength in the social sciences, with a lot of international appointments. A couple people there knew my work well. They said, won't you come and teach for a month? This was kind of interesting to me. I said, well, why not? Italy is close to Venice, and a beautiful part of the world. This is 2004. Yes.

40-00:47:35

Rubens:

Before you went to Illinois?

40-00:47:38

Smelser:

Yes. We went there in the spring of that month. I lectured on terrorism at that point. I was already in the middle of my book. They were actually worried about my lecturing on terrorism, because there had been a couple of incidents of attacks on professors in Bologna in the past year, professors who were thought to be politically not right. So they had a security guy come to my lecture. A sleepy Italian security guy who read the newspaper during the whole lecture. Nonetheless, they thought it was worthwhile doing. I taught on economic sociology. I consulted with perhaps a dozen individual students on their own research, and hobnobbed with a lot of the faculty. It was a very pleasant, engaged time. The sort of thing you like to do in retirement.

A couple of years later, on the advice of a former student, I went to the Institute for Social Sciences in Vienna. Vienna, one of my most favorite cities in the whole world, going all the way back to my Salzburg period, in which I went to Vienna a couple of times and fell in love with it. It was just wonderful to go back to Vienna. I gave a few presentations on comparative methods. They were especially interested in that. I lectured at the University of Vienna a couple of times. Once on terrorism. Again, kind of spread myself around. We really, really enjoyed the cultural aspects. You know this new European custom of giving concerts in churches? We fell in love with that institution, so we were into the St. Peter's in Vienna several times, listening to concerts and choral presentations and so on. Went around to lesser churches. We went twice to Berlin, though not to teach. Well, I did the Simmel lectures earlier.

We went to the Berlin churches all the time. It's a nice way to spend time. You're not hurried. It's not like a vacation.

Then we went back to Trento in 2009. They wanted me to lecture on different sociological topics. A kind of student of mine, Peter Wagner, a German, who'd gone to Taranto, was the one who organized it. I supervised some of his work when I was in Berlin. We became very close. Once again, I always joke. I say my life is now being organized by former students. The last trip is going to be to Bern, which is going to come up a month from now. I'm going to be teaching for an entire week on my work at the University of Bern, arranged also by a German former student, who now teaches at the University of Bern. These are nice little episodes, post-retirement episodes, that I enjoy ever so much.

40-00:50:38

Rubens:

The lectures will be a on—

40-00:50:41

Smelser:

On selected topics. It's a very intensive presentation I'm going to be presenting. Twenty-eight hours of seminars in that one week. Just as bad as these oral interviews.

40-00:50:55

Rubens:

What will your audience be?

40-00:50:57

Smelser:

They cancel all classes for the graduate students for this week, and so it will be faculty and graduate students who will be there. I'll give a couple of biographical presentations. I'll give a lecture on psychoanalytic applications. I'll give thoughts on the history of the field of collective behavior, since I contributed to it. Historical analysis. Joppke is the name of my host, a dedicated former student. He said, "We want to represent those parts of your work that you feel are most important." I will not talk about American higher education, because it's too peripheral to the interests of European students, but nonetheless, it's—

40-00:51:43

Rubens:

Will you write these out, Neil?

40-00:51:46

Smelser:

They're all finished. They're ready to go, yes. I have them ready to go.

Interview #21 September 29, 2011

[Begin Audio File 41]

41-00:00:00

Rubens: Good morning, Neil.

41-00:00:01

Smelser: Good morning.

41-00:00:02

Rubens: I think we should start with talking about the book *The Odyssey*. In your mind, when you left the Center and came back to Berkeley, you were going to begin writing, but 9/11 intervened.

41-00:00:37

Smelser: The idea of *The Odyssey*, I can't trace exactly when it started. It was well before I became director of the Center. I put it in my own mind that when I went abroad for the EAP, I had these expectations—I believe I explained it—that I thought I was going to do mostly my own work. I didn't have an engagement with the EAP itself. I knew I had to do the job, and I knew that I would do it conscientiously, but the student's experiences, my relationship with the students, the whole kind of event, were not salient in my mind. I was no foreigner to going abroad, especially in England, so I felt this was a kind of continuation. One of the things that, in a way, took me by surprise was how much I related to the students, and how much I identified with them, and how I formed these relationships with them of a kind of avuncular character. Supportive. At the same time I was evaluating their work, I was able to form these very kindly, Mr. Chips-type relationship with them.

What I saw in this was most of these students really experienced a kind of inner drama about the year. I had kind of a crude way of explaining it. I call it the U-shaped experience. I know I mentioned this in an earlier interview. When they arrived, they were all completely manic, ready to go. This was a wonderful experience. They were on this epic adventure. Then the reality set in. Winter set in. We thought that Thanksgiving dinner was a very good thing to have, because that was kind of the beginning of the descent into winter gloom. In the spring, they were fully engaged, and then they were going to get panicky about it's now ending, and then they get reengaged heavily once again. Well, that was a very crude, offhand way of describing things, and not everybody fit it exactly. I began to conceive of this thing as kind of a drama. It was, of course, reminiscent of many of my times abroad. I remembered my own biography, even though I hadn't put it into words. This thing began incubating and not exactly forming itself into a project, but I kept thinking about it.

In fact, when I wrote that book on committees, I began to think of the inner drama of a committee, which is not usually very dramatic. It's thought of as

being really quite boring, generally, but I thought, well, no, there is a beginning, there's an end, there's a journey. I even wrote it up and had some language in that about the inner emotional drama of being on a committee, even though you don't feel terribly excited about it from an emotional point of view. Then, on top of that, I began talking about it. I began formulating this idea in my mind. All the emotional ingredients, including the idea that it's a special occasion for people when they take a moratorium, and they feel special about themselves, and what are the psychological consequences about feeling special about yourself? Well, a lot. You feel good, you feel euphoric. At the same time, you feel guilty that you've been selected for something like a special journey.

I began to put it all together, and then Katherine Trow, the wife of Martin Trow, was writing a book on the Tussman Experimental College at Berkeley [*Habits of Mind*]. She and I got to talking, and I said, "Well, it must have been a kind of odyssey for these students, who would have these two years aside of their undergraduate career in which they were in this special journey with this leader and with a special structured experience and so on." She began interviewing me on the idea of what an odyssey experience really is. There, I had to kind of put it together. There is a section in her book, which is kind of the results of this interview she had with me, in which I articulated, much more than I'd ever done before, what the characteristics of what this special experience might be. Then, of course, when I was at the Center, I saw the whole thing unfolding again. Every participant there, even though they were adults, and even though they weren't students, had a kind of annual trip out of that. They were all engaged, and it was very communal. Nobody, even the most introverted and self-engaged person, didn't experience some kind of collective euphoria and adventure and positive feelings with that.

41-00:05:40

Rubens:

This was something you structured at the Center?

41-00:05:43

Smelser:

No, it happened. What we structured was freedom. We structured the capacity of these people to get together, in a group, and we organized minimal collective activities, such as lunches and a few seminars and so on. They experienced this annual journey, which they generally experienced as a great adventure. I always would tell them in the orientations, as I've mentioned in other interviews, "This is going to be the best year of your life, and next year is going to be the worst, because this is going to be over, and you're going to go back to the real world."

At that point, I decided this is something that is a generic phenomenon, and I'm going to do something big with it. I couldn't do it at the Center. I was doing a lot of other things. I was in the middle of the encyclopedia. I just didn't have the time, but I knew that that was going to be one of my first projects after I got out of the Center. I referred to it in my presidential address,

which was basically on affect and ambivalence. I referred to it, as I say, in the book on committees in the early nineties. So I was ready to go. I just didn't know what it was going to encompass. I didn't know how ambitious I was going to be in trying to bring all kinds of different experiences into it. When I came back to Berkeley, I began reading in the areas I knew were going to be important for me. I read Victor Turner on pilgrimages, for example. A magnificent author who really brought a lot of wonderful insights into the nature of pilgrimage. He engaged me. If I had a single dialogue with anybody, it was with Turner, in that book.

41-00:07:49

Rubens:

Where was he at the time?

41-00:07:51

Smelser:

He was at the University of Chicago. No, I think he had already died. He died relatively young. He wrote on pilgrimages, on drama, on the secularization of religious life. Very imaginative scholar. He wrote on contemporary protest movements as being in the same genre as some of the protest side of some of the pilgrimages. It was just very engaging. I didn't copy him, but he stimulated me to many new ways of thinking.

41-00:08:29

Rubens:

Had you met him?

41-00:08:31

Smelser:

Never. Only knew his work. Then I began reading and beginning to think and bringing new things in. I got the idea that the ordeal was a sort of odyssey, even though it had a negative connotation. It had some of the same drama as voyages and trips. I got the idea of treating psychotherapy as a journey. These are the things that began to accumulate. It was a lot of intellectual activity that was going on. I was just getting inside of it. I had one set of notes I called idea pages, and it was as long as the rest of my notes combined. As I would get ideas, I'd always enter, what do I want to make of this? Accumulations of my own approach, and so on, so forth. But then came the terrorism engagement. I had to put that on the shelf. I did casual reading. I knew I was going to get to it, but I had to delay, really, basically, I'd say two years, before getting into it.

Then I got this email from James Billington at the Library of Congress. He's the congressional librarian. It so happens that Billington was in Oxford when I was. He was one year ahead of me as a Rhodes Scholar, and I knew him when I was in Oxford. We didn't become friends, but we knew one another well enough that we permanently remembered—he was a Russian scholar. I read some of his work, he read my work. He was at Columbia for years, and then he went into all kinds of more applied work. He was at the Smithsonian and finally got appointed librarian of Congress. Very dynamic sort of guy. He wrote me this letter, saying, would you like to come on this Kluge Fellowship? This was anything from three months to a year, in which you just came to the Library of Congress, did what you wanted. They gave you a

salary. It was just one of these wonderful things, like the Center. There were a few other fellows, but it wasn't meant to be a collective experience in the same way. I jumped at the chance. I didn't want to spend a whole year in Washington. I knew I didn't have that amount of work left to do. I said, okay, I'll come for three months. I just buried myself in the new material that I knew I wanted to get into. I got into a lot of folklore literature about dramas, about rituals. Read a lot about honeymoons. All these new subjects that got into—

41-00:11:29

Rubens:

You had a desk and they paged materials for you?

41-00:11:35

Smelser:

What I had was an unbelievably efficient system. I would type into a computer demand system for books, up to ten, twenty books. Within three hours, they were all in front of me. Unbelievable. Then I'd just put them back on a shelf, and they would disappear. I formed some relationship with other scholars there, particularly Gerhard Casper, the former president of Stanford. Legal scholar. I knew him from my early years at Berkeley. He was in the law school here. Then he went to the University of Chicago. He was one of the main candidates for the presidency of the University of Chicago, and then he got selected to Stanford. I knew him when I was at the Center. We showed up. We hadn't had a fellowship together. We really bonded. It was really a wonderful part of that. In the meantime, really solid work, really efficient work, that went on under those circumstances. I had just put the finishing touches on the terrorism book when I first arrived. Threw myself 100 percent into the odyssey book. Got it all put together. All that was left to do when I came back to Berkeley was to write it.

Of course, I have to tell you that the writing of that book was kind of a sheer joy. For one thing, it had this biographical side to it. That, of course, engaged me a lot more than a lot of more depersonalized scholarship. I talked to Lynne Withey about it, at the UC Press, one day. I was having lunch with her on another matter. I advised the press periodically, so I was a kind of sociological ear. I decided to mention this project to her. I hadn't been talking more than ten minutes, and she said, "Let me sign you up." She was so excited about it. Well, it caught her imagination. She began telling me of odysseys she'd been on. Everybody did that when I talked about it. So she wanted the book. That side of it was completely taken care of immediately.

41-00:13:45

Rubens:

You didn't have to shop it around. It was just so simple.

41-00:13:47

Smelser:

There it was. They wanted it, they wanted it badly, and they took it. I told her I had a couple of biographical pieces that I wanted to put in because I thought they would fit. She said, "Make a whole chapter out of the biographical side. Furthermore, make it the beginning." She had this idea of what would engage readers. That biographical part wouldn't have been nearly so significant in

there if it hadn't been for her active encouragement. I sort of was ambivalent. Do I want to show myself off? Do I want to personalize this that much? I was ambivalent about that material, but she absolutely kept pressing me. It so happened that I had enough odysseys in my own life that there was no problem about it.

41-00:14:38

Rubens: Did she serve as your editor, literally?

