Oral History Center
The Bancroft Library

University of California
Berkeley, California

Lester Telser: Beyond Conventions in Economics

Economist Life Stories

Interviews conducted by
Paul Burnett
in 2017

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

****

It is recommended that this oral history be cited as follows:

Lester Telser, "Lester Telser: Beyond Conventions in Economics" conducted by Paul Burnett in 2017, Oral History Center, The Bancroft Library, University of California, Berkeley, 2018.

Economist Life Stories

The impact of economics in our society is hard to overstate. Economics structures government policy, guides decision-making in firms both small and large, and indirectly shapes the larger political discourses in our society.

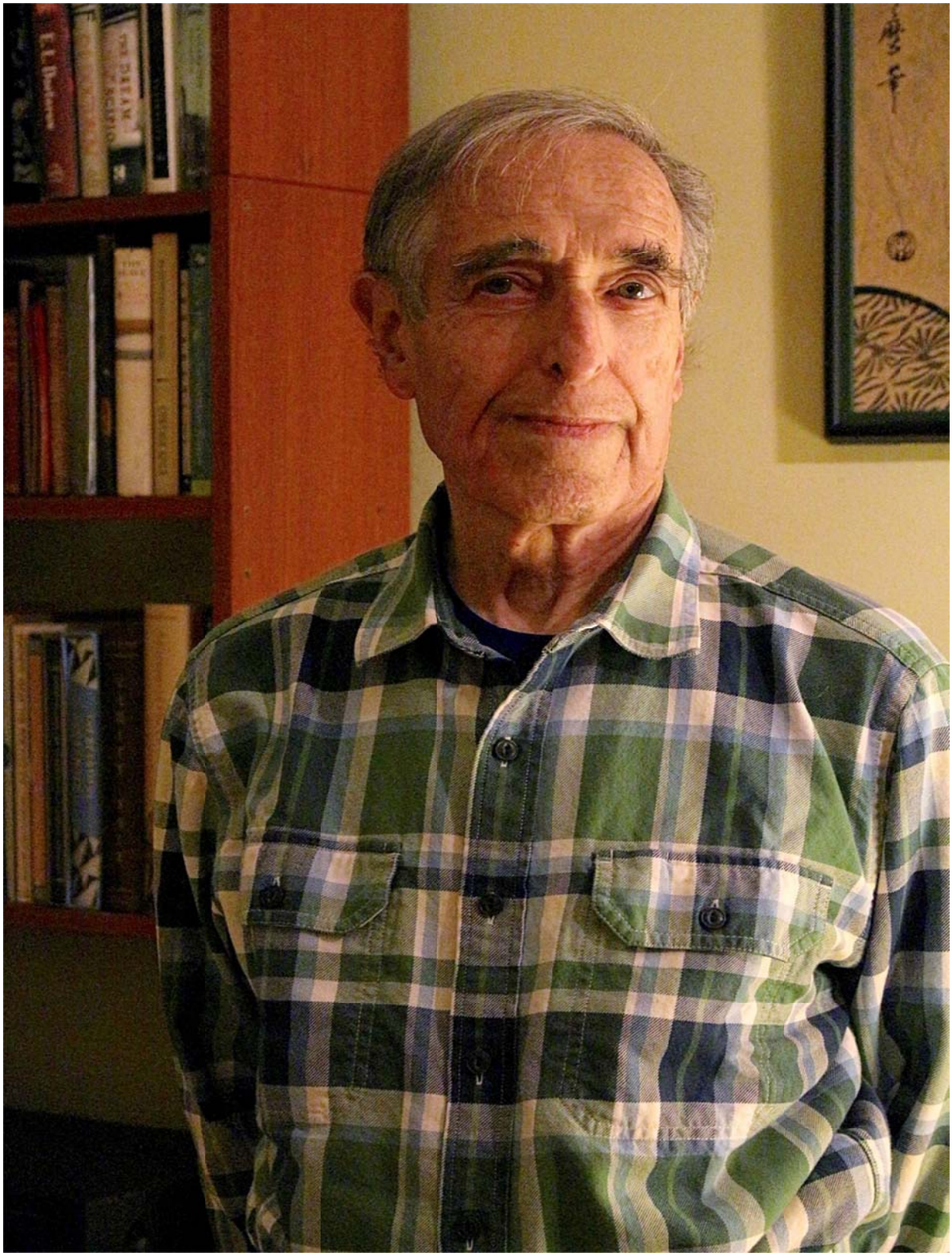
To enrich the understanding of the influence and sources of powerful economic ideas, the Becker Friedman Institute for Research in Economics at the University of Chicago set out in 2015 to capture oral histories of selected economists associated with Chicago economics. The aim was to preserve the experiences, views, and voices of influential economists and to document the historical origins of important economic ideas for the benefit of researchers, educators, and the broader public. This oral history with Lester Telser, conducted in ten sessions in Chicago, IL, from July to October 2017, is the third interview for the project.

Economist Life Stories is more than a collection of life histories; it chronicles the history of a scholarly community and institutions at the University of Chicago, such as the Graduate School of Business, the Cowles Commission, and the Department of Economics. It also reflects the achievements of faculty and students in the domains of economic policymaking and private enterprise around the world. Although this project focuses on the leaders and students of the University of Chicago Department of Economics, the Graduate School of Business, and the Law School, we hope to add more stories from economists around the world as the project expands.

Acknowledgments

Hodson Thornber and Paul Burnett organized the project with Toni Shears and Amy Boonstra of the Becker Friedman Institute, with important support from an advisory group of historians and economists.

Financial support for this work was provided by Hodson Thornber, a member of the Becker Friedman Institute Council, whose contribution is gratefully acknowledged.



Photograph by Leena Schwartz, 2016

Lester Telser is Professor of Economics Emeritus at the University of Chicago. A student at Chicago in the 1950s, Dr. Telser was first a professor in the Graduate School of Business until 1964. Dr. Telser's life work is the theory of the core, a variant of game theory that involves coalitions of agents as opposed to individuals working to maximize their advantage. He used sophisticated mathematics to study why and how certain forms of markets are organized without appeals to more established concepts in economics. As both a student and colleague at Chicago economics department, and as a fellow at both the Cowles Commission and the Cowles Foundation, Telser is a key witness to the transformation of the field of economics after World War II.

Table of Contents

Interview 1: July 10, 2017 1

Family background — Father from border town (Russo-German), late 19th century — Father's prosperity, education, cultivation, facility with languages — Uncles' migration to US prior to World War I — Father in Russian Army during war, POW of Austrians — Father's rescue by sister Esther — Cousin Jacob studies engineering at the *Technische Hochschule* in Germany when Albert Einstein was on faculty — Grandfather's failed "hedge" on World War I — Grandfather's artistic side — Family connection in small village between Telser and economist Bob Aumann — Uncle, Isaak Kubovitsky, brother Leon a spy in underground during World War II and facilitator of Adolf Eichmann capture in Argentina — Grandparents and father move to Chicago in 1924 — Father and mother married 1929 — Mother's family from Poland, fleeing Bolshevik rule, arrival in Chicago, 1924 — Secular Judaism and Zionism — Telser's second cousin, J.D. Salinger — Surviving scarlet fever — Volatile fortunes of friends and family from 1920s to Great Depression — Depression's effects on Telser family —Telser's friends' linkages to the University and to famous historical figures — Writing for high school newspaper — importance of good teachers — Notable graduates of Hyde Park High School — Childhood interests — Taking in refugees from Germany and Belgium during World War II — Losses to the war and impact of Holocaust on family — Unwitting discovery of Manhattan Project war work — The appeal of mathematics — First encounter with work of John von Neumann and his influence on Abraham Wald and Paul Halmos — Minimum-cost "Stigler" diet as minimax problem — More on Adrian Albert — Courses with Abba Lerner and Alvin Hansen — Hansen's question about the failure of a rise in prices with an increase in the money supply, relevance for 2017 — "Regulation" vs. appropriate rules — Frank Knight on Hayek and Austrian economics — Judgment vs. rules, example of Alan Greenspan and Black Monday