41-00:14:40

Smelser: Yes, yes. She did a few special projects on her own. Usually it goes out to some of the other people. I have worked with Naomi Schneider more than anybody else, because she's the social science person. Lynne took this over personally. It was her project. Jim Clark took over a couple of my projects personally. The one on psychoanalysis, he simply said, "Do it. I'll be your editor." It was nice working with her in that way, too.

41-00:15:12

Rubens: It comes out in 2009. How long did you spend writing it, from this period of being the fellow at the Library of Congress?

41-00:15:20

Smelser: About a year. Was I doing other things? My usual life was going on. It took about a year to write it up. Then there's this year or so of production, and it came out.

41-00:15:34

Rubens: So you were pleased with it?

41-00:15:36

Smelser: I love that book. I really do. It's in a category like no other book I've written.

41-00:15:44

Rubens: Very philosophical, and I want to ask you some questions about it. It has this wonderful cover.

41-00:15:50

Smelser: My niece designed the cover.

41-00:15:52

Rubens: I thought that was the Christo fence.

41-00:15:54

Smelser: It is, but she put it together. She's a graphic artist, and she works for UC Press. She designed the cover of the University of California book. She's going to design the cover of the John Reed book. Okay, go ahead with your questions.

41-00:16:08

Rubens: She did a very good job. This is specifically Jess's question that I'm asking, since he couldn't be here today. One of the interpretations, the arguments you

make, seems to be that life consists of a continuous sort of structuring, destructuring, restructuring —of integrating, disintegrating, reintegration. Is there a self that you see that might be continuous, that goes through all of these processes, or is the self undergoing the same kind of construction, deconstruction?

41-00:16:52
Smelser:

These just happen. Things you don't expect to be odysseys can turn into them. If you have a very special year in fourth grade in which you develop a certain relationship with a teacher, and it develops and so on. In relation to a sense of permanent self, I think, is the question that you're asking. I regard these kinds of experiences as events and episodes that are continuously ingested into an always-evolving self-identity. They will change in one way or other. They become special romances that people continuously tell stories to themselves about. It becomes, I am this kind of person. I like this kind of thing. I love these kinds of people that I knew in this experience. I never realized I could get so emotionally involved. I see these episodes as being very important parts of an evolving self image. It's just not a static thing. These episodes of moratoria, or times away, have special significance for self image.

41-00:18:01
Rubens:

Clearly, this is a question that gets into debates about structuralism and post-structuralism, and the idea of self being contested in postwar sociology. Can you locate any of your thoughts on this in a larger movement?

41-00:18:26
Smelser:

I can comment on it. I had a funny episode down at Stanford. I was talking to a bunch of people at the Humanities Center down there, which is an institute on the Stanford campus. It's been more or less taken over by the postmodernist types. We were interested in maybe a little collaborative joint project for a few fellows who were in the humanities at the Center to get together with the Humanities Center, and maybe have some kind of collaboration and so on. I was engaged in an exploratory conversation. We were talking about what themes might be appropriate for some kind of collaboration. I said, in a moment of innocence, to these people, "Well, we might take the different social sciences—anthropology, sociology, psychology, and so on—and have a kind of comparative view of the evolution of the self as it's been regarded in these disciplines." I thought that was not a bad idea. I suddenly set off this, "Self?" They blew their stack, because one of the ideas in some of the postmodernist writing is the self is not a real thing. I was committing a sin of essentialism there by bringing up the idea of the self.

41-00:19:51
Rubens:

The self, in their language, is something that's made and remade.

41-00:19:57
Smelser:

Totally constructed. There's no way you can call it a thing. I was, of course, getting close to it when I was using this language. Anyway, that's kind of the

background. I never sold myself on this particular view of the self, even though I've just talked about it's evolving quality, it's changing all the time, and so on. That's not foreign to that way of thinking, but I don't get epistemological about it. I don't say there's no such thing as a self. I am basically a skeptic with regard to a lot of the postmodernist thrust. I would have some trouble rewriting, or rethinking, the kinds of insights I think I generated in this book to some kind of post-structuralist or postmodernist view of the self, or of the world. It's very process-oriented and, in some sense, relativistic in that I take a distance away from these things and try to draw out—but I'm interested in the typical. Postmodernists aren't interested in the typical. This is a book about the typical and how it manifests itself in so many different, apparently dissimilar, situations. That's very foreign to the postmodernist way of thinking, because I'm still engaged in social science. I explained at the beginning, I'm interested in general regularities in social life. I wouldn't be tempted to put it in that tradition in any serious way.

41-00:21:37

Rubens:

On the very first page of the text, you write, "The odyssey experience growing out of the necessities of the human condition..." While this may be a difficult question to answer in full on the spot, I think many reading these interviews would be extremely interested to know, what, to you, is the human condition and what are its necessities?

41-00:22:10

Smelser:

I had in mind to begin with the universality of the presence of time and space. Even though there are a lot of relative differences in how cultures and individuals characterize time and space, it's an exigency that impinges on life, whether you like it or not. Space is always a barrier. Anything you do, space has to be taken into account. You've got to move from one place to another. That takes expenditure of energy. It takes knowledge. It takes effort. Time, also, is a constraint. Also, there are certain rhythms in human life. The life cycle above all. The seasons. The maturation. The menstrual cycle. Just name it. Time is everywhere with us and puts a lot of limitations on adaptation. That's it. At the end of the book, I think I refer to essentials of the human condition. I said that the whole process of socialization, from the very beginning, is a matter of disrupting patterns that people are following in their own development. Maturation is, in a way, communicating to the growing child that this adaptation is no longer right. We shake you up. There's a crisis of socialization, in which certain patterns are regarded as infantile or not growing up and so on, so you put pressure on. Then there's a crisis kind of period. No, I don't want to leave that phase of life. I want to stay there. That's the exigency. That's one of the life conditions I have in mind, simple socialization. This is a recapitulation of the kind of structured moment in life that then gets deconstructed. There's a certain floundering period, and then a renewed level of consolidation of the individual self or persona or developmental level. These are the kinds of things when I say it's rooted in the exigencies of life.

41-00:24:27

Rubens:

You say, “Rooted also in the human struggle to deal with ambiguity, ambivalence, constant attention, and eternal flow. The dynamics of odyssey experiences cannot be fathomed without embedding them thus.”

41-00:24:47

Smelser:

That’s what I meant.

41-00:24:49

Rubens:

Here’s another question from Jess. Would you talk a little bit about the tension between the human awareness of mortality and the human imagination of immortality that you touched on in the end of your book, in your additional theoretical reflection chapters? First, metaphorically, does a proper odyssey experience always require death of the previous self? In a different way, in a more literal way, can you see an awareness of one’s finitude motivating these odyssey experiences in a way that Ernest Becker saw culture as being a denial of death?

41-00:25:38

Smelser:

That’s a tall order.

41-00:25:39

Rubens:

That is a tall order.

41-00:25:40

Smelser:

I guess I could say the following. At one level of consciousness, the human condition does involve a kind of denial of death. I have in mind the common observation that comes out of some of the psychiatric literature. That, until a certain age, people sort of believe that they’re not going to die. It’s associated with youth. Some imaginative authors have often said that the year in which this sets in for many, especially religious people, is age thirty-three. That’s when Christ was killed. You begin facing the idea. And, of course, changes in the body, changes in health, changes in the knowledge that time is passing come into play. Knowing that everybody has died in the world. It becomes elevated more to consciousness. My idea was not that every odyssey is literally a recapitulation of the life cycle. That would be, I think, an idle and very irresponsible claim. However, the symbolism of mortality, which, in fact, sooner or later, becomes kind of universal, even those kinds of changes that do not literally involve death and rebirth, they get assimilated into this universal imagery. Sometimes that imagery gets into it. If you look at the phenomenon of religious conversion, which is one of the things I treat in this book, it’s almost always some kind of rebirth imagery. You shake off an old self that dies. Then you enter into a new and better life. I have a feeling that, insofar as almost all human encounters usually involve some kind of engagement, then separation, then reengagement, then re-separation, that symbolism of the life cycle and death can get assimilated into all these things. I even suggested and tried to pull the reader in the direction of saying, this is what happens in committees. Committees die. I would say some of the most active and engaging moments in odyssey experiences will be experienced as a

denial of death. This is wonderful, it's going to go on forever, and so on. I'm really realizing myself in some way or other. I don't want to get too cosmic about all of this. That's the way I guess I'd have to summarize that.

41-00:29:04

Rubens: That's not what this is about.

41-00:29:06

Smelser: No.

41-00:29:09

Rubens: Good. I think that satisfies him. Then one more question he had about trauma. Trauma has attracted a lot of attention. You've written on it. Can you explain the analytic differences you see between the notion of trauma and the notion of odyssey? When you say, for instance, the odyssey experience is, in a word, a kind of repetition, compulsion that offers temporary respite from eternal attention, some key words are shared with the definition of trauma. This is not a question that engages you at all?

41-00:29:45

Smelser: I guess I would have to say that it's hard to draw an analytically neat parallel between the notion of trauma and the notion of odyssey. Some odysseys involve traumatic experiences. I have in mind the almost superimposed trauma that ordeals impose on people. I've explored it in that section on ordeals. Military training, rehabilitation programs. I even get into the literature on brainwashing. All of these have trauma as an ingredient, but the logic of trauma is, in a way, more compressed. It's just a small part of the whole process. Even positive odysseys, like travel, for example, can have a traumatic—and I do continuously point to the negative side of odysseys in that your life was shaken up, and you are in unfamiliar territory, and you kind of don't know what's going on, and there's some self reevaluation in all of them. An element of danger or uncertainty or mystery about odyssey experiences would come closest to trauma, but they're not always traumatic. It's just an overlapping set of concepts.

41-00:31:15

Rubens: You were pleased with the book, which is wonderful. How was the book received?

41-00:31:21

Smelser: As I told you, I think, in connection with some other projects, I tend not to be an avid seeker-out of reviews. I let the reviews come to me, I suppose. I guess I would have to say that every review I saw, which must have been maybe six or seven, was really quite positive in its assessment. They were always looking for things elsewhere in my own work that were relevant, and did this mark some kind of change, or whatever. We usually established a sort of dialogue. Jeff Alexander wrote one review of it, in which he said it was an advance in my work. He pointed out some parallels with my theory of collective behavior. He said the reason it's an advance is that, in his early

work, Smelser really treated the collective movements as somehow or other arising out of the human condition of pain or suffering or dissatisfaction or a strain. Which was a big part of that book. Why do social movements occur? Always an element of strain was one of the main analytic categories. He said, now he's rooted it into the human condition generally. You don't need to assume that they come out of unhappiness and so on. He saw that as a real change in the theoretical outlook. It caused me to think a lot about whether or not that was really the case. But I rooted it into more universal aspects, and not out of "these are troubled episodes" in the same way.

41-00:33:17

Rubens: So many more were positive.

41-00:33:18

Smelser: They were extremely positive. They're just so integrally related into the rhythms of life in general, not just when people are feeling unhappy.

41-00:33:29

Rubens: I thought it was a delightful book. It is very uncharacteristic, in some ways, but it has that thread of autobiographical moments that you've incorporated elsewhere, and of course is so studied in the variety of experiences you analyze and the research you bring to bear.

41-00:33:40

Smelser: There's another element relating to writing style that I would like to record. It's not specifically about this book, but this book is kind of a culmination, you might say. I had noticed that in my earlier work, and I would say this goes up through, certainly, the Industrial Revolution book and the theory of collective behavior book and in some other essays in the early part of my career, I believe I had a more explicit preoccupation with being methodologically and scientifically correct. One thing that makes that early work a little more difficult, I suppose, or less immediately engaging, is that I carry on too many running commentaries on what I was doing, from a methodological point of view or from a theoretical point. Fighting ghosts in the world. Critics, maybe. This was a way of, I suppose, establishing my own credibility as a social scientist, if I can take that kind of distance. I didn't think about that, but now recapitulate that as possible reason for that. Then, over time, my writing style tended to become less cluttered with these methodological and theoretical asides, and more direct, just getting to the point. Saying it. All theoretically relevant, but not stopping and telling the reader what I'm doing, which tends to be a deadening influence on a lot of prose. In a lot of the essays, as they accumulated over time, and in my writing style generally, I tended to just say it rather than telling the reader what you're going to say, and then say it, and then tell him what you've said, and what its significance is, and why it's this way and not that way. I believe that's been an improvement in my capacity to communicate, and it reached a kind of culmination in this book, which is certainly highly theoretical in its

implications. I don't say, here's the theory. I guess it's possibly a return of my journalism directness or something of the sort.

41-00:36:04

Rubens:

Well, I thought there also is—and I commented on it vis-à-vis the terrorist book—an interjection, ever so judicious and useful, but of a personal experience.

41-00:36:16

Smelser:

Yes. In particular, I use those boxes. Here, I use chapter two. Actually, in the John Reed book, I persuaded John Reed to write up eight decision moments in his own career, and we're entering them into the text as illustrations of biographical, concrete illustrations of the more general treatment we're giving in the book. The reviewers of the Reed volume especially picked out those episodes as being meaningful. I wanted to make that stylistic comment about my own evolution.

41-00:37:03

Rubens:

So this was fun, and it's still in print.

41-00:37:06

Smelser:

Oh, yes. I get people talking to me about it. I still talk about it myself. I regard it as a nice culmination.

41-00:37:17

Rubens:

There was one more culmination. Should we turn to *Reflections on the University of California: From the Free Speech Movement to the Global University?*

41-00:37:22

Smelser:

Yes, that's fine.

41-00:37:24

Rubens:

We've talked about aspects of the functions of the university, from free speech to the global university. We talked about several of the chapters in *Reflections*. Did we talk about how the book came about?

41-00:37:38

Smelser:

No, no.

41-00:37:40

Rubens:

Odyssey comes out in 2009, and this came out in 2010. You're churning stuff up and you're looking at ways to tie up certain ends, intellectually.

41-00:37:53

Smelser:

Yes, I think that's right. There's always some kind of trigger that goes into my decision to put something into a readily available form. Most of this book was not available to the general reader before. Three or four articles in it that were published before, but that's all. About two-thirds of it is not published. I never really conceived of it as being something I would want to previously publish.

Just as in the case of the book on psychoanalysis, two pieces of writing that I did more or less coalesced into a decision to bring all of my writings on psychoanalytic writing together. In the late 1990s, I wrote this essay on Erik Erikson as a social scientist. It was given at a commemorative ceremony in San Francisco. My friend Bob Wallerstein had wanted me to write something about Erikson, so I wrote this thing. Erik Erikson not thought of as a psychoanalyst, but as a social scientist, because he did write a lot about the larger society. I wrote a thoughtful and reflective piece on what kind of social scientist he was. Not uncritical, but at the same time, respectful of him. Then the presidential address. It's all about psychoanalysis. These two things were new. Everything else I had written, it's in the sky somewhere. Published in really obscure places. I thought, well, why not bring it together?

The things that occurred that told me that I might want to bring some of my writings together were the following. First, I had, over the years, been subjected to suggestions from colleagues that this 1965 episode in my life with Martin Meyerson, that I write it up. I would never meet Mac Laetsch anywhere without his first saying, "Have you written that up yet?" He would give me a lot of that. A lot of other people were suggesting that, saying "we didn't know that period of history that well, and you were in on it, and why not write it up?" That was one line. The second thing is, when I wrote this big memo for Massey and Peltason in 1994, just before I left to go to the Center, Peltason showed it to Atkinson, who came in as president about the time I went to the Center. One year after I went to the Center, Atkinson came in. Atkinson read it, went out of his way to write me a letter on it, saying, "Why don't you publish this?" He said, "This is fantastic." I didn't respond to it. I was down at the Center. I was just thinking it was a nice thing for Dick to say, but it didn't spur me onto any action at the time. Then the third thing that crystallized was my work with this Committee on Surprises, in which I volunteered to write up an analytic essay in connection with that committee's work. I was very pleased with that. I thought it was a good recapitulation of some of the political dynamics that the university had experienced over time.

These things came together, and suddenly there was something like a critical mass of things that I'd written very recently. I had a little trouble persuading myself to include major reports, or the third part of the book. The one on education, the one on lower division, and the one on athletics. I chaired all the committees, for one thing, and I also single-handedly wrote the reports. It wasn't a staff member. It wasn't other members of the committee. I did it. So in a way, they were my authored pieces.

41-00:42:21

Rubens:

Well, that section has a unifying title for the pieces, "Marrying Analysis and Action."

41-00:42:26

Smelser:

Well, that's really what it was. The university was facing a problem in each case. In the case of the education school, that school was in crisis. In the case of lower division education, it was in the middle of the 1980s, when there were these savage attacks from William Bennett, from the American Council on Education, and other quarters, on the quality of undergraduate education. That was a relevant thing. Then, of course, the athletics thing was generated mostly by Tien's ambition, but nonetheless, it was timely. As I said in the introduction, these issues have not gone away. It all kind of fell together, and I decided, well, I'll do this. They were mostly these things about the University of California, but on the other hand, these other essays that I've written, for example, on affirmative action and on multiculturalism and diversity, they certainly applied to the University of California, even though I framed them in general terms, affecting higher education in general. Somehow or other, I began thinking about this. I put together a table of contents. Seemed to make sense to me. I turned to my friend Lynne Withey. Ten minutes of conversation. She said, "Let me have it." That was the buildup to that.

41-00:43:52

Rubens:

It was published in 2010.

41-00:43:57

Smelser:

2010. The book was reviewed a few times, very positively. I personally believe that if I hadn't written that book, I would not have received the invitation to do the Kerr Lectures.

41-00:44:08

Rubens:

Is that right?

41-00:44:10

Smelser:

I believe so, because the book came to the attention of my colleagues in the CSHE. You know I was on the committee of the Center for Study of Higher Education that chose the Kerr lecturers?

41-00:44:23

Rubens:

Oh, I didn't know that. You had been interim director of CSHE.

41-00:44:32

Smelser:

No, this was after I'd come back from Stanford. The Kerr Lectures came about in 2002. They put me on the committee, and I was a vote on the committee to bring people like Charles Vest and Harold Shapiro and Hannah Gray to come give the Kerr Lectures. Last year, I didn't get an invitation to come to the committee meeting. They removed me from the committee. It turned out, Jud King explained to me, that I was a candidate to give the lectures. I didn't know that I was kicked off the committee. I just thought they were late in scheduling its meeting or something of the sort. Various people on that committee had closely read and knew what was in my book on the University of California, and had communicated very positively to me. David Gardner was on the committee. Dick Atkinson was on the committee. Jud

King himself was on the committee. Karl Pister. That committee was kind of inbred, but on the other hand, I was a bit surprised I was chosen to do the Kerr Lectures. I guess we're on to that subject now.

All four previous lecturers were ex-presidents of major institutions. Princeton, Chicago, MIT, and Stanford were the parent institutions of the four previous Kerr lecturers. Every one of them was published as a book. As a matter of fact, part of the money that came to the center from systemwide to set up the Clark Kerr Lectures, in 2001, was dedicated toward subsidizing its publication with the University of California Press. There's a contract between the center and the press that these lectures will be published. Well, they have to be reviewed by the Center. The press insisted on that, right. Once again, there's this linkage with the press that showed up again. They will come out, presumably, in 2012.

41-00:47:00

Rubens:

Jud King was responsible for the press when he was in the office of the president.

41-00:47:05

Smelser:

I knew that, because I was on—they called it Board of Control at the time, and Jud would always come to the meetings of the Board of Control. That's where I first got to know him.

41-00:47:16

Rubens:

We talked about that. That was quite a distinguished honor, to—

41-00:47:20

Smelser:

I'm sort of living it. I'm putting a tremendous amount of time into the preparation of these lectures, and doing a lot of the reading that I previously did casually. When some important book on higher education would come out, I would make a point of looking at it because it's one of my areas of research and continuing interest, but now I've thrown myself into it as a major project.

41-00:47:47

Rubens:

Have you been enjoying it?

41-00:47:50

Smelser:

Well, yes. I'll have to tell you that the quality of literature that deals with the contemporary university, say, from about 1990 on, is very dismal.

41-00:48:04

Rubens:

Really?

41-00:48:05

Smelser:

Oh, yes. It's a lot of doom and gloom, and it's the end of higher education as we knew it. Education is getting commercialized. It's become corporatized. Faculties are losing tenure. Also, it's dismal from a standpoint of who's to blame. Many people blame the administration and administrative bloat. Many

people think the faculty are wasteful and irresponsible and have given up their commitments. Just the dirge is unbelievable. One time, Sheldon Rothblatt said to me, in kind of a light moment—we're pretty good friends and we talk openly with each other—he said, "Neil, don't you ever get sick of higher education?" He's written a lot of historical work on it.

41-00:48:59

Rubens:

He ran the center for—

41-00:49:00

Smelser:

He ran the center for a long time, just after I had my brief period, I think. So I get a little tired of some of the literature, because it's all doom and gloom. What I decided to do is to take this fact that higher education draws so much from the view that a whole new world is coming, like online education or corporatization—and a lot of people celebrate that as a kind of coming new world. Most of the rest of the world says, no, it's the end of the world. Everything we knew and loved about higher education—its commitment to truth, its remove from the world, its high levels of purpose—they're all getting sabotaged and undermined by commercial values and so on. I call it the Pangloss/Cassandra complex. Extreme reactions to change. I'm building this as a major feature of the Kerr Lectures. What's this all about, all this choosing of higher education as being such an arena for extreme reactions, and shooting from the hip, and proclaiming the death of institution, proclaiming the birth of others, and blaming people all the time. And, in many respects, irresponsible analysis, because you know the world is more complex than these simplified formulae that come out. That's going to be a big part of the lectures themselves. Why this particular complex of reactions to change in the institution of higher education? I've got a lot of ideas on that subject.

41-00:50:56

Rubens:

This is particularly a time when so many students are so accomplished, successful, whether it's first-generation immigrants, or the children of undocumented workers. Of course there's the issue of who pays and doesn't pay, the burden on the middle classes. All of these entries into the system that never—

41-00:51:11

Smelser:

Everything is one-sided. Everything is normative, everything is value-loaded, and everything is emotional. You asked me about the literature, whether I'm enjoying it. Well, it's certainly instructing me, but it also led me to say, what's going on? I'm going to try to objectify the literature rather than just quote it and cite it. Say, why do we have all of this? But then I'm going to try to make a realistic diagnosis of some of these really fundamental changes that are going on, like the decline in public support. Like the terrifically rapid increase in non-tenured faculty in the whole system. It includes teaching assistants. It includes freeway flyers. It includes all kinds of other things. I'm going to say, what does this mean? That's what people say—tenure is ending. I'll talk about every facet that I can think of of commercialization, and try to evaluate

whether or not it's the end of the world, or whether or not it's just another adaptation on the part of the higher education system. I will not ignore these issues that are so much the subject of doom and gloom and tortured thought, but I'm going to try to step back. The first lecture is going to be historical analysis. I'm going to say that informs what's going on now. The second one is going to be how historical analysis generally explains a lot of the things we see going on in higher education. Then I'll get down, in the third lecture, to the actual nuts and bolts of some of the institutional changes we now see. That's going to be what the lectures are going to be about.

41-00:53:03

Rubens:

Despite the financial and budgetary constraints, so much incredible research is taking place at the university. The applications of research are amazing. The diversification of the student body is remarkable. There's a plethora of learning modalities -particularly opportunities for students to do field work and their own research.

41-00:53:22

Smelser:

A bit of it is the generational resentment. I'm advertising the lectures, and I'm going to try to do a scholarly analysis of this peculiar system. There will be some original sociological interpretation of what's been going on, and I hope that will be a contribution that's not often made. So, there we are.

The first three lectures will be at Berkeley. Actually, I'm giving the third lecture twice. I'm going to give it in Riverside, which is sort of the contractual ingredient, that one of the three lectures is given on another campus, as a UC, rather than just a Berkeley, event. The first lecture is going to be in mid January, the second in late January. The third, at Riverside, is going to be in early February. Then the next week, I'll give the third lecture again, here at Berkeley. So all three will be available to the Berkeley audience.

41-00:54:59

Rubens:

Tell me about pursuing a dormant passion for being a thespian.

41-00:55:21

Smelser:

Okay. There is, in my own background, that I talked about somewhat. During my young years, up until about the time I went away to college, at age eighteen, my father was a teacher of speech and drama in Phoenix Community College. He got a master's in drama at the University of Iowa, and got a job in the Depression at Phoenix College, where he spent his whole life. In the early half of his career, he was teaching speech and drama. In the meantime, he was going to summer school and summer school and summer school. Trips back to the Midwest. He changed his career into teaching philosophy. He's always preoccupied with general issues of society, politics, and the world. He was happier with that change. But in the drama period, he directed plays, he wrote a play, at the college, and at an institution called the Phoenix Little Theater. A little civic organization in a smallish town at that time.

He recruited me to come help him build the sets. The flats and the props and so on. We would go out there on Saturdays and Sundays and make up the stage. I was handy enough, and he would teach me how to do things. I went to all the plays. That was part of the picture. He chose plays that were not exactly right for that little provincial community. For example, one I remember, he put on a production of *Ghosts* by Ibsen, which is a very serious and provocative play about venereal disease and society's problems. A very Ibsen-Shaw-like play. He's always an intellectual, and he's always thinking about things just rather than light comedies and so on.