Interview 2: July 11, 2017 27

Guidance with college admissions from a teacher — Work with high school newspaper and friendship with Russell Bensley — Selective service draft — Roosevelt College, talent and diversity of faculty and students — Early exposure to the theory of games — Early criticism of von Neumann and national cultures of science — Abba Lerner's perspective on signal contribution of Keynes to economics — Consideration of mathematics and statistics as academic paths — Impact of Great Depression on career choice — Mentoring influence of cousin Lester Seligman and Abba Lerner — Graduate fellowship to Harvard University — Interest in Keynes stimulated by Depression — Interest in macroeconomics revived by Black Monday in 1987 — Chance encounter with Sumner Slichter — Life as Harvard student — Impression of Alvin Hansen — Unintended lesson of course with Edward Chamberlin — Instruction in economics at Harvard, Wassily Leontief, Gottfried von Haberler, and lack of fit — Leontief's input-output tables — Influence of Kenneth Arrow's *Social Choice and Individual Values* and John Chipman on switch to University of Chicago for graduate school — Work for George Tolley and T.W. Schultz project on inventories of agricultural commodities — Contrasting Harvard and Chicago cultures — Research assistantship at Cowles Commission with Hendrik Houthakker —Cowles Commission at Chicago — Relationship to Chicago workshops — Stimulation from visiting scholars — Colleagues at Cowles, many veterans — James Tobin's candidacy to head Cowles and Milton Friedman's opposition — Tjalling Koopmans and postwar mission at Cowles —

Houthakker and Telser research separate — Military interest in economic planning, linear programming, example of railroad research at Cowles — Milton Friedman's attitude to linear programming, "non-economics," as common view at Chicago economics department — Example in tension between Jacob Marschak and Milton Friedman over Telser's thesis proposal, specific vs. general analysis — earlier pursuits of Keynesian, econometric models of the economy at Cowles, e.g., Carl Christ — Political significance of *methodenstreit* at Chicago — Wisdom of Frank Knight with respect to models of markets — Relationship between models and real world — Economists as engineers — Gérard Debreu and pursuit of general equilibrium — Friedman-Stigler aversion to general equilibrium, externalities, group influence on decision-making — Presence of such questions in classical economics — Leo Hurwicz and reservations about markets as information processors — Influence of Milton Friedman on dissertation research and Telser's hostility to mathematical economics during the 1950s — John von Neumann at Cowles

Interview 3: July 12, 2017 54

The "mindboggling" nature of futures markets — Rational organization of risk with policy complications of price supports of commodities — Graphing over a century of cotton prices — US commodity-loan programs (Commodity Credit Corporation)— Difficulty of getting data and help from T.W. Schultz — Schultz's conditions for USDA on research independence — Working as cooperative agent for the USDA — Influence on views of government — Schultz, Allen Wallis, and economics in the real world — Friedman's interest in dissertation because of relation to demand for money — Hedgers and speculators in futures markets — Holbrook Working — What really happens in futures markets — Keynes-Hicks model of speculators selling price insurance in futures markets — Harold Demsetz and re-evaluation of economics research on speculation — Price stabilization of profitable speculation or hedging — William Baumol's error — Regulation of futures markets, especially in Europe, despite auto-equilibrating effects of futures trading — Stabilizing effect of price supports — Theodore Schultz and Henry Wallace — Wallace's assistance to Zvi Griliches in dissertation research — Schultz's question on organized markets and the Telser-Higinbotham answer —Normal backwardation — David Meiselman and dabbling in futures as an experiment — Apparent inefficiency of markets in speculation with D. Gale Johnson — Friedman's replacement of Marschak on thesis committee — Friedman's interest in rational expectations — Jimmy Savage's disavowal of conclusions of his collaboration with Friedman — "Safety First and Hedging," first article — Constrained maximum, problems of convexification, and of making economic predictions based on approximations — Continuous but undifferentiable curves — "The tool that you use should not distort the fundamental aspects of the problem that you're interested in" — The real significance of the appendix to the 1947 edition of *Theory of Games and Economic Behavior* — Burnett on Mirowski's thesis on the meaning of the appendix for the economics profession — Meeting Sylvia and marriage — Military service and moving to Monterey, CA — US Army psychological research — Experiences as an army specialist — Assistant professor at Iowa State College — Offer from UC Berkeley — Offer from U. Chicago — Poaching effort from Johns Hopkins in 1960, counteroffers from U. Chicago — Rebuilding the Graduate School of Business with multidisciplinary group, new computing initiatives

Interview 4: July 13, 2017 84

Work practice of an economist — First Remington Rand vacuum-tube digital computer, UNIVAC-1, in administration building, use for agricultural and monetary economics — "Food Prices and the Bureau of Labor Statistics" with William Kruskal — Problems of categorizing data and biases arising therefrom — Politics of data collection — Problems with Green Stamps — Market research panel data, problems of — Susceptibility to mathematical analysis, with Robert L. Graves — The problems of monopoly in the 1950s — George Stigler's critique of Chamberlin and Robinson on monopoly — Telser's contribution to debate: measurement of elasticity of demand — Tobacco market research: competition on advertising spending rather than price and oligopsony purchases from primary producers — Influence of Aaron Director — Research on resale price maintenance — Advertising as barrier to competition or to entry into the market — *United States v. The Times-Picayune Pub. Co.*, 1952 — Challenging assumptions about relationship between industry concentration and advertising-to-sales ratio — "Iterative Estimation" paper (*JASA*, 1964) and working with panel data — Advertising as avenue to entry, not barrier — Monopoly is endogenous — Monopoly as bugbear —Natural monopoly — Present-day issues in universities surrounding freedom of speech and relation to extracurricular activities of Chicago economists in the 1950s and 60s —Chicago economists' views of Friedrich Hayek as an economist — Importance of liberty, freedom of speech, and rules pertaining thereto — Refusal to join Mont Pelerin Society — Personal meaning of liberty — Effects of antitrust law on competition and proposed solutions — Encountering hostility to University of Chicago and departing from economic orthodoxy — Labor unions and criminal infiltration in the 1950s and 60s — Public-employee unions today — Importance of rules matched with good judgment — Positive correlation between wages and concentration ratio of industry — Labor unions as counter to monopsony power of employer in isolated markets (e.g., coal mining towns) — Frank Knight's history of economics course — Importance of ethics in economics and in education — Economists' views of R.M. Hutchins — Robert L. Graves — Work on functional analysis and integral equations: differential vs. difference equations — Role of T.W. Schultz in foundation of the National Science Foundation — Telser's service on NSF panel — Politics of government research funding decisions — Mathematical similarity between business or government planning and gun laying — Getting in trouble with the Russians for using Markov equations to help sell Cornflakes

Interview 5: July 14, 2017 112

Basil S. Yamey, and work together on margins in futures markets — Popular narrative that margin speculation led to Great Depression not true — Work with Harlow Higinbotham: price volatility as determinant of margin requirements — Visiting the Chicago Board of Trade on the day and the first trading day after John F. Kennedy was assassinated — Lessons: market "rationality" in response to available information — More on work with Robert Graves and quadratic vs. linear programming — comparative statics vs. dynamic models — Calculus of variations and Leonhard Euler — More on difference equations — Fellowship at Cowles Foundation in 1964-65 — Exposure to Herbert Scarf and Robert Aumann — Limitations of game theory for economics, and the emergence of the theory of the core — Lloyd Shapley's discovery of Olga Bondareva: cooperative game as linear programming problem — Martin Shubik connecting game theory to general equilibrium — Scarf incorporates results from Bondareva and Shubik into applied economic theory — Centrality of linear programming to