41-00:57:46

Rubens:

He was the producer, director. Did he also play?

41-00:57:50

Smelser:

He didn't act. He directed. I did a little high school acting. I played in a somewhat cheap mystery play, and I also played in a production of *Lost Horizon*. Really quite liked it. My dad came and saw both the plays. He commented on my work. I was also very big in public speaking in high school, competitive public speaking and debate. All these were my activities. Suddenly, went to college, all this stuff died. I gave up playing the violin. I didn't get into any acting. I thought of going onto the *Crimson* and didn't go into the *Crimson*, from a journalistic side. I just threw myself into my studies. I became a kind of academic fulltime. Interest in drama underwent a kind of incubation. All the times we've been in England, our chief entertainment has been drama. We're going to see some plays when we go now to London. The love for drama didn't die at all. I love the Gilbert and Sullivan musicals. I love the serious drama. I love the live drama. But I never took part in any.

[Begin Audio File 42]

42-00:00:06

Smelser:

The theater continued to be a big part of our lives. We went all the time in London. Every time we would go to New York, when I was on some kind of scholarly assignment or foundation work or something, we'd always make a point of going to some kind of play, or plays, when we were there. The year we lived in New York, 1989 to '90, we made an explicit vow to ourselves that we were not going to calculate economically what plays cost us, but we were going to stay away from expensive dinners. This was our contract with ourselves as to how not to go broke in New York.

In the meantime, our son, Joseph, who, in his days in primary school—it's the equivalent of third and fourth grade in London when we were abroad for the Education Abroad Program—he was in an English school, St. Michael's. They had an extremely active drama participation on the part of kids this age. He played all kinds of roles in all kinds of plays. He played Menelaus in one of the Greek representations. He was very good and obviously a very motivated actor. When he came back here and when he was at the Head-Royce School and at the CPS, he was very active in drama, and went to

Oberlin, majored in drama. Of course, he ended up being, now, a stage manager. That's been his major career. Here in Berkeley, we were early followers of ACT, and since then have migrated mainly to be season ticket-holders for both Berkeley Rep and Aurora. We go all the time. Our son, Joe, was the stage manager at Aurora, and then ACT for a while, before he went to Seattle. Now he's stage director at the Shakespeare Theater in Washington, D.C.

Now, my own role. We joined, just before going to Palo Alto, a group called Berkeley Drama Section. It's one of these faculty wives' creations over the years. No longer faculty wives. Just people, usually couples, go. It's a small—one of the sections of the club. We decided to join it. What it does is to engage, monthly or every six weeks, approximately, during the course of the academic year, play readings. You read to each other. We found that very entertaining. We stayed in it for a couple of years.

42-00:02:57
Rubens:

Who decided what was going to be read?

42-00:02:59
Smelser:

They always have a little committee, or a person they designate as producer for the year, or something of the sort. A person takes charge, and then recruits, usually, members of the group to do the readings. It's very amateur, very self-regarding sort of outfit. We enjoyed going to it. We dropped out when I went to Stanford. We just didn't make those trips up for those meetings. Came back. Didn't join right away. Somehow or other, I got taken under the wing of Grace Fretter. She is the widow of Bill Fretter, who was the Dean of the College of Letters and Sciences for years and years and years during my career on the Berkeley campus. I knew Bill because I'd get these outside offers to go to other universities, and usually the College of Letters and Sciences would join in the effort to keep me here. So I'd go and see Bill Fretter every couple years, so we got to know each other that way. Then later, he was always hanging around the Faculty Club. He owned a vineyard up in one of the counties. Bill and I were friends. I didn't know his wife very well. He died quite some time ago, but she has survived him. She's now ninety-four or five years old. Very active in this group. She sort of spotted me as a person who'd be a good reader of these plays. About four years ago, I sort of idly agreed to play the part of a vicar in a play. I turned out to be sort of a lecher, seducing women around this congregation during the whole play. It was a very funny play, and so I decided to read in it. It was a lot of fun. I enjoyed it. It was completely amateur, and you didn't act fully, but you'd try to put yourself into the role even though you were reading it and not memorizing and acting fully. That was kind of a hit, so they continued to ask me to read in things. I read in a Noel Coward play. I read in an Alan Bennett play on Guy Burgess, the English spy who's living in Russia. I read the Burgess role. As I say, I recently read a role of Napoleon. So we keep active in this. Maybe twice

a year, I will do a reading. It's all very enjoyable and I get a lot of positive feedback.

42-00:05:41

Rubens:

Has Sharin been a reader?

42-00:05:43

Smelser:

She is totally terrified of public appearances. She would be the last person in that whole group to ever appear in a play. It's just not her style. Anyway, she's very supportive of my doing it. I always call Grace my mentor, my muse. I'm her protégé. We have a good, happy relationship with each other, and I'm always thanking her and everything. Then there's this outfit called the Town and Gown. It's longstanding, at least a century old, I think. It is what its name suggests. It's made up of people from the university. It's a women's club, made up of women from the university and women from the community. They meet. They have a lot of cultural events, a lot of speeches, some musical events, and they have an annual play that they put on, an amateur play. Sharin got chosen to be a member of Town and Gown. This had nothing to do with me. She was just nominated by a few people. I think Grace was actually active in nominating her, and a few others that we know in our social life. So she got elected. That, then, permitted me to be possibly recruited.

She's very active in the drama productions of Town and Gown, Grace. I was encouraged to go to the tryouts about two and a half years ago. This is all completely amateur, but nonetheless, a real production. You learn the lines and you interact with the other players, and there's a set, and there's a director. It's really a performance. Grace encouraged me to go for the tryouts about two years ago. They were going to produce the play *Harvey*, about the imaginary rabbit. I went to the tryouts. What you do is just go around the room and read. The director and the producer called me up and they commented on my voice. Said, "We'd like you to play the lead." His name is Elwood or something. He's the guy who imagines the rabbit. I was sort of extremely stunned by this, flattered, but I said, "You know that I'm going to have to be away for my Guggenheim trip. That's committed at least a week away, and if I take that role, I think it will ruin the play for my being absent that long." I said, "Is there some way I could take a lesser part?" Because I was eager to do it. So they gave me a lesser part, but it turned out to be kind of a major part. I played the psychiatrist, Dr. Chumley, who puts Ellwood into an asylum. He's kind of a weird guy. He gets converted into believing that the rabbit really exists. It's all very comical. So I played this psychiatrist, who's an irascible old character. A very pleasant role for me to take. A lot of shouting, a lot of hamming. A comedy role. Loved it. I absolutely loved it.

You get a lot of strokes in this if you did all right, so I got a lot of positive strokes. Every time we'd go down to a Town and Gown event, people would come to me, "Are you going to go out this year?" and so on. I did. I go in for tryouts. They were putting on three one-act plays. One was Chekhov's *A*

Marriage Proposal. One was *Sorry, Wrong Number*. This one had been a movie, a very scary movie, with Barbara Stanwyck. Then a third, a French farce. They asked me to play in *A Marriage Proposal*. I played the landowner who's trying to marry his daughter off to this neighbor. There were only three of us in the play. It was a beautiful part. I loved it.

42-00:11:03

Rubens:

What did you love about doing this?

42-00:11:12

Smelser:

I guess the exhibitionist side of me that I also experienced in my lectures. I was always motivated to give lectures, and I always believed that they should have something of a dramatic element in them. It wasn't to show off, but you have to make them come alive. You can't just spew out summaries of articles and things and expect students to be in any way positively motivated or engaged. I tried to lend an intellectual coherence to what I was trying to do. I prepared very well, always, and tried to give a lecture some kind of structure so that you built it up into a series of questions, and then you began to try to resolve some of the questions. I had a self-conscious sense of trying to organize lectures in a way that would have, at least, some minimal drama to them. I would sometimes joke with the class. I said, "This lecture is going to be a symphony." Meaning movements. This is the climax. So I would joke about it to my class sometimes. I think that's all kind of filtered through the influence of my dad and a lot of the speaking I did in high school. I had a background of this sort of thing. There was this frankly exhibitionist ingredient in the whole thing.

42-00:12:34

Rubens:

You could also partake without exerting too much.

42-00:12:38

Smelser:

Well, yes. But there were quite a few rehearsals. Rehearsed almost every evening. A lot of time commitment, but it was always in the evening hours and so on. I also liked working with the other people. That was it. You really form a little tight-knit, odyssey-like group. Just an adventure that you're in and so on. In addition to acting the role of Tschubokov, who finally gets the daughter married—it's a highly comical one-act play, a beautiful play—I also acted in *Sorry, Wrong Number*. I played the murderer. Actually stabbed a woman on stage. It was not a very large part, but it was an essential part to the play. Then a very minor part in—

42-00:13:37

Rubens:

Foreign role for you, of course –the stabber, violent.

42-00:13:44

Smelser:

I joked with them, saying I missed my murder fix on weekends and would get withdrawal symptoms from not enacting this brutal murder. Again, a small part, but a very different one from the other one. Then a tiny little part as a member of the board of trustees in this French comedy. So I was in all three of

them, but in the third one, I had only one line. It was just a cameo-type thing. But once again, I enjoyed it tremendously.

42-00:14:11

Rubens: So you think you'll continue this?

42-00:14:15

Smelser: The tryouts begin in January. I'll go back. I heard a rumor that they're contemplating a musical, which I think would rule me out. I have had to sing in a couple of these reading plays, but it's not my forte. But if there's a play and there seems to be a spot where I might be able to play a role, I'll do it again. They put them on in April.

42-00:14:41

Rubens: Did you tell me that you're a bit of a mystery buff when I was asking you about books you read? Do you read mysteries?

42-00:14:49

Smelser: Well, some. I don't do it so much anymore. It usually was my airplane reading. I'm a tremendous fan of Ken Follett, his type of mystery. Len Deighton. I read quite a bit of Len Deighton and some of these others. Light reading.

42-00:15:08

Rubens: There is a wonderful exhibit in the Brown Gallery, on the other side of the Doe Library. It's about mystery writers in the Bay Area. Very, very well-conceived.

42-00:15:18

Smelser: I might drop by it.

42-00:15:22

Rubens: You would enjoy seeing it, I think.

42-00:15:24

Smelser: I wouldn't say reading mysteries is a big part of my life. Particularly, it's kind of atrophied in the last three or four years. Less traveling. I don't read them at home. I consider them a waste.

So that's my drama career as it's unfolded. It had a big incubation between age eighteen and age seventy-nine, and then I took it up.

42-00:16:04

Rubens: Let me look at my notes.

42-00:16:07

Smelser: You wanted me to reflect on sociology at Berkeley.

42-00:16:09

Rubens: Yes. This is maybe a little odd in terms of transition.

42-00:16:14
Smelser:

No, I'm prepared. I'll probably put in a couple of remarks about the American Sociological Association as well, which I was closely engaged with my whole life. When I came to Berkeley, 1958, the sociology department was only six years old. It had been a department called Sociology and Social Institutions. The faculty subgroup of social scientists, including Alfred Louis Kroeber and other powerful voices on the campus, did not want a sociology department on the campus. There was a department called Social Institutions, which was quite philosophical. It was sort of dominated by nineteenth-century theorists. It was not at all a general department. It was kind of a freak accident creation of the Berkeley campus, the history of which I'm not totally certain. A man named Taggart [Frederick J.] was the big leader of that department for years and years and years, and he resisted a sociology department.

1952, they brought in kind of an all-star sociologist named Herbert Blumer, and they formed a department called Sociology and Social Institutions, as a gesture to the old one, bringing those faculty members in. There are only a couple of them left. Along with Clark Kerr's encouragement, the department began a really aggressive recruiting and growth pattern. Those were the rich years, of course, fifties. They brought in a number of superstars, all of whom had either been or were destined to be president of the American Sociological Association. I'm talking about Philip Selznick, Marty Lipset, Herbert Blumer himself, Kingsley Davis. These were all sociological superstars, and they formed the core of this department, whose mission it was to build. The building process was well underway by the time I got here. They'd recruited several young people. Erving Goffman was one of them. I came in '58. The department was in full swing, fully growing, so ambitious. The university was rolling in money. That's really what makes it such that it could be called a Golden Age, because of this growth, and it was already rocketing itself up into national recognition, which it didn't have before at all.

There was a good collegial relationship among the people. I felt distance from some of them. I never liked Kingsley Davis very well, and I had a very remote relationship with Herbert Blumer, who was sort of the king of the study of social movements and collective behavior. I think he resented the fact that I was even writing on the subject. He was chairman when I was hired. We had a civil but somewhat remote relationship. Never came into open conflict, but didn't really have much to do with each other. I had a real feeling of membership. My feeling of identity with the department was a feeling of excellence, a feeling of relevance to the national scene. I was very happy at Berkeley in those circumstances. Was a full member of the department, promoted early. Seemed to enjoy the full confidence of my colleagues. It was almost like it was my main environment. I didn't really have much sense of the larger campus. It was a large campus. Berkeley suffers from a certain amount of siloization. Though I interacted with quite a few people from the history department, and some from the political science, just because of the generality of my interest, and there were a few people in anthropology and

economics that I had known either before—so I ventured out, but the department was my place.

Then came two events that decisively influenced my relationship to the department. First of all, the department itself fell apart in the Free Speech Movement. Absolutely fell apart. Divided hopelessly between people who were actively engaged with and really supported the students, the leftwing group, and then people who were turned off by it and who ultimately sort of left. This was Nathan Glazer, Marty Lipset. Goffman left, though he wasn't especially alienated from the politics. Some people from economics, like Rosovsky and Landes, went to Harvard. The department had a rebuilding job to do. Most of the people who left were kind of on the conventional right. There wasn't really a right wing in the department, with a couple of exceptions. It was just moderate, standard, committed academics, and these other people got politicized. I did not welcome this division. People started fighting with one another. There was always the pressure to take sides. I was very impatient with the left, even though I still kept friendships with people like Matza. I felt a little more identified with what the center was, but I was sort of the center of the center, not a reactionary who thought the world was falling apart.

42-00:21:34

Rubens:

Had Bellah come in by then?

42-00:21:35

Smelser:

Bellah came in a little later. We hired Bellah in '67 or so. I believe it was '66 or '67. I was active in hiring him, actually, because I knew him at Harvard. I had known him at Harvard. I actually advised him. When he was just about to take the job, he called me up and he said he had had this background. He had gone to a lot of Communist Party meetings. He had gotten into a little trouble at Harvard with the McCarthy period. He was very worried about his political future at a place like Berkeley, because it had had the Loyalty Oath and everything. I really, basically, was the one who kind of persuaded him to come. He called on me because that was when I was in Meyerson's office. That polarized the department for almost twenty years. They stayed polarized during the antiwar protests. The divisions between the right and left in the department spread out to their ideas about what graduate training would be like. A lot of fights about who new appointments should be, according to their politics as well as their academic excellence. The whole thing was just fights. That was a period in which I was progressively more alienated.

In fact, when I had my huge offer from Harvard in 1970, along with the Yale and Penn offers, Roger Heyns came to me and said, "Neil, we're prepared to do anything for you to keep you at this university. What can we do?" I actually mentioned to him the appointment as university professor. One of my motives, it wasn't just that I wanted the glory of that position, was I could teach where I wanted, when I wanted. There was an escape. While I was still

budgeted in the department, I was supposed to teach on other campuses, and I would teach at my own whim. Now, I always made it a point, after I got that, to teach a full load, as it was defined for me. I never took advantage of it. I took very seriously the imperative to teach on other campuses. That's the side that I never confessed to anybody, but I was increasingly getting alienated from that divisiveness in the department. I continued to play a role in the department. I was chairman in the seventies for a couple of years. I served roles on major committees, and I chaired it later in the early nineties, but I disengaged. I taught on other campuses during that period. I went abroad for a total of three years in the seventies. First of all, sabbatical, and then the Education Abroad Program. But I would continue to go to meetings and I served on committees and I taught my big share of graduate students. I think I probably had more graduate students than anybody in the department. But that political divisiveness was something that was not for my blood. That feeling of—it was truly some alienation—never died. During my entire remaining career with the department, I kept this distance.

Now, things changed around 1980—I guess I can put it about that time—in which the bloody fighting over the old issues seemed to die down. People were beginning to retire, so that particular division didn't exist. New people coming to the department didn't quite know what all the fighting had been about, so they didn't exactly join in those coalitions. There were still divisions in the department between those who were methodologically-oriented and those who were softer, you might say. There were divisions according to one's political emphases, but they weren't so salient as they were before. Then, of course, there was a lot of dispersion of subject matter, sub-specializations in the field. You tended to talk to people that you had more in common with. It still was a department that had a certain amount of fragmentation and only a limited level of civic commitment to it.

42-00:26:10

Rubens:

Civic commitment meant?

42-00:26:12

Smelser:

Meant the department, as a unity, trying to further the interests of the department, voting consistently according to the quality of the people we were trying to promote, avoiding getting involved in huge fights that would spill over into the dean's office or the office of the chancellor. These things are never helpful, and I never liked them at all. It was a very interesting thing, of continuing to be a citizen of the department without having my heart fully in it. Over time, it began to wane. Of course, I left in '93. I still had a lot of individual friends in the department, and still do see people. Lots of students, but they're mostly scattered around the world. In '93, of course, I made a break with the department by leaving for my time in Stanford. No, '93, I left for the president's office, then I went to Stanford. Then I came back. Now, the department, it kept its excellence. No question about it.

42-00:27:33

Rubens: That's what I wanted to talk about.

42-00:27:34

Smelser:

No, no. I have absolutely no doubt about the new appointments it made. They brought themselves around, and they're still up in the top one or two or three departments in the country. It kept its stature as a department. Chicago went downhill. It was the first. Wisconsin is still extremely strong. Places like Princeton and Stanford have moved up, but Berkeley is still right in the top. It's very elite. That, I like, the quality of the people. I have not made a point of engaging myself with the younger faculty members. When I came back, they asked me to teach one course, so I taught it. But then I sort of felt, do I want to reengage in the department? I didn't feel I had much heart to do it, to get it back into the—I was emeritus, and I would have a more limited role anyway. I could have taught indefinitely in the department. They tried to get me to teach a theory course later, and I did agree to teach with Victoria Bonnell, one of my best friends in the department, but she then got appointed some administrative position and she didn't give the course, so I didn't teach it.

Then I also got this teaching role in the School of Public Health in 2002. That was just right. It fit what I wanted to do. It was a pleasure to teach these very few, really highly qualified social scientists, who were the fellows. I interacted with an economist and political scientist with whom I had a good relationship. It was interdisciplinary. It was one term a year. It had some income, but that was a completely incidental part of it. I stayed on there. I'm still doing it.

I went back and gave a colloquium to the sociology department. Basically came back two years ago. They wanted me to come back and talk about my experience in the department, talk about the history of the department. That was a very gratifying appearance. A great, huge crowd showed up for it, both students and faculty. I felt quite honored, and a bit surprised, actually, considering my coolness to the department, that they should give me such a warm reception. That was a bit of a surprise, but that was my problem rather than the department's problem.

42-00:29:58

Rubens: Are there people there that you have, over the years—

42-00:30:05

Smelser:

Claude Fischer is one of my good friends there, and Michael Hout, a very outstanding survey analyst, political sociologist. I had an ongoing relationship with Nancy Chodorow, who was my psychoanalytic link. She went through the institute. I was kind of her guide and mentor, so she and I had a quite close relationship.

42-00:30:23

Rubens: She's moved to Boston now, is that right?

42-00:30:25

Smelser:

Yes, she's retired and moved to Cambridge.

42-00:30:37

Rubens:

Then Bellah. How long did—

42-00:30:40

Smelser:

Bellah and I had a break. It was all over a completely incidental and tragic matter. When we moved out of our house, we got into that threatened lawsuit about a parking spot. His wife was especially ferocious in this sort of thing. I didn't feel comfortable with Bellah, even though I personally didn't have much—we were fairly close before that, but we didn't have much to do with one another afterwards. Arlie Hochschild, I'm very close to. We have lunch every several months, six months. She wrote an introduction to my theory republication. I like Victoria Bonnell, who's a Russian scholar that I hired here when I was chairman. But they're scattered and individual relationships. There's a young faculty member who was a post-doc in my teaching at the public health school, who got a junior appointment in sociology, named Cybelle Fox. She and I have maintained a kind of friendship. She comes back to these seminars. These are the individual contacts, but they don't come through the departmental context. They come from older friendships that thrived.

42-00:31:53

Rubens:

And Marty Lipset? You mentioned that you were going to rent his home in Stanford. Did you stay in touch with him?

42-00:31:57

Smelser:

Somewhat, yes, I would say so. Not frequent or intense. I respected Marty's mind enormously. A really smart man. I had some trouble with his personality. He was a little—He was too self-promoting for my taste. I kind of put that aside, and we interacted very well. We had collaborated earlier. We kept a relationship, an ongoing relationship. I kept a close relationship with Henry Rosovsky, who's an economic history friend. Selznick is a neighbor. We had a long-going relationship. He died about six months ago. Or a year ago, maybe. Of course, I had a continuing, ongoing relationship with quite a few students. All of those that appear in that festschrift are still people that I—we don't seek each other out. We see each other. I keep a very close relationship with Gary Marx. He and I are very close friends. So there are still a lot of linkages around. Hal Wilensky, who went to political science. He was one of the people who left the department in the heavy years. He went to political science. Reinhard Bendix went to political science.

42-00:33:36

Rubens:

Did you stay friends with him?

42-00:33:37

Smelser:

Oh yes, until his death. Guy Swanson, whom I wasn't terribly close to, but we had a certain good relationship intellectually, went to psychology. I kind of

lost track of him. He's also died. These are all linkages of people around. Some of them kind of continued through the American Sociological Association when I would go to the meetings and so on. You just casually get together with your old acquaintances. Many students.

42-00:34:14

Rubens:

Was there ever a point when people would refer to the Berkeley School of Sociology, as was done for Columbia or Chicago?

42-00:34:22

Smelser:

In the early period, yes. It had the reputation of being first-class scholarship, committed to more politically relevant subjects, heavy on theory. If it was weak on anything, it was weak on methods. Nonetheless, it had a strong voice in institutional, historical, and political sociology. That was its kind of range of identity. It sort of kept that, in a way. It also then got a reputation of being more radical than many departments, and more prone to inner conflict than many departments in the country. Now its reputation as a Berkeley School is probably diminished at the present time. It's gotten eclectic, it's spread out. It's kind of lost its character, except for being first-class. Substantively, you can't pinpoint it in any of these general reputational categories. One of the people in the department, Michael Burawoy, has tried to rewrite its history, almost always, as being a representative of, quote, "public sociology." He's created a little movement that he himself has headed up, called public sociology. He's a kind of former Marxist, very critical, very—

42-00:35:53

Rubens:

You taught a course with him once.

42-00:35:55

Smelser:

We taught a course together. I had a somewhat fraught relationship with him.

42-00:36:01

Rubens:

So his public sociology is something that he doing at Berkeley?

42-00:36:14

Smelser:

No, no, it's his own national movement, but he tried to rewrite the history of Berkeley sociology being a history of public sociology. When I first came back from Palo Alto, they had a colloquium on public sociology that he was just then beginning to trumpet. He and I were the colloquium members, and we really locked horns there. Probably I became more aggressive to any colleague than I've ever been before during that. I wasn't irresponsible, but it was fundamental objection to exactly what kind of world he was defining, and how he was rewriting the history of the department that didn't really jibe. He now asked me, just six months ago, to contribute to an international dialogue on public sociology. I said no thanks. I don't know what I can say that's new. I was polite enough. I said, besides, I'm spending all my time on those Kerr Lectures. He wrote back, very enthusiastic, "Oh, you're giving the Kerr Lecture? Wonderful." A rave and so on. "I have to come. I'm very engaged in all these issues I know you're going to discuss" and so on. But we never see

each other socially. It's sort of a non-relationship that I have with him. In any event, I guess that's my take on where the department has been.

One of the things that I may do—you asked where I'm going after my lectures—quite a few people have asked me to write a history of the department. I'm not too enthusiastic about doing that, but that's one thing possibly on horizon. I'm not doing it just to prove Burawoy wrong, but I could yield a lot of personal history and reflections on colleagues. It would be a mix—institutional, biographical.

42-00:38:16

Rubens:

I imagine, at the same time, being able to talk about trends in sociology generally.

42-00:38:20

Smelser:

Oh yes, there would be a lot to say. No question about it. Though I can't say I've decided I'm going to do it. It's one of the candles that are waiting out there, possibly to be lit.