Telser's work on the core — Von Neumann and the "Assignment Problem" at Cowles Commission — Ancestor of matching software for medical school residencies — Comparison of theory of the core to Arrow-Debreu theorem — Telser's approach and the Chicago tradition — Francis Ysidro Edgeworth — The core in relation to how a market actually works — New economics made possible by increases in computing power — The empty core explained — *Treasure of the Sierra Madre* example — Misinterpretation of insolubility of some aspects of theory of the core — Example of empty core leading to natural monopoly — Thinking of core theory in terms of the evolution of markets — The importance of rules — Chicago law and economics tradition — Lauchlin Currie — Passion and reason in economics — Zvi Griliches — Collaboration and difficult people — Collusion and cooperation as terms — Year at Cowles Foundation at Yale — Working out the concept of strong convexity

Interview 6: October 16, 2017 137

Strong convexity, rational expectations, and collaboration with Robert Graves — Tailoring math developed for the physical sciences to economic problems — Centrality of Chicago to a more mathematically sophisticated approach to the measurement and description of economic trends — Simultaneous work of R.A. Polyak on strong convexity — Intellectual stimulation from year at Cowles Foundation at Yale, 1965 — Consulting for RAND Corporation, 1960s — Integrating study of real decision-making in business into economic theory and models — False economic assumptions about advertising — Relationship between economics and place; the nature of the University of Chicago — Moving from Graduate School of Business to economics department — Workshop system — Culture of economics department — "Why Should Manufacturers Want Fair Trade?" — Questioning conventional assumptions about monopoly and oligopoly — Expert witness in *Federal Trade Commission v. Kellogg, et al.*, 1972-82 — *United States v. American Tobacco Co.*, 1911 — More on "Why Should Manufacturers Want Fair Trade?" and resale price maintenance — Graduate student dissertations in 1960s, Kristian Palda, David Meiselman, Nathaniel Wilcox, Mary Sullivan — Theories of Competition workshop — Questioning Edward Chamberlin's assumptions about effects of price changes with market data — Effects of corporate concentration and problems of measurement and classification in the analysis in *Competition, Collusion, and Game Theory* — Mason Rosenthal on location of gasoline stations — *Competition, Collusion, and Game Theory*, origin of project

Interview 7: October 17, 2017 162

Kristian Palda dissertation and the serendipitous economics data from court cases — Cournot model for competition — F.Y. Edgeworth and the precursor of indifference curves — Teaching tools and highly technical course in econometrics — Development of Cournot-Nash equilibrium — Adaptive expectations, permanent income stream as subset of — Efficient estimators and Gauss — Search theory and George Stigler — Allen Jung's research on automobile dealer pricing — Relation of economics of information to core theory — Dominated and undominated coalitions, and the nonempty core — Misleading terms in game theory and influence of German language — Group rationality vs. the individual basis for rational expectations theory — Article, "Voting and Paying for Public Goods" — Core rationale for dealmaking, logrolling in politics — — Arrow's impossibility theorem, problems with — More on appendix to second edition of *Theory of Games and Economic Behavior* — Revealed preferences, problems with — Taking time into account in core theory — Static vs. dynamic prisoner's dilemma — Reception of

Competition, Collusion, and Game Theory among Chicago faculty and students — *United States v. Addyston Pipe and Steel Co.*, 1899 — Collaboration with William Best, John Egan, and Harlow Higinbotham, for A.T. Kearney consulting — "The Theory of Supply with Applications to the Ethical Pharmaceutical Industry"— Research for the Pharmaceutical Manufacturers Association — Research sponsored by Merck, and consequences for research

Interview 8: October 18, 2017 186

"Organized Futures Markets: Costs and Benefits" — T.W. Schultz and D. Gale Johnson, interest in market stability in agriculture — Standard Hicks/ Keynes model of futures markets — Organized futures markets convey price information and structure the costs of such conveyance — Similarity between futures and money — Importance of rules and structure in markets — More on consulting work for A.T. Kearney — *Economic Theory and the Core* — Engaging other theories in economics — Telser's work as mathematically sophisticated institutional economics — More on "Treasure of the Sierra Madre" problem — "You can't just wave your hands and say, 'Markets will always work'" — Theory informing observations — Viner Industries and kind-characteristic functions — Storage, supply, and the core — Correlograms — Measure theory and economics — More on core theory, Telser c. 1978 and Telser 2017— Overemphasis on economics of information — "Cooperation, Competition, and Efficiency" — mathematical sophistication of economics — Black Monday task force, 1987 — Keynes' "greater fool" theory — Disagreement with Milton Friedman about causes of Great Depression — Necessity of good rules and good judgment — Influence of Frank Knight's teaching — Task force recommendations

Interview 9: October 19, 2017 211

Underlying questions driving research — Gardiner Means — Economic questions versus economic slogans — Core theory as paradigm shift — *The Theory of Efficient Cooperation and Competition* — Constraints as norms — Marketing a theory— Influence of T.W. Schultz and D. Gale Johnson— Recent research on circuits and the problem of the Seven Bridges of Konigsberg — Relationship between firm structure and market structure — Welfare of firms and consumers — Research, innovation, and the core — Example of Manhattan Project — Research and free riders — Changing research landscape in 1980s United States and challenge of state sponsorship of research — Schumpeter and problems of patents — Editing for journals — Online publishing — Evolution of the Department of Economics at University of Chicago — Impact of digital revolution — *Joint Ventures of Labor and Capital* — Using core theory to understand aspects of the firm, e.g. relation between compensation of workers and size of firm, distribution of stock prices — Relationship between mathematical tools, e.g., fractiles, and underlying human behavior under analysis — "You're not going to get people to start looking for things, if there's nobody interested in what you're going to find"— Bonuses vs. profit-sharing— Block booking on the old motion-picture industry— Example of principal-agent problems — Historical context of game theory, e.g., strategic bombing — *Classic Futures: Lessons from the Past for the Electronic Age*

Interview 10: October 20, 2017 236

Applying core theory to electricity generation and distribution — Assembly lines — Technological change, economics of — Software industry and zero marginal cost — Social drivers of demand, e.g., conspicuous consumption — Use of core theory to explain market failure — Economists as market designers — Causes of the Crash of 2008 — Economic literacy and the poverty of public policy discourse — Core theory, cooperation, and voting — Minority control in a democracy — "John von Neumann's Growth Theorem Confirmed by Empirical Evidence II" — Reputation of Economics profession during and after the Great Recession of 2008 — Keynesian liquidity traps — Core theory explaining the weak economy recovery post-2008 — Message of economics in changing times — Lessons from mathematics, life, real-world work, mentors, and colleagues — Working in the diamond business, and negotiating terms rather than prices — Teaching — Why are things the way they are? — *le problème de Bachet* — "The Fed's Real Job," banking stability — 2008 crash and government bailout — Importance of cultivating judgment — Dodd-Frank — Derivatives — The nature of the public interest — Wife Sylvia and family

Interview 1: July 10, 2017

01-00:00:18

Burnett: This is Paul Burnett interviewing Doctor Lester Telser for the Economist Life Stories Project of the Science, Technology, and Medicine Series, for the Oral History Center. And we're here in Hyde Park, Chicago, and it's July 10, 2017, and we're here on our very first session. So, to begin, can we talk about your family background, where your family's from?