42-00:38:34

Rubens:

This usable sociology, social science—

42-00:38:37

Smelser:

That's going to be very interesting, because that, in a way, is, quote, “public sociology,” without the radical tint. In the time that remains, let me say something about it. It has to do with my relationship with John Reed, which I have covered. CEO of Citibank. We were on all these boards of directors together. He was on my board of trustees when I was at the Center. We became, gradually, built over a long period of time, both acquaintance, then friendship. When he married for a second time, we got on famously with his second wife, and we became families together over time. After I retired from the Center, we kept contact. They invited us to come to their French home on this island off the coast of France. We went there for a week when we were traveling around Europe and I was giving lectures. We went and spent a few days with the Reeds. We would always stay in their apartment in New York. They have six homes, one of which is a loft apartment in New York that they put their friends up as guests when they come to town. We always stayed at their guest apartment. We visited them in Massachusetts, where they now live.

In 2002, he came to me and he said, “I've been interested in topic on applied social science for all my life, but I've never really done anything by way of fostering anything in it.” He'd set up this big foundation, but he hadn't done anything to support social sciences yet. He approached me. He had gotten to know me as being a leader in the field, and certainly very interdisciplinary during my time there. He developed an intellectual respect. We developed a personal respect for each other. He asked me, “Won't you take the lead in putting together some kind of project on applied social science?” Don't know that we came across the word “usable” yet, but that evolved. I kind of thought

about it. I wasn't quite sure. I had written a couple of articles that had drawn some attention to sociology and social problems. It wasn't foreign territory to me, and I was quite intellectually satisfied with the work I had done on the topic. I said, "OK, John, I'll do it, but I don't know what we're going to do. I don't know what the project is going to be." I said, "I'm most comfortable with some kind of book-length project on the idea, but should it be mine, should it be a collective thing, should it be an edited volume? Should it be a series of colloquia? I don't know."

We agreed that we would have a meeting. I believe it was in 2004 or 2005. 2005, I think it was. Anyway, I was in the middle of my odyssey work, I think, when it happened. We got together six all-star social scientists from different fields. Some of them had been to the Center. John knew some of them independently. I knew them. I knew the leaders in the field because of my work in the Social Science Research Council. We got six people together in New York. We got Citibank—John paid them handsomely out of his foundation for them to come. We had perfect attendance with these very busy people because he bought them off, and they came to New York and we had a really lively and productive meeting.

42-00:42:28

Rubens:

Should we say who they are?

42-00:42:29

Smelser:

Yes. There was Alan Krueger, from economics, who has just become the head of the Economic Advisory Committee to the president. There was Steve Stigler, a statistician. There was Dick Scott, and organizational theorist. There was a guy named James Peacock, an anthropologist. The psychologist was a very famous cognitive psychologist friend from Princeton. Finally, a political scientist, whose name also is slipping my mind but will come back. Anyway, we got together. They were all—

42-00:43:22

Rubens:

Reed's role was going to be—

42-00:43:24

Smelser:

It wasn't clear. Reed's role wasn't clear. We had this meeting. I took extensive minutes. I thanked everybody. They gave tremendous suggestions as to what you might put into such a volume, mostly from their own fields. I was aware of a lot of the things that were going on, but certainly not everything. Jervis was the name of the political scientist. Rob Jervis. Susan Fiske was the psychologist. That's the whole list. Reed was there, and I was there. I chaired it. Reed hosted it in the bank. All this happened. Then what? What do you do? Are people going to co-edit it? Are they going to write for it? Who's going to do what? So on and so forth. Reed and I batted this thing back and forth. He didn't want it to die, and I didn't want it to die either. I was, by that time, engaged, but I wasn't sure what I was willing to do.

Reed and I talked fully about it, and finally I came to the point where I was ready to make a commitment. I said, “John, I’ll take the leadership in this. Furthermore, I want it to be a book written by one person, not a collection.” These collections, you never get any unity at all in them, no matter what you do to the authors. You can beat them up, you can demand a common style, a common approach. Never works. You just do not discipline scholars in this way. It would be yet another collection. I thought it would have more limited impact. So I said, “John, I’d be willing to undertake this.” He then, immediately, because it was not a neutral statement—this is a multiyear project, no question about it—John then, being a practical businessman, began negotiating with me as to what it would take to do it. He wanted to pay me to do this project. He had this enormous foundation. He wanted to pay me. He wanted to supply me with any consultation I wanted. He wanted to supply me with an unlimited amount of funds for research assistants and travel. He had it.

We decided to run the money through the Russell Sage Foundation. My old friend Eric Wanner was willing to do it. He had a big budget so that I could really work on this fulltime. I was seeing the end of the road with respect to the odyssey book, so this was something possible. My calendar was free. So I began what turned out to be three very hard years of independent research on my part, commissioning research assistants to go out and master certain points of the literature that I knew were very important. For example, decision making under conditions of stress and uncertainty. A vast literature in psychology and the business school literature and disaster literature that I knew about, but didn’t know. I would commission my research assistants selectively to go into different—

42-00:46:41

Rubens:

Where were you getting them, literally?

42-00:46:43

Smelser:

Berkeley. I decided not to hire anybody in sociology, because that was my field. I got two people from the business school. I got a couple of people from psychology. I got a person, who later became a graduate student in ethnic studies, that I’d used as a research assistant in my California book. I’d choose these people selectively. Paid them through the university as research assistants on a university pay scale. Got the money from Russell Sage Foundation, paid it out, and so on. In the meantime, I was doing an unbelievable amount of independent research on my own. The fact that the encyclopedia was in my history was very important. I had just done so much coverage of the whole social sciences that I was at a tremendous advantage. It was a comprehensive subject.

Reed and I would collaborate. I did all the writing. He’s not a writer, he’s not a scholar. His whole life has not been in this field. However, we would discuss every issue before I would take it up. I’d get all his input. I would do

the research. I'd show him outlines, I'd show him drafts. We'd go visit each other a couple or three times a year, either in Massachusetts or he'd come out here. He was extremely actively engaged. Gave me all kinds of feedback into the material.

42-00:48:10

Rubens: Was he an intellectual?

42-00:48:12

Smelser: Well, in fields he knew something about. He couldn't get into some aspects of—

42-00:48:19

Rubens: But it was a good working relationship?

42-00:48:21

Smelser: We had a very good mutual respect. There was no question that I was going to be an author, because I was the one who was doing all the work, but the question of his authorship was in question. What would he be listed as? Co-author? Associate? There are all kinds of possibilities. This is always a ticklish issue in authorship. I sort of took my lead from Paul Baltes, my co-editor in the encyclopedia. One day, in the middle of the thing, he said, "Neil, I want your name to be first." Because, normally, "B" comes before "S." That's the convention in co-authorship. "I want your name to be first," he said, "This is your child more than my child." So he took this lead. I told John one day, I said, "John, this may be or may not be a ticklish subject in your mind, but I think you should be listed as a co-author, second author. So it will be Smelser and Reed." He was totally floored and delighted to in that level. I don't feel I lost anything by doing that, even though he wasn't an author in the usual sense of the term. It's going to be listed that way. He's continued to engage himself. I show him everything I'm doing. I'm now in negotiation with the UC Press. I've sent them photographs. It's in press. Usual, long birth process.

42-00:50:21

Rubens: Was there ever a consideration of another press?

42-00:50:25

Smelser: I dickered with Oxford Press, with Cambridge Press, with UC Press. I wanted to have a series of alternatives. John thought all three would be good, though he left it to me. What happened is Oxford Press has gone into some crazy direction. The guy said, "This has to be 200 pages long." They wanted it to be much more practical and formulaic and so on. I just wrote him back and said, "This is not our book. Very sorry." Oxford has gone much more kind of commercial, and they do some management books. They're getting into that arena that this book is not. Then Cambridge Press expressed an interest in it, but they dawdled. Almost irresponsibly dawdled. Send me a list of people you want to read this for reevaluation and so on. They just kept quiet and quiet. In the meantime, the University of California Press was going ahead with it. So I

wrote John, said, “Look, we’ve got a situation here. It may not be your first choice, but I’d like to do it. I’d like to go ahead and get this book published.” “Fine,” he said, “That’s great.” Publishing in the world of university presses nowadays is extremely difficult. They’re all under the gun of budgetary stringency. They were very worried about length. I had to dicker and fight about length. Fortunately, the reviews were so strong that, in a way, I had some capital. If it was really strong reviews like this, they liked the book.

42-00:52:08

Rubens:

So they take the ones you pick? Do they find their own?

42-00:51:11

Smelser:

Well, no. They say, give me a list. I gave them a list, and they chose the two I said would be best. It’s a little bit corrupt, but these are eminent scholars who would take the book seriously. There were two extremely positive reviews. That always strengthens the hand of an author when you get these strong reviews. I volunteered to do a certain amount of cutting. A modest amount of cutting. I didn’t want to decimate the book—

42-00:52:43

Rubens:

I’m assuming the reviews that came in didn’t ask for much revision.

42-00:52:48

Smelser:

No. No, they both were absolute raves. They found errors, which are always in a manuscript. They liked some parts of it better than others, but they both said, publish as is. That was basically their bottom line. The UC Press has the editorial board. They insisted it be approved by that editorial board before any book can be published. Oftentimes, that editorial board takes an independent line, and if they don’t approve it, it’s not in. That’s not typical. Most editorial boards make sure the procedure was right or something, but they don’t substantively get the book. I had a very funny episode you’ll have to know about. One of their committee is always, you read this one, you read that one. Then they bring it to the committee, and then they discuss it. They take it very seriously. They write very serious reviews. I once was asked by Jim Clark to be on that editorial committee, and I said I wouldn’t do it. It’s too much work. I knew what the culture was. Anyway, the reviewer loved the book.

42-00:53:57

Rubens:

Who was this?

42-00:53:58

Smelser:

It was a man named—was it Howard or something? He was an historian, I believe. He thought it was absolutely great. He said this book could be published as is. However, I had one quote in there about the British and their colonial policy, and in that quote was a statement from another author, not mine, that the problem was mixing ethnic groups in different colonies, and you get strange results, like Iraq. No basis for nationhood at all. They’re just so twisted. So I was making a point about, this is one of the origins of terrorism. It was talking about terrorism and economic development

difficulties. I made this point about the limits of nationhood in the former colonies, because they were ethnically so diverse, or religiously so diverse. In the quote, this guy said, it all started with Northern Ireland. That the British got this religious diversification in Northern Ireland. That was it. It was in a quote. I didn't make the point. This guy is an ardent Irish nationalist. Complete. He said, "Only one change I want to request in this book." He said, "It's historically wrong." He blasted the author that I quoted from, saying he was not a scholar. He went on and on.

I never knew the guy from before, but he was evidently an avid Irishman. He not only said this has to leave, has to be out of there, or has to be corrected historically, he said this is a flaw of the book. He went on, and he sent me many references. He was going crazy on this one issue, British-Irish relations. You are supposed to give a little response to these reviewers. So I wrote and said, "That point is completely incidental to my analysis. I'm just going to take the quote out and make my point anyway, which is completely valid." That's how I handled it. It's kind of typical. People get bugs in their own minds. This was, fortunately, easily handled. It wasn't central to my concern. We're hoping that this book has impact. We're very hostile to hyper-rational formulae of applying social sciences. Rational choice gets badly treated in our book. These managerial handbooks get very badly treated. They're all gimmicky. We are very impatient with that. At the same time, we also try to avoid a completely eclectic sort of idea that there's no structure. There are a lot of social scientists who sort of say applied knowledge isn't possible, and go on to give reasons why. We tried to take a middle ground in this. Now, whether that will enhance or detract from the general attention the book gets, we think it's realistic, we think it's very sensible. We try to assemble enormous amounts of substantive knowledge that play into decision making, that play into the understanding of organization, that play into policy making, that play into the evaluation of the effectiveness of policies. Just a whole variety of subjects.

42-00:57:47

Rubens:

When is it going to be out?

42-00:57:49

Smelser:

I don't have a deadline for it. UC Press is not exactly the most rapid organization in the world. I have insisted that my niece, Claudia, take charge of the cover. She'll do that. They're happy with that. I'm hoping that it will come out in the spring of 2012. My realistic expectation is it will be on their fall list of 2012.

42-00:59:00

Rubens:

Well, I think we're through for now.

42-00:59:01

Smelser:

Ok. We'll tie things up, if we wish, after you get the editorial work.

42-00:59:05

Rubens: I always have some left-overs. The upshot of the Kluge Fellowship at the Library of Congress was that you were able to get far ahead with the writing.

42-00:59:15

Smelser: That was pretty essential for my odyssey work.

42-00:59:24

Rubens: Did you move there?

42-00:59:25

Smelser: Yes, we lived in University of California housing. I swung a deal to live there on Sixteenth and Rhode Island. They had a provision that people could live in it if it wouldn't be taken up by faculty, and they had one room on the eleventh floor. Sharin and I got that room. We stayed there for three months. Beautiful spot. Not far from Dupont Circle. Just a metro ride to the Library of Congress.