01-00:00:58

Telser: Okay. My father comes from a very small town on the border between Russia and Germany. At that time, it was Russia, but it is now, I believe, Lithuania, and, the German side was in East Prussia. The Russian town was called Vladislavov. And he was the youngest of five children; he had an older sister and two older brothers. The oldest sibling was a girl, his sister, then there were two boys, then a sister, Pearl, and then my father.

And my grandfather was very prosperous. He owned a tavern in the town and was probably the richest person in the town. So, for example, my father always had private tutors, and as far as I know, never attended any sort of a public school. He may have attended a Hebrew school or something like that occasionally. But as a student, he was taught German, and of course, he knew Russian, Hebrew, Aramaic, Lithuanian, and later, after World War I, he lived in Belgium for three years and he learned French.

01-00:02:57

Telser: When he was in Belgium, he learned French, and when he came to the United States with his parents in 1924—his father and mother, my grandparents—he then learned English. So he was an amazing linguist, and with, I would say, a very cultured interest in music, philosophy, literature, and in many ways, intellectually, very outstanding. And, his two brothers went to the United States before World War I. His oldest sister was married and lived in Memel, which was in Germany, and the next sister, Pearl, was married and moved to Brussels, Belgium, with her husband, whose name is Isaak Kubovitsky—I'll come back to him later—a very distinguished family.

But my father was supposed to inherit the business, so in order to do that, he had to serve in the Russian army. I should have mentioned that he was born in 1891. In 1912, he was twenty-one years old, and he was then drafted into the Russian army. That is probably one of the worst years to be drafted into one of the major powers' armies. So the war broke out in 1914. Before then, I should say, he was moved around. The Russians owned a very large part—well, practically all of Siberia, and they had designs on the British Empire in the south, so he was stationed for some time in a city called Tashkent, which is on the border between Russia and I think it's Afghanistan.

Anyway, so the war started, and the Russian army there was transferred to fight the Austrians. From August to September, 1914—I should mention that

the general commanding the Russian army was Brusilov, who was one of the famous Russian generals. And they were in a huge battle with the Austrians, and I am told, total casualties were 300,000, and his regiment, when they finally came out of the line, only 40 percent of them were still alive. Then, the battle went reasonably well, but one of the officers said to him, "Well, Jew, you came back!" So he said something appropriate in Russian, and at the first opportunity, he surrendered to the Austrian army, and he spent about a year or two as a prisoner of war of the Austrians.

However, his sister, Esther, was married to a German citizen, and he was a *feldwebel*, which means a sergeant in the German army. When the Austrians began to sort of conk out, the Germans took over and so, his brother-in-law was on the other side in that part of the war front. And so, he was transferred from the custody of the Austrians to his sister, Esther, in Memel, because his brother-in-law was a sergeant in the German army—and you should understand that a sergeant in the German army is a much bigger shot than a sergeant in the US Army. And his brother-in-law survived the war until 1918, when he died of influenza. The other thing that is interesting, which will come up later when we're talking about the Holocaust, because there's a lot of experience in my family related, or because of, the Holocaust: his brother-in-law got an Iron Cross, and this will turn out to be important later.

Anyway, and then, because his brother-in-law died in the influenza [pandemic of 1918], his son, Jacob, who was my first cousin, got a scholarship—and remember, this is still part of Germany at that time — to the *Technische Hochschule* in Berlin, which we would say is the counterpart of MIT, although I think it'd be more accurate to say that MIT is modeled after the *Hochschule*, and one of their faculty members was Albert Einstein. So it's a very prestigious operation, and he had a master's degree in chemical engineering; this is another factor that becomes important later on. Now, that covers my father's side of the family. On my mother's side, she was born in 1904.

01-00:09:47

Telser: I have to say more about my grandfather. My grandfather was very talented. I almost forgot; this is an important insight into his business acumen: When the war began, he said, "Well, when there's a war, one side wins and the other side loses. I don't know who's going to win, the Russians, or the Germans." So he sold his business, and he invested half of the funds into German Marks and the other half into Russian rubles. It turns out, however, that this was a hedge that didn't work, because both sides lost. Now the amount of money involved was pretty substantial. In US dollars, in 1914, it was $25,000 in rubles, and $25,000 in Reichsmarks. I don't know what that would be the equivalent today, but pretty substantial, perhaps on the order of a million dollars or so, at least.

I should also mention—this will come up later: he was also very talented in many ways. Let's see. He wrote in Hebrew. He wrote poetry in Hebrew and transcribed it in calligraphic, beautifully written Hebrew in books, and he would give a copy of a volume to each of his children. So each daughter and each son got one copy of the poetry that he wrote and that he transcribed into beautifully written Hebrew letters.

01-00:11:58
Sylvia Telser: And bound the books!

01-00:12:00
Telser: And bound the books, yes. And he was also very skilled in whittling and carving things with a knife, and cabinet making. He was a very talented man.

01-00:12:17
Burnett: Can I ask, he was an engineer? Is that right?

01-00:12:19
Telser: My grandfather?

01-00:12:20
Burnett: Yeah.

01-00:12:21
Telser: No, he was a businessman.

01-00:12:22
Burnett: He was a businessman. He was investing in different kinds of things. Was there a specialty that he had?

01-00:12:29
Telser: Well, in addition to having a tavern, which was a central sort of meeting place for the people in the town, part of the business was also smuggling. My uncle would refer to him as a merchant, but what he did is, he traded between Russia and Germany without paying tariffs. So that was another substantial source of income. I can give you another little story involving my aunt Pearl and Isaak Kubovitsky. This will be related later to a very well-known economist. In that town, there was a very famous cantor, and the cantor was Kubovitsky's father, and there was also a rabbi. But the rabbi and the cantor did not get along, and my grandfather sided with the cantor, because he said, "A good cantor is really very hard to find, but rabbis are a dozen for a kopeck." So, the rabbi left and ended up as a chief rabbi in Edinburgh in Scotland.

And then, to leap ahead in the story, it turns out that his grandson is Bob Aumann, who is a very famous economist. And I knew Aumann when we were both together at the Cowles Foundation in Yale, and Aumann did very important work in economics and he got a Nobel Prize. And at the dinner for those who got honorary degrees from the U of C—I had proposed him for the honorary degree long before he got his Nobel —he was sitting next to me, and

we started exchanging stories about families. And it turns out that his grandfather was the rabbi who had been forced to leave the town by my grandfather. So we had this connection going back, I don't know, seventy years or so, and that was kind of amusing. So, this is a sideline about my grandfather, and I had forgot. And my grandfather was a very shrewd and very likable person, in many ways, very outstanding.

Anyway, after World War I, since both Germany and Russia had lost, Memel was no longer German, and I guess it was transferred to Lithuania. So the family left. My grandmother, grandfather, and father left, went to Belgium, where they lived with his daughter, Pearl, and her husband. And then, to leap ahead on Kubovitsky, to mention him — as you know, one of the leading war criminals was Eichmann, and Isaak Kubovitsky's brother [Leon] was very important in the underground during World War II [rescuing Jews], so he was what you might call a professional spook.

Then what happened was, when the Israelis found out where Eichmann was, that he was in Argentina, Kubovitsky was the minister, the Israeli minister, in Buenos Aires, because they decided it would be a good idea to have somebody experienced in this sort of thing to be on the scene in case there was any trouble in capturing Eichmann. So, Leon Kubovitsky was the minister and had this kind of a background, although I do not think he was actually involved directly in any of the operations to get Eichmann. But this sort of gives you an idea of another branch of the family, which, I would say, in general, was pretty distinguished. We'll come to another one a little bit later.

Anyway, so my grandparents and father came to Chicago where they had two sons, and my father first worked with one of them, Julius, during the 1920s, and then around 1930 or so, started his own business making mattresses. Before that, in 1929, my father married my mother. And my mother, by the way, who was born in 1904, was ten years old when the war started in the little town that she grew up in, which was in Russia, now part of Poland, called Grajewo, and there were some big battles in Grajewo between the Russians and the Germans. The Russians didn't do that well in the northern part of Europe, and her father had died in 1907, so she, her mother, and her older brother and younger sister, moved to Białystok, which was then in German hands. So they spent most of World War I in the German regime, rather than under the Russians, which was much better.

What I do remember from her is that when the Bolsheviks took over in 1917, the Bolshevik army essentially took over that part of Poland, and they were eventually forced to depart as part of the—I'm not exactly sure what was going on—the Versailles Treaty or something, and Poland became independent. But my mother did not want to stay in Poland, and she left Poland and came to the United States to be with her sister in 1924. So she came to the US in 1924, but most of her family stayed behind in Poland. And

by the way, her name, that family, is Greenspan. And it turns out, although I never verified this, that my maternal grandfather had an older brother, Greenspan, who came to New York in the 1890s. So there is a vague possibility of some kind of a connection between Alan Greenspan and me, but we never met.

So, my parents got married in 1929, and I was born in 1931. And then the Great Depression was well underway by then, and it was pretty tough, pretty tough on my family.

01-00:20:41
Burnett: Can I stop you right there, and ask you about how you learned of these stories, from your grandfather, from your father? And, these are stories about a lot of chaos, a lot of tragedy. How were those stories recounted as family tales?

01-00:21:07
Telser: Well, I should mention, there's one other important point that's relevant to your question. My grandmother died in 1933 or four. No, I was three years old.

01-00:21:23
Sylvia Telser: Thirty-four, you were—

01-00:21:24
Telser: Nineteen thirty-four. So she died in 1934, and we moved. At that time, we were living on the west side in Humboldt Park, and in 1936, we moved from Humboldt Park to Hyde Park in a larger apartment, and my grandfather lived with us. So my grandfather lived with us from about late 1936 until he died in 1940. And by the way, my grandfather was born in 1859. This will turn out to be important in another part of the story, a little bit later. So, my grandfather lived with us. He did not speak English; he spoke Yiddish. So I could talk to him and learn Yiddish, but most of the stories I heard from my mother, mostly from my mother, and some, from my father.

My father, he was not religious. He is one of these ardently Jewish, non-religious Jews. And the other thing that I think is important that distinguishes both my parents from a lot of the other Jews of that generation, is [that he was] strongly anti-Communist and anti-Bolshevik. And this, I think, is, to a large extent, because of their direct experience with the Bolsheviks in the early years, who were pretty bad, although not as bad as they were later on. But there were never any particularly fond feelings for Stalin or the Communists or any of those. But they were very ardent Zionists.

Another thing that I should mention about my uncle Isaak, and the family living in Brussels, and that is, they were ardently Zionist, and my uncle actually built the first brick factory in Jerusalem in 1919, but the British objected because they did not want buildings in Jerusalem built of bricks. They wanted them to be built of stones to fit in with the general old-city

outlook of Jerusalem. So, most of these, many of these stories, I got from my mother, some from my father, and some, from my *zayde*, my grandfather.

01-00:24:22

Telser: My mother came to the Chicago in 1924, and both my parents and grandparents came just before the passage of the McCarran Act in 1924, when immigration into the United States virtually ceased, or became, at least, very, very difficult.

So now we go back. It's now around 1936. My grandfather is living with us, and he used to sit at a table and write his books and I would watch him. He was very artistic. One of the little things I could mention is the way he made a cheese sandwich. He did not just take a piece of bread and put a piece of cheese on it. What he would do is, he would cut the bread in strips, he would cut the cheese in strips, and put one strip of cheese on each strip of bread, and eat it as if they're canapés. And so it was a very elaborate procedure. And he also had made very intricate, wooden—I don't know what you'd call them—things that you would put on your desk to hold—letter holders and things like that, very intricately carved letter holders, ink stands, and so on. So he was really a very talented man.

Now, another part of the story which involves my grandfather and the date is: He was born in 1860, and his oldest sister had a child also born in 1860. And his oldest sister married a man called Copeland. The Copelands had a daughter Fanny, born in 1859, who later married a Salinger. The Salingers came to the Chicago in 1882. But my grandfather [and her future husband]—the uncle and the nephew [by marriage]—were the same age, and they played together from childhood on. And then when my grandfather lived with us, Doctor Salinger would visit my grandfather every Sunday. Now, Dr. Salinger had two sons. One, Samuel Salinger, became a very prominent ENT doctor, and the other son, Solomon Salinger, moved to New York and was a cheese merchant, cheese importer.

And one of the main topics of conversation between Salinger and my grandfather was about Salinger's nephew, who was being kicked out of one boarding school after another, and had a very hard time. It turns out, that's J. D. Salinger. So J. D. Salinger is actually my second cousin. And when I was a kid, I was about eight or nine years old or so, I would hear this discussion about, "What will become of Jerry? Why doesn't he finish school and settle down and stop this fooling around?" So it turned out, he turned out pretty well. [laughter]

In grammar school—now let's go back— I had some serious illness. One of the common illnesses in those days was scarlet fever. There was no penicillin, no antibiotics, so scarlet fever was a big killer of little kids, and if you survived, there were usually serious side effects, and in my case, mastoiditis. And the one who operated on me and did a very good job was actually Samuel

Salinger, Doctor Salinger, who was my father's second cousin, I guess, second cousin, yeah. Anyway, so Doctor Salinger was my second cousin. Oh, he was my father's first cousin once removed. Okay, I got it right. Anyway, so I had mastoiditis, and survived. One of my cousins had mastoiditis and did not survive.

When we moved to the South Side, I attended Kozminski School, which is still here, and I did pretty well in school, but nothing particularly special, except for the fact that I read a lot as a little kid. Oh, and I should mention that we had a number of relatives who had been very wealthy in the 1920s and went bankrupt in the Great Depression. In fact, there were two of them, two of the families. One, Levy—I should mention that my paternal grandmother is Levy. And so, one of the Levys went bankrupt, and his son, for not hard to understand reasons, became quite radicalized and went to Spain in the Abraham Lincoln Brigade in the Spanish Civil War. And the other one, well, they lost a lot of money, but they managed to live reasonably well during the Great Depression.

But the Depression did hit the family very hard. It also hit my father very hard, and his business foundered. It was a struggle, and I remember—well, not so much then, but a little bit later—there was difficulty in having enough money to pay the rent. And the effect of the Great Depression on our family and all the other families was really quite severe. Since we lived in Hyde Park, and in those days, University of Chicago faculty were not very well paid, a lot of the kids or children of the faculty attended publics schools, and one of them was my friend, Alan Albert. And one day, he comes to school—I always remember this—and he says, "Do you know who was at our house last night?" And we said, "No, who was at your house last night?" And he said, "Albert Einstein!" Why was Albert Einstein at Allen Albert's house? Because Allen's father is Adrian Albert, who was a very famous mathematician. So Einstein had come here to give a seminar and after the seminar, there was a sort of little party at Albert's house.