Oh, I tell you something very funny. It's actually in the odyssey book. We went to Washington. We thought it was going to be like going to West Virginia. We took a camper. We wanted to go to Maryland beaches. We wanted to go to Virginia campgrounds. We wanted to spend our weekends traveling around the area. We got so involved in Washington cultural life that it was a mistake to take the camper. We went a couple of times to Maryland. We really enjoyed the time there. I wrote it up as one of the episodes in the odyssey book.

42-01:00:43

Rubens: When you were in Virginia, did you get up to Washington much?

42-01:00:46

Smelser: Not really. We traveled in West Virginia. We traveled to Kentucky and Tennessee, eastern Iowa. We went up to upstate New York. We went to the fall colors in New England.

42-01:00:59

Rubens: Washington is really quite—

42-01:00:01

Smelser: Oh, it's wonderful. We're going to be going there frequently, with our son there. Every time we go to Philadelphia, we're going to add a few days on to go to see him in Washington, D.C.

42-01:01:12

Rubens: Now your term at The Guggenheim comes to an end— .

42-01:01:19

Smelser: This February. Sixteen years. One of my students is taking over my place. Not as chair, but as social science member. That's Mary Waters from Harvard. That's very nice.

42-01:01:32

Rubens: You were interviewed by Harry Kreisler, in his Conversations With History series.

42-01:01:44

Smelser: Yes, we worked together in the Institute of International Studies for years. He was an assistant director when I was there as associate director. I admired him tremendously for taking on this Conversations with History project, which has turned into a very big thing. He's very skilled.

42-01:02:04

Rubens: He's produced a really an interesting book.

42-01:02:06

Smelser: He's very skilled. The two interviews I had with him were very gratifying, and they got some attention.

42-01:02:14

Rubens: I learned a lot about you from them. They were a useable, concise history.

[End of Interview]

AFTERWORD: Written Reflections, Neil J. Smelser, September 2012

In these concluding remarks I develop a few topics that were not either not referred to at all or not referred to amply in the oral history interviews; add to the the remarks on the Clark Kerr lectures, which had not been yet given at the time of the final interview; indicate some possible directions of my research in the years to come; and close with a few comparisons between oral communication and written prose.

Interests and Foibles

At one point the interviewers asked me about personal idiosyncracies, habits, and hobbies, and I discussed my amateur habit of doodling on Styrofoam coffee-cups and my practice of collecting coffee-mugs from campuses around the country (and Europe) at which I have had occasion to lecture at or visit. I would mention a few other items that occur randomly to me.

- I have a near-addictive relationship to jigsaw puzzles and crossword puzzles. The first stems from a very early age—as early as two, according to the accounts of my parents, at which time I developed a fascination with jigsaw puzzles, a precocious skill in working them, and a reaction of fury toward any other child who would mess up my puzzles-in-process. The passion lasted all through my years of schooling, and I have up to this day thoroughly enjoyed working on them. I most enjoy nature scenes and fine painting as puzzle themes. My finest accomplishment, several years ago, was assembling a 9,000-piece puzzle of Breugel’s “Tower of Babel.” I inherited the interest and skills in crossword puzzles from my mother, who was something of an addict, and I complete at least one puzzle daily, before retiring. Both puzzling habits are consistent with my general scholarly and personality trait of relishing the creation of order out of chaos.
- I have cultivated a few specialty food preparations and worked them into a kind of fine art. These honed skills contrast with my general incompetence in culinary matters. First, I have a special salad dressing with an oil-and-vinegar base (also basil, salt, dry mustard, and pepper), which I picked up in my years of having meals at Harvard’s Society of Fellows (1955-58). It is a kind of personal trademark for me, and I share the exact proportions only with close friends and people who beg for them. Second, I make a special apple sauce from home-grown apples, using maple syrup (rather than sugar) and cinnamon as ingredients; I make a year’s supply each year at apple harvest time from trees at the edge of our property, and we often bring a jar as a house gift when we are asked to social occasions. Third, I regard myself as an expert in preparing mashed potatoes, and am the dedicated maker on holiday occasions with the family. I boast shamelessly about all three of these skills and receive my share of compliments.
- I have had a lifetime love of golfing (now discontinued because of arthritic knees). This was derived from my youth, when my father and brothers would golf together, and my brothers and I would go out on our own to the sun-baked public courses in Phoenix. My golfing skills were better-than-average but not superior. I follow professional golfing on television, along with football, basketball, and baseball. Hiking and camping have been my other lifetime avocations.

We still own a small Volkswagen camper and go on several adventures in the Western states each year.

Delivering the Clark Kerr Lectures

I covered my selection, preparation, and some content of the Clark Kerr lectures (January-February, 2012) in the oral interviews. I should say a few words about their delivery on the dates of January 21, January 28, and February 7 (on the Berkeley campus) and on the Riverside campus on February 14. After all my years of lecturing I discovered to my surprise that I was nervous, almost as I was in my early lecturing career, about delivering them, especially the first one. I regard this nervousness as an indication of how important it was to me that I was chosen to be the lecturer. No one from the selection committee even indicated to me why I was selected, but I think it was mainly because of the favorable reception that my book *Reflections on the University of California* (2010) gained after its publication. In the end, delivering the lectures was an exceptionally gratifying experience, with large turnouts for all, and with engaged and intelligent questioning following the delivery of each. Corrected for the inevitable amount of flattery and lying on the part of friends, the responses to the lectures were generally positive and encouraging. The book based on the topic of the lectures—*Higher Education: The Play of Continuity and Crisis*—is now in press with the University of California Press (I prepared them in final, publishable form in advance of their oral delivery). The entire experience was as gratifying as anyone could wish. The lectures were posted on u-tube immediately after their delivery and I watched them and thankfully found few signs of a feared decrepitude or lapses in concentration.

I report one light incident. For the third lecture I dressed in formal attire, because that lecture was followed immediately by a black tie dinner of the Berkeley Fellows (a UC Berkeley institution made of up administrators, faculty and generous friends of the campus). The main activity for the Fellows is a dinner each February at the Chancellor's house. I explained the reason for the attire to the audience in a humorous manner. Later in the lecture period, when I was pacing back and forth during the question period, the cummerbund accidentally became unhooked and came off in my hand. After a moment I recovered, held the cummerbund in the air, announcing, "My cummerbund has come off," and then a moment later, "This is not what it might seem!" That suggestion of a strip-tease on the part of an 81-year-old scholarly lecturer drew more laughs than any of the items of academic humor that I had introduced to lighten the lectures.

Possible Future Research Projects

I should say a few words about intellectual prospects that are on my horizon for the next few years. The first, and by now ongoing, line is to extend some themes in the Clark Kerr lectures that merit further development. I am carrying this extension out under the heading of "academic cultures". The aim of the research is to depict the emergence of subcultures that have accumulated in the course of the vast complication of the purposes (missions) of colleges and universities. One purpose of this research would be to identify and document the evolution of cultures—those of undergraduates, graduate students, professional school students, faculties and disciplines, administrators and managers, and various kinds of staff. New varieties of political culture (gender and race-ethnic politics, gay politics, environmental politics, political

correctness, and reactions against all of these) have also entered the scene. Evolution of all these types have taken place in and co-exist within discrete, multiple-function organizations of universities and colleges. This makes them subcultures within discrete organizations and intensifies conflicts among them. This circumstance makes for several peculiarities and several corresponding integrative problems for academic cultures: managing conflicts among them through rationalization, ritualization, and coordination; papering over of conflicts; insisting on unity despite diversity; or, alternatively, pressing the primacy of one or another cultural variant. Such peculiarities impart a peculiar character to cultural change, cultural diversity, and cultural conflict in academia. The aim of my studies is to sort out and give an intelligible rendition and explanation of these dynamics.

A second line of research would be to resuscitate a major project—the history of sociology in the twentieth century. This would be a combination of intellectual history and an attempt to tease out the personal, intellectual, organizational and societal forces producing both the content and changes of direction in the field. Though its exact place in the analysis is as yet uncertain, I also intend to include a biographical element in my interpretations, since as a professional sociologist I have lived through the second half of that century and gained both knowledge and perspective about the field in that personal history. I have already, at different time periods, conducted research on the emphases of journals, on themes of presidential addresses, and on the ebbs and flows of different research perspectives. To extend this research to a point of completion would be a major enterprise, as would be writing it up into an informed and intelligible volume.

A third line would be to write up a history of the Berkeley sociology department, 1952 to the present. This has not been properly done. I have had the advantage of being a member of that department from 1958, with, however, a period of absence from 1994 to 2001, and maintaining the distance of an emeritus since retirement in 2001. No other faculty member has enjoyed that length of tenure. I would combine research into the departmental archives with my own knowledge of persons and situations that make up its history. The book would be a mix of institutional history, the parades of persons and perspectives in the department, its political history, and my personal assessment of and reflections on all of these.

I should add that not all of these projects will likely be pursued because the time remaining for my scholarly activity will no doubt not permit it. I may discard one or more of these enterprises if my interest flags or if new ideas occur to me and new opportunities arise. I do know that I intend to be intellectually active as long as I can and as long as my work appears to be meaningful to others.

On Oral Presentation (Including Oral History) and Written Exposition

After transcribing the nearly two-dozen sessions that constituted this oral history, the interviewers presented me with a written transcription of them, and asked me to edit them as I saw fit and, ultimately, to approve them for the limited form of publication that is scheduled. I went over the whole text, identifying and correcting errors of transcription, improving meaning, correcting spelling and grammar, and eliminating occasional awkward expressions and bêtises. By and large, however, I let the spoken prose stand, encouraged in doing so by the interviewers-editors and by my own conviction that the tone and quality of the interviews was improved by

leaving them, in large part, as the product of what came out in oral responses to questions and in oral explorations. In following this strategy I came to a few reflections on the differences between written and oral communication, and I include them here.

In my whole career of scholarly writing (and to a lesser degree in correspondence with others), I have noticed how contrived much of it is. Much of this stems from the fact that in writing for publication or other public dissemination, one always writes with a diversity of imagined audiences (including reviewers, scholarly colleagues, students, and the “general reader”) in mind, and produces text that reflects the diverse—also imagined—perceptions of reactions on the part of all these audiences. Unlike most verbal expression, writing also affords the opportunity to go back to the text, often many times, and refine meaning, correct real and imagined errors, clarify, and clean the text. All this monitoring and correcting creates a more polished form of communication, which, however, is more contrived and less spontaneous.

Oral communication, in which one is typically responding to conversational norms that are activated when one speaks to another, typically does not permit all these refinements. One is called upon by norms of communication and civility to produce a certain tempo in speaking. One starts a sentence, then reconsiders and starts over; one is typically speaking to one, not many audiences; and self-corrections must come in the form, not of erasures and deletions, but of starting over again, perhaps with an entirely new sentence or direction of thought. When this conversational flow is recorded and read by the utterer, moreover, that utterer is often chagrined by the apparent jerkiness, lack of organization, repetition, grammatical inelegance, and messiness of the text. It is said of some speakers that they think in whole paragraphs (one newspaper reporter once asked me if that was true of UC President David Gardner, and I said, “probably so”), but that has to be a rarity. At the same time, oral presentation yields a certain spontaneity that is often more creative and revealing than the multi-corrected textual presentation available through writing. It shows the mind at work from moment to moment, whereas written presentation yields of more considered and organized and no doubt “safer” flow of meaning. [At one time in my career I attempted an intermediate strategy, which I mentioned in one of the interviews. That was in preparing my text, *Sociology*, published first by Prentice-Hall in 1981. For reasons that have now escaped me (I think that speed and efficiency of composition were uppermost in my mind), I decided initially to dictate the text of every chapter into a recording machine, have it transcribed, and then edit it into final form. (I call this “intermediate” because it was oral expression meant ultimately to be written.) It was extremely difficult to hold the requisite amount of information in mind as I dictated; many incomplete sentences and thoughts appeared in the transcribed dictation; and the time involved in subsequent editing nearly cancelled out the economies gained in speed of oral composition. Furthermore, the dictation process was psychologically, even physically exhausting, because it was such a labor to hold that much complex material in mind, much less speak to the multiple requisite audiences in the conversational flow. A proper textbook emerged in the end, but I resolved never to attack a writing project of that complexity by speaking it out first.] All the tensions between more spontaneous oral and more disciplined written communication appeared in this oral history. Reviewing the text generated all the reactions that come in reading one’s own spoken words. Nevertheless, I have mainly resisted turning the results of the interviews into polished written prose. I believe that the recollections, thoughts, and analyses presented in these interviews probably have more value in revealing my work, life, and style than written memoirs would do. These thoughts lay behind my minimal editing of the oral transcripts, and my decision to live

with the less disciplined but franker communication than heavy editing and rewriting of my oral reflections would reveal.