And then later, in the fifties, when I was in charge of the seminar program at Chicago—so, every Thursday night, we would have a seminar with visiting economists, and once, it was Paul Samuelson. So I asked my son, Joshua, whether he would tell the kids in school the next day who was in his house, to guess who was in his house Thursday night. Actually, he didn't tell them that. I always remember, Joshua, for some reason, maybe he picked it up from me, didn't think as much of Samuelson as Albert thought of [Einstein]. Although Samuelson probably thought of himself as the equivalent of Einstein, most people would not agree with him. [laughter]

01-00:34:05

Burnett: So you're growing up in grammar school, elementary school, in Hyde Park, and your father's business had foundered. He started in '29 in mattress distribution, or manufacturing?

01-00:34:21
Telser: Manufacturing.

01-00:34:22
Burnett: Manufacturing, so that you can understand—

01-00:34:23
Telser: Well, it was a little later. It was around 1930 or ’31.

01-00:34:27
Burnett: Okay. And so, you’re making do, both parents were working or trying to scrape by, but, you’re in Hyde Park, which is in close proximity to the University of Chicago, which is this great engine of research, and so there’s some kind of partial compensation or insulation from the tremendous market depression that you were experiencing at that time. And so, you grew up in an environment where learning is important. All around you, not just your family, but your neighbors’ kids, or the kids that you played with on the street, there was a good chance that their parents were educated, as well. So it’s this really, really great environment for you to grow up in, I imagine.

01-00:35:18
Telser: Yes, definitely. But I didn’t really develop outside interests until I got to high school. Now, in high school, one of my best friends was Russell Bensley, and Russell lived with his grandparents. Russell’s mother had been a professor in the medical school at the U of C. They were originally from Canada, by the way, and Russell’s mother was a professor in the medical school here, and she moved to the University of Toronto. And so, in order for Russell to continue regular schooling that he had started in Chicago, he lived with his grandparents.

Now, his grandfather is one of the original faculty members of the University of Chicago, and he was a very distinguished man. And when I would go with Russell to his house, what really impressed me was, he treated me like an adult. He was really, very impressive. He was a rock-ribbed Republican, and he was a charter member of the American-Soviet Friendship Council, started, I don’t remember, 1933 or something, and he talked to me like an adult. He said, “Well, you should have diplomatic relations with other countries whether you like them or not. This is a sensible way to behave.”

Another thing I should mention about Bensley is his next-door neighbor, Edward Teller. Now, Edward Teller knew that his phone was tapped by the FBI, so whenever he wanted to make a personal phone call, he would come over to the Bensleys to make the phone call. And it occurred to me and to Russell that that was kind of foolish for him to think that the FBI would not also tap Bensley’s telephone, [laughs] and that they were unaware of what he was doing. But that’s another minor little story.

Anyway, Russell and I were very close, and Russell was very interested in newspapers and journalism. Now, in high school, there was a branch of the

Hyde Park High School that was small and in the neighborhood. So the first year of high school was at the branch, and the next three years were at the main. And it was in high school that I began to encounter really outstanding teachers, for example, the one, and I always remembered this: I had an English teacher, Mrs. Slocum, and she had us read the play, *Julius Caesar*. And one of the first things she asked us was, "Who was the hero of the play?" So I said, "Well, it's obvious. It's Julius Caesar." And she said, "No, Julius Caesar is not the hero of the play. Marcus Brutus is the hero of the play. Marcus, his ancestor, is the founder of the Roman Republic, and Marcus Brutus wanted to get rid of Caesar to restore the Roman Republic." That had a big effect on me, and I must say that *The New York Times*, in the review of the current production of *Julius Caesar* in Central Park, doesn't seem to get this at all. [laughter] Anyway, they do not understand Shakespeare's *Julius Caesar*.

So I remember the algebra class. Alan Albert and I were in the algebra class and the teacher, I remember her, Mrs. Hansen, she was worried that Adrian Albert would look over Alan's homework and be very critical of what she was doing. Well, she obviously did not know anything about Adrian Albert. He couldn't care less about [laughs] a high school algebra class. Anyway, starting in my sophomore year, and Russell was a year ahead of me, I became very interested in the high school paper, and I started writing weekly columns; and in my junior year, I became the feature editor; and in my senior year, the main editor. And Russell, because of his great interest in journalism, got very interested in understanding how newspapers actually functioned.

So, for example, we would go to the printer and watch them setting up the type and proofing it, so we actually saw the printing of the paper from the very beginning. And for photo engraving, we would go to the Chicago Tribune Tower and watch them making the photo engraving blocks for our high school paper, and so on. Now, the Hyde Park High School paper was one of the leading papers in Illinois, and it usually won the prize for being the best newspaper in the state. And one of the famous graduates, by the way, of Hyde Park, was Steve Allen, who is a television personality. So there was Amelia Earhart, Steve Allen, and by the way, and Paul Samuelson was a graduate of Hyde Park High School. And if you look at the page at the—what do you call it—the Google, Wikipedia page or whatever, one of those television pages, they have two economists who are graduates of Hyde Park High School: Paul Samuelson and me. [laughter]

So I was very interested in journalism, and writing columns, and I wrote short stories, and I also had a very good geometry teacher, Joseph Nyberg. Now Nyberg had been originally an instructor of mathematics at University of Wisconsin, and the story is that he was a radical and had been fired, so he ended up as a high school teacher in Hyde Park. That was my first exposure to real mathematics, and it had a very strong effect on me. In my third year, I had a course in trigonometry, where all you had to do was complicated arithmetic

calculations in trigonometry, and that killed my interest in math. So from my third year of high school on, no math at all.

01-00:43:24

Burnett: Can I ask you—one of the purposes of these oral histories is that we'd eventually like to use them for classrooms, K-12 classrooms, and one of the assignments or parts of the basic curriculum for grade three, across most of the United States, is for grade three students to think about, or talk about, what it's like to be a grade-three nine-year-old, right? And so, I'm wondering if you could reflect a bit on taking yourself back to being a kid, and what made you interested in the things that you were interested in. You mentioned mathematics, your first introduction to that, but also these other things. What kind of kid were you? Let me put it that way.

01-00:44:23

Telser: Okay. There was an important element for me. Well, first of all, I was completely ignorant of the fact that there was a discipline such as economics. I knew that there was chemistry and physics and things like that, but I had no idea that economics was considered some kind of a scholarly discipline. But the main background in the family was business, so one of the things that I was interested in is trading comic books and trading sports cards. It still amazes me that, I didn't have any money, I couldn't buy any of this stuff, and somehow, by making shrewd trades, I built up a pretty good inventory of comic books and baseball cards, which, unfortunately, my mother threw out. If I had them today, I would probably be very rich. [laughter] But anyway, like a lot of mothers, she threw them out, but I had some highly regarded baseball cards and comic books, so that was one thing.

Also, I played baseball and basketball, and sports like that were important. Now in particular, the baseball that we played was not softball. It was hardball. So we played baseball in Washington Park, which is a big park about a mile west of us. And many people don't really appreciate the difference between real baseball and softball. And one of the things that happened to me, that, as a matter of fact, left me with sort of a permanent injury: I remember, I was playing, it was either shortstop or left field, and there was a very hard line drive, and I jumped for it, and I managed to get it in my glove, but I dropped it, and the ball hit my left thumb and snapped it all the way back.

So it was seriously injured, and I remember, there was an osteopathic hospital across from our apartment, and I remember going there and they put it back in position. They pulled it or whatever. They're experts on this sort of thing and they put it back, but my left thumb is seriously deficient ever since. And one of the guys that we played with, Joe Lukinsky, he was a very good baseball player. He was so good that, later, he tried out for the Minor Leagues, and I think he did get into one of the lower-level Minor League teams, but he didn't make it very far, and he ended up becoming a rabbi. [laughter] But sports was important. And baseball was important.

01-00:48:06
Burnett: And, music, the arts, in your family?

01-00:48:10
Telser: Well, that's sort of interesting. Although I became interested in classical music in high school, and my parents were very interested in classical music—and I had cousins who were very good pianists and had recitals, and I would be taken to these recitals and be completely bored, but I was forced to go—I really didn't like classical music at all. And what really changed me was that a course in high school was compulsory on learning to appreciate music, and what I remember was that the teacher played Tchaikovsky's *Fifth Symphony* every day for about, I don't know, three months. And instead of being bored, it became very interesting. But I have to admit that, as a reward, she said, "Tomorrow, I'm going to play *Rhapsody in Blue*," and everybody cheered. [laughs]

01-00:49:32
Burnett: Wow.

01-00:49:34
Telser: So, music was not a major interest. Oh, and there was another thing, by the way, on comic books and baseball cards. There's another important factor, element, that I forgot to mention, and that is, we had refugees living with us. First, we had a German refugee, Werner, in 1938, and his family was allowed to get out from Germany. His father was a cabinetmaker, so that he was a special category because of his skill. So they were allowed entry into the US. And after Werner, the second refugee was from Belgium, [speaking in accented voice] Henri Stein, [resumes normal speaking voice] or Henry Stein. And his parents, they were also from Belgium. They managed to get out of Belgium into Portugal, and the war had started, but the US was not in a war, and so they were on a neutral ship. They were able to cross the Atlantic, come to the US, and Henry Stein joined them in New York.

And then the third person that we had was an orphan from an orphan asylum—orphan asylums were broken up: Bernard Salas. And so he lived with us until he was drafted into the Army, which I think was in 1944 or something like that. But there's an interesting other experience connected with Bernard. He had an older brother, Meyer Salas, who had been also in the orphan asylum that was disbanded, and he became a printer, and he was a musician. I think he played the trumpet. He was pretty good at it. And so, Meyer Salas was drafted into the Army and was in the band.

However, by 1944—and he was in Europe—there was a very desperate need for infantry, so Meyer was transferred out of the band and into the infantry. And then, something happened: he was killed. And what I very vividly remember is, in that kind of a situation, you get from the Army, you get a package with what was in his pockets, last remains, and a Purple Heart. And that, that leaves an indelible impression on you. And that, not everybody,

fortunately, has that experience, but it made a big impression on me, and certainly on the other people in my family.

And then, of course, then if we come back, besides high school and so on—this brings me back to the Holocaust—all of my relatives who were in Poland were killed in death camps. And of my relatives in Western Europe, one, Lisette, my cousin, was killed by the Nazis; my aunt and uncle, Pearl and Isaak, were hidden on a farm in Belgium; and Simone was able to get out of Belgium earlier, and she got to New York. The one that I knew best at the time was Simone. She was the oldest of the Kubovitsky kids. And my uncle, Isaak, refused to be a member of what was called the *Judenrat*. The Nazis set up councils of Jews who were supposed to pick the ones who would be saved, or, I'm not exactly sure if they were saved or the ones who would be sent to the camps, or what. If you weren't saved, you would be sent to the death camps.

Anyway, he refused. And if you refused, that would be your end, but he was the president of the General Zionist Organization of Belgium, the Netherlands, and Luxembourg. So he was very prominent, and the underground smuggled him out and they lived on a farm. Now it's funny. He visited us in 1946 after the war, and I remember, he told us that he spoke French with an accent. So whenever he would go into a village to buy groceries, he kept a pipe in his mouth to sort of disguise the imperfect accent or pronunciation of French. Anyway, we lost a lot of relatives, many relatives. I don't know.

Oh, and then, of course, the Gidanskys, Esther and her husband, and I guess, yeah, their daughter Paula was in France, so she survived. And Jacob, my cousin, the one who had the master's degree in chemical engineering, he was in Dachau, but, because he was a chemical engineer, the Nazis used him in a tire plant. And after the war, he wanted to be released, but the Russians captured the camp and they took him, and he had to work. He was not allowed to leave Russia until he was sixty, when he emigrated to Israel.

01-00:56:37
Burnett: My goodness. Well, speaking of what it's like to be nine years old, I think of, 1940, you were nine years old. Is that right?

01-00:56:46
Telser: Yeah.

01-00:56:47
Burnett: And 1940 is when the first refugee comes to stay with you. Is that right?

01-00:56:53
Telser: No, no. The first refugee was '38.

01-00:56:58
Burnett: Okay. So, what did your parents tell you about these people coming to stay with you? What was the explanation?

01-00:57:08
Telser: Well, they said that the Nazis had taken over, and there was a program to exterminate the Jews. There wasn't any—

01-00:57:21
Burnett: Sugar coating.

01-00:57:22
Telser: Yeah. No, no, there wasn't any attempt to avoid or to tell stories or anything like that. And of course, at first, we were getting letters, and the letters would come with German censorship markings, opened, and so on, and then the letters stopped. And when the letters stopped, it was pretty obvious what had happened. There was one, now, my uncle, Sam, my father's brother, they had a refugee. He was a German refugee and his family had been wiped out. But anyway, he was drafted into the Army.

01-00:58:34
Sylvia Telser: Ernie.

01-00:58:35
Telser: Ernest, yeah, Ernie, Ernie Hecht, and he was drafted into the Army and became, I believe, a first sergeant or something, and he got a Silver Star; that's a medal for combat. And of course, Salinger was in combat for eleven months, and he had a pretty hard war and afterwards cracked up, which is not surprising. In those days, except under extreme pressure, you weren't really supposed to be under combat conditions for very long periods of time. But in World War II, that wasn't the case. They needed people. I remember, in college, for example, when I started college, there was one guy who was in a platoon that landed on D-Day. And he told me that, of his platoon, there were only three survivors by the end of the war, which was out of about, I don't know, seventeen or so.

01-00:59:57
Burnett: Right. Tremendous losses during that period. But as I hear your family's story, I think about, back to World War I, many historians say, one of the important causes of World War I was this extreme nationalism, and this national pride, and gamesmanship among the Great Powers. And then in the story of your family, they're kind of liminal, in the sense that they're literally between two countries: on the border, and your grandfather was dealing between two countries, and on the down low, as it were. And so, there's this experience of being part of something and not part of something, being kind of outsiders. Did that impact you in a certain way, or did it impact your family, thinking that, in terms of your sense of belonging, is it more at the family level? Because your family is really—you have to rely on each other, because of these tragedies, because of these forces.

01-01:01:15
Telser: Well, my family, let me put it this way, they were very strongly pro-FDR, and they had a very high opinion of FDR. I would say there were certain

reservations, but they still had a much higher regard for FDR. Now, the other thing that happened, though, and this is sort of partly of my own—I won't say considerable, but there was a certain amount of antisemitism in the US, and as the news came out after the war, it changed. Now, for example, I, personally, did not experience any antisemitism in the Army, at all, or very little, very minor in high school, practically nothing, even during the war. So I think that there was a change. It isn't so much that we changed, but I think that the American people changed as a result of these experiences in the war. I had one cousin who was in combat, but he was in the Pacific, so that was different. Ernie was the only one, that we knew reasonably well, in combat in Europe.

But I think that there were very strong patriotic feelings, the feelings that the United States was unique, a special place, and that there was no other country like it in the world. And that, actually, I would say, was reinforced when Sylvia and I and the family, when we lived in Belgium for a year. And I must say that, by and large, we did not find a very friendly attitude by the Belgians towards us or towards Americans.

Now, I don't know if it's because, in general, that the Europeans are not as nice to each other, not just to Americans, but to any other Europeans; I don't know if that's what's going on, or it's because they're particularly anti-American. I don't know. But when you live in a foreign country like that, it becomes clear. But the Belgians, I think, were worse in this respect, but of course, the only other place, foreign country, that we lived in for a while was in England for, I guess it was three or four weeks or so, but that was different. There's a sort of a different attitude in England. But the Belgians, you really had the experience of a somewhat unfriendly—and that reinforces positive feelings that we have about the United States.

01-01:05:23

Burnett: Right. And, I suppose, during the war, there was more of the war propaganda, "Why We Fight." A lot of that "Why We Fight" propaganda was to define the United States as a more inclusive place, that democracy unites us all, that kind of thing, versus an enemy that is about saying, "We're better than everybody else." And I think that probably influenced folks.

So, World War II changes so many things. It changes so many things in the economy. It changes so many things in terms of the kind of social science that you're going to get into. The war itself, how did that affect daily living in your family? The market is kind of suspended during World War II. There's price administration. There's ration cards, and all of this kind of stuff. Can you talk a little bit about that experience? You were young then.

01-01:06:29

Telser: Yeah. A lot of things were rationed, but we didn't have a car, so gasoline rationing did not affect us. There were rent controls. At that age, the war, in that sense, did not have that much of an impact on me. [*camera stopped, and*

narrator recalls the fact that his father's business went bankrupt during the war] That was important; somehow, I hadn't focused on it. But, it is certainly true that, by 1942, it was no longer possible for him to remain in business, because—

01-01:07:39
Burnett: For your father.

01-01:07:40
Telser: —yeah, because he couldn't get the materials to make mattresses. It was just over. You had to have priorities. And so, only businesses that were directly involved in war work or something like that were able to get the raw materials to continue in business, and he couldn't. So the business folded up, and he ended up working in a jewelry store for others. So by then, he was in his fifties, middle fifties, and it really wasn't possible for him, and his income decreased. And one of the important effects of this, well, although it came out a little bit later, is that then, my mother worked. So she had to go to work in order to keep the family going. And by the way, my grandfather died in 1941. I was ten years old. We moved into a smaller apartment. The rent was reduced. It was not as nice an apartment. Well, it's an indirect effect of the war, because his manufacturing business had collapsed, so that was, I would say, probably the main cause of this. It wasn't actually the war itself that caused the reduction in our standard of living. I think that would be a correct way of describing it.

01-01:10:12
Burnett: Right, so the family found itself in more challenging circumstances.

01-01:10:18
Telser: Yes, and poor.

01-01:10:20
Burnett: Right. And so, we can link this up then with your attending high school. When did you start high school? What year, do you remember?

01-01:10:32
Telser: Let's see. In 1944. I was thirteen in February, 1944.

01-01:10:41
Burnett: Okay. And so the war ends, and there are some lags after the end of the war, because there's a lot of instability. I think there's a lot of strikes after World War II, and prices are going up, and so there's a bit of a struggle in the immediate aftermath of World War II as well, and that's when you're in high school.

01-01:11:08
Telser: Yeah, well, let's see. The war ended about fifteen months after I had entered high school. In fact, one of the things I vividly remember is, I was going to visit my cousins who lived within a block of our apartment on December 7, 1941, and I was going down the street. It was a Sunday, and all of a sudden,

people opened windows and they said the Japs attacked Pearl Harbor. And so I very vividly remember the opening of the war.

There's another aspect to this that I didn't mention that comes back to me now. One of the things that I did when I was about thirteen or so is, a friend of mine and I went to—there was a rifle range in the west stands of Stagg Field. And the coach, it was a U of C coach. He was an Olympic medal winner, and so he was teaching these young kids competitive marksmanship, and in the lobby of the west stands of Stagg Field.

Now the rifle range was on the south side, and then there was a blocked-off steel door on the left side. And it turns out, as we found out afterwards, that the pile[1] was behind the steel door on the left side, and what I remember doing is, I picked up a very strange Erlenmeyer flask. It was not made of Pyrex, which was the usual material; it was made of some kind of a plastic. And I took it to my science teacher in high school—this is my first year of high school—and he looked at it, and he said, "Gee," he'd never seen one like this before. [laughs] So they had these things in boxes outside the entry. Of course, I suppose they assumed that nobody would know or have any curiosity about what kind of stuff was going on behind these steel doors.

But after the war, I was interested in writing. There were two things that I got when the war ended that were very important for me: one was a portable typewriter, and the other was a bicycle. You couldn't get either one of those things while the war was on. And the typewriter, of course, was very important for me because I was writing these columns, and I wrote some short stories, and so on, and I learned how to do touch typing. So that was a sort of a beneficial effect.

01-01:14:49

Burnett: And I guess, an early passion of yours was writing, just to be explicit about it. And around that time, you're beginning to have an interest in mathematics, because you had that good math teacher who exposed you to things. Before you got bored with trigonometry, which I assume was boring because it was just so repetitive, and it was just plugging in formulas and so on, what was exciting? What was the initial excitement for you about math?

01-01:15:25

Telser: Well, the thing that really aroused my interest was the fact that it was a logical structure, that there were a sequence of propositions, so that according to certain rules of logic, one thing would follow another thing, that there was no room for doubting what was correct and what was incorrect. You could prove something or you could prove it wasn't true. It gave you a feeling of order, that you could really understand something very fully about what was going on. It was something that I had not really been aware of before.

[1] Chicago Pile-1, the world's first nuclear reactor, built as part of the Manhattan Project.

01-01:16:22
Sylvia Telser: Was that Nyberg?

01-01:16:23
Telser: What? Yeah, Mr. Nyberg. Did I mention—

01-01:16:27
Burnett: You did mention him, yeah, Mr. Nyberg. He was accused of being a Communist or something, so he got shunted into—

01-01:16:35
Telser: And what he did, he actually wrote a textbook. What he did was, the course was really Euclid, so that you had axioms and theorems, and nobody had ever presented mathematics in that way to me before, and it was a revelation. It was a whole new world to see how these things worked. And then I was very disappointed when we were doing trigonometry that none of this entered. Although the thing that was interesting is that when I went to Roosevelt in college, I had a course in trigonometry, and it was completely different. The course in trigonometry at Roosevelt was really a branch of mathematics that was just like the course of geometry. You're learning something about the structure of functions using trigonometric series. That was a different experience than I had in this trig course.

01-01:17:57
Burnett: And what made the difference for you? Was it the way in which it was presented?

01-01:18:02
Telser: Well, it was the content. Well, let me put it to you this way. Many people seem to think of mathematics as arithmetic, but mathematics is really not arithmetic. Arithmetic is just a bunch of rules to calculate certain useful things. But with mathematics, you're not doing that. You're trying to find deeper relations among certain entities that are not easily visible, and some of these are very surprising. In fact, I had, just a couple days ago, an experience of this sort of thing in what I was doing. It turns out that there was a relation among entities that I had never—[phone rings] it was a complete surprise. I did not anticipate that there would be such a relation at all among these entities. It was completely new. And I'm not sure I really understand it right now, but that is the sort of thing that you get out of math. Again, to leap ahead—

01-01:19:39
Sylvia Telser: Wasn't that Norman Rudy, though?

01-01:19:44
Telser: No, no. The symbolic logic course; that's another course. There were two: one is Norman Rudy, the other is Lionel Ruby, and Ruby was the one who taught the course in symbolic logic. Now, symbolic logic is a relatively new discipline in mathematics, started mostly by Boole in the nineteenth century, and it is a method of applying symbols and relations to study statements, or

assertions, as an attempt to try and make very clear logical implications about relations in the real world. And so, it's a way of converting a method to things that you see in the world that are not obviously numbers or mathematical or geometric. It's a way of thinking of things that was completely new and different, starting