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CURRICULUM VITAE

- Name:** Neil Joseph Smelser
- Birth:** July 22, 1930, in Kahoka, Missouri
- Home address:** 109 Hillcrest Road, Berkeley, CA 94705
- Professional address:** Department of Sociology
410 Barrows Hall
University of California
Berkeley, CA 94720
- Marital status:** Married, 1954, to Helen Margolis (divorced 1965);
married to Sharin Hubbert, 1967.
- Family status:** Four children:
Eric Jonathan Smelser, born 1957; Tina Rachel Smelser, born 1959;
Joseph Neil Smelser, born 1968; Sarah Joanne Smelser, born 1971.
- Education:**
- 1936-48: Primary and secondary public schools
in Phoenix, Arizona.
- 1948-52: Harvard College. B.A. in Social Relations.
- 1952-54: Magdalen College, Oxford University. Final Honours School
of Philosophy, Politics, and Economics. B.A. 1954; M.A. 1959.
- 1954-58: Harvard University. Ph.D., 1958.
- 1963-71: San Francisco Psychoanalytic Institute, Research Training
Candidate. Graduate psychoanalyst, 1971.
- Teaching positions:**
- 1958-60: Assistant Professor of Sociology,
University of California, Berkeley.
- 1960-62: Associate Professor of Sociology,
University of California, Berkeley.
- 1962-72: Professor of Sociology, University of California, Berkeley.
- 1972-94 University Professor of Sociology, University of California.
Emeritus, 1994-Present.
- Research appointments:** 1955-58: Junior Fellow of the Society of Fellows,

Harvard University.

- 1959-61: Center for the Integration of Theory in the Social Sciences, University of California, Berkeley.
- 1961-63: Faculty Research Fellow, Social Science Research Council.
- 1962: Auxiliary Research Award, Social Science Research Council.
- 1965-66: Research grant, National Science Foundation.
- 1970-71: Research grant, Ford Foundation and American Academy of Arts and Sciences.
- 1973-74: Guggenheim Fellowship.
- 1975-76: Research grant, Russell Sage Foundation.
- 1976-77: Research grant, Ford Foundation.
- 1978-79: Research grant, Russell Sage Foundation.
- 1980-82: Research grant, National Institute of Education.
- 1989-90: Visiting Fellowship, Russell Sage Foundation.
- 1992 Residential Scholar, Study and Conference Center, Bellagio.
- 2006 Kluge Fellow, Library of Congress

Administrative and related positions:

Assistant to the Chancellor for Student Political Activity, University of California, Berkeley, 1965.

Vice-Chair, Department of Sociology, University of California, Berkeley, 1965.

Assistant Chancellor for Educational Development, University of California, Berkeley, 1966-68.

Chair, Policy Committee of the Berkeley Division of the Academic Senate, 1971-72.

Member, Executive Board, Berkeley Faculty Association, 1972-73, 1974-76, 1979-83; 1991-92;
Chair of the Board, 1975-76; 1980-81.

Associate Director, Institute of International Studies, University of California, Berkeley,
1969-70, 1972-73, 1981-89.

Chair, Department of Sociology, University of California, Berkeley, 1974-76, 1991-92.

Staff Member, Department of Psychiatry, Cowell Hospital, University of California, Berkeley, 1966-76, 1981-82.

Vice-Chair of the Governing Board, Joint Medical Program, Berkeley and San Francisco Campuses of the University of California, 1977-78.

Director, Education Abroad Program of the University of California, United Kingdom/Ireland, 1977-79.

Chair, Educational Policy Committee, University of California, Berkeley, and Member, University-wide Educational Policy Committee, 1979-80.

Member of Governing Board, Joint Health and Medical Sciences Program, Berkeley and San Francisco Campuses of the University of California, 1979-82.

Chair, Commission on the School of Education at the University of California, Berkeley, 1981.

Chair, Berkeley Division of the Academic Senate, 1982-84.

Chair, Task Force on Lower Division Education in the University of California, 1985-86.

Vice-Chair and Chair, Academic Council, Assembly of the Academic Senate, University of California, 1985-87.

Faculty Representative to the Board of Regents, University of California, 1985-87.

Clinical Supervisor, Psychology Clinic, University of California, Berkeley, 1984-93.

Member, Committee on Committees of the Berkeley Division of the Academic Senate, 1988-89.

Acting Director, Center for the Study of Higher Education, University of California, Berkeley, 1987-89.

Chair, Blue Ribbon Commission on Intercollegiate Athletics, University of California, Berkeley, 1991-92.

Special Advisor on Long-term Planning, Office of the President, University of California, 1993-94.

Director, Center for Advanced Study in the Behavioral Sciences, 1994-2001.

CoChair, Commission on General Education in the 21st Century, University of California, 2003-2007.

Professional activities and memberships:

Member, Committee on Economic Growth, Social Science Research Council, 1961-65.

Member, Comparative Development Group, Institute of Industrial Relations, University of California, Berkeley, 1961-65.

Advisor Editor, American Journal of Sociology, 1961-62.

Editor, American Sociological Review, 1962-65.

Consulting Editor, Philosophy and Public Affairs, 1972-75.

Consulting Editor, Comparative Studies in Society and History, 1973-81.

Member, Council of the American Sociological Association, 1962-65, 1968-70, 1973-75, 1996-98.

Member, Executive Committee of the American Sociological Association, 1962-65.

Member, Publications Committee of the American Sociological Association, 1962-65.

Member, Pacific Sociological Association.

Member, American Association of Rhodes Scholars.

Member, International Sociological Association.

Chair, Group on Theory and Method of Comparative Studies, Institute of International Studies, University of California, 1966-94.

Member, Board of Directors, Social Science Research Council, 1969-73; Chair of the Board, 1971-73.

Chair, Sociology Panel of the Behavioral and Social Sciences Survey, sponsored by the Social Science Research Council and the National Academy of Science, 1967-69.

Member, Committee on Problems and Policy, Social Science Research Council, 1975-77.

Member, Steering Committee of the Undergraduate Curriculum Development Group of the American Political Science Association, 1975-77.

Co-Chair, Nominations Committee, Social Science Research Council, 1979-80.

Member, Executive Board, Western Center of the American Academy of Arts and Sciences, 1976-77, 1979-84. Co-chair, 1976-77, 1979-84.

Member, Council of the American Academy of Arts and Sciences, 1981-94.

Member, Special Projects Committee, Center for Advanced Study in the Behavioral Sciences, 1979-92.

Co-Chair, Research Committee on Economy and Society, International Sociological Association, 1980-86.

Member, Board of Trustees, Center for Advanced Study in the Behavioral Sciences, 1980-86, 1987-93, Chair 1984-86, 1993-2008.

Member, Committee on Basic Research in the Behavioral and Social Sciences, National Research Council, 1980-88; Chair, 1982-84; Co-chair, 1984-88.

- Member, Committee on Nominations, American Academy of Arts and Sciences, 1981-84.
- Member, Board of Trustees, Head Royce School, Oakland, California, 1980-86.
- Chair, External Advisory Committee on Sociology, Harvard University, 1981-87.
- Member, Visiting Committee for Sociology, Harvard University, 1988-90.
- Member, Subcommittee on Humanism, American Board of Internal Medicine, 1981-84, 1989-90.
- Chair, Search Committee for the President of the Social Science Research Council, 1985-86.
- Member, San Francisco Psychoanalytic Institute, 1971-Present.
- Member, Executive Committee, International Sociological Association, 1986-90, 1990-94.
- Member, Program Committee, International Sociological Association, 1986-90, 1990-94.
- Member, President's Committee to Review the Social Sciences (Behavioral) at Yale University, 1988-93.
- Member, Advisory Committee on Sociology, Yale University, 1993-98.
- Member, President's Advisory Committee on Undergraduate Education, University of California, 1987-89.
- Member, Scientific and Academic Advisory Committee to the President of the University of California on the Energy Laboratories (Livermore and Los Alamos), 1988-92.
- Member, President's Advisory Council (National Laboratories), University of California, 1992-98.
- Member, Board of Trustees, Russell Sage Foundation, 1990-2000.
- Member, Advisory Committee to the Board, American Board of Internal Medicine, 1992-99.
Chair, 1995-99.
- Member, Kuratorium, German-American Academic Council, 1994-98.
- Member, Executive Committee, International Social Science Council, 1994-95.
- Member, Committee on Techniques for the Enhancement of Human Performance (National Academy of Sciences), 1994-95.
- Member, Committee on International Security and Arms Control (National Academy of Sciences), 1995.
- Member, Commission on Social and Behavioral Sciences and Education (National Research Council/
National Research Council), 1995-Present. Chair, 1996-2001.
- Member, Committee on Executive Office and Budget, American Sociological Association, 1996-98.

Member, Committee of Selection, John Simon Guggenheim Foundation, 1995-Present.
Chair, 1996-Present.

Chair, Visiting Committee on the Institute for Social Research, University of Michigan, 1996.

Member, Task Force on Examination Standards, American Board of Internal Medicine, 1995-96.

Member, Strategic Planning Oversight Committee, American Board of Internal Medicine, 1996-98.

Member, German-American Academic Council, 1999-2001.

Member, Committee on Science and Technology to Counter Terrorism, National Research Council, 2001-2002.

Chair, Panel on Behavioral, Social, and Institutional Issues (Committee on Science and Technology for Countering Terrorism), National Research Council, 2001-2002.

Chair, Panel on Understanding Terrorists to Deter Terrorism. National Research Council, 2001-2002i.

Chair, Division of Behavioral and Social Sciences and Education (National Research Council), 2001-2003.

Member, Committee to Prepare A Physician Charter, a committee representing the American Board of Internal Medicine, the ACP-ASIM Foundation, and the European Federation of Internal Medicine, 2000-01.

Member, Committee on Managing Physicians' Conflict of Interest, American Board of Internal Medicine, 2003-04.

Consultative activities:

Series Editor in Sociology for Prentice-Hall Publishers, Englewood Cliffs, N.J., 1961-79.

Consultant, National Advisory Commission on Civil Disorders, 1968.

Member, Task Group on Research and Development in Education, President's Science Advisory Committee, 1968.

Consultant and Senior Advisor, Presidential Advisory Commission on the Causes and Prevention of Violence, 1969.

Member, Technical Advisory Committee, Carnegie Commission on Higher Education, 1968-73.

Member, Advisory Committee, Carnegie Council on Higher Education Project on the Undergraduate Curriculum, 1976-77, 1979-80.

Consultant, California Task Force to Promote Self-Esteem and Personal and Social Responsibility, 1987-89.

Consultant, Nobel Prize Committee on Economics, 1989.

Academic and related awards:

- 1952: Graduated summa cum laude, Harvard College
- 1952-54: Awarded a Rhodes Scholarship from Arizona for two years' study at Oxford University
- 1954: Gained First Class Honours in the Final Honours School of Philosophy, Politics, and Economics, Oxford University
- 1955-58 Elected Junior Fellow of the Society of Fellows, Harvard University
- 1968 Elected Fellow, American Academy of Arts and Sciences
- 1971 Named University Professor of Sociology in the University of California
- 1973 Elected Vice-President, American Sociological Association
- 1974 Elected Member, Sociological Research Association
- 1976 Elected Member, American Philosophical Society
- 1982 Elected Chair, Theory Section of the American Sociological Association
- 1987 Named Chancellor's Fellow, University of California, Berkeley
- 1889-90 Visiting Scholar, Russell Sage Foundation
- 1990 Elected Vice-President, International Sociological Association
- 1992 Resident, Bellagio Study and Conference Center, Rockefeller Foundation
- 1992 Elected Member, The Tocqueville Society
- 1993 Elected member, National Academy of Sciences
- 1994 Awarded the Berkeley Citation by the University of California, Berkeley
- 1995 Elected President, American Sociological Association, for 1997
- 1995 Georg Simmel Guest Professorship, Humboldt University, Berlin.
- 2000 Elected to Society of Fellows, American Academy of Political and Social Sciences
- 2002 Recipient, Mattei Dogan Foundation Prize for Distinguished Career Achievement, International Sociological Association
- 2006 Named Kluge Chair, Library of Congress
- 2006 Named Foreign Member, Russian Academy of Sciences

2011 Appointed Clark Kerr Lecturer, University of California, Berkeley for
January-February, 2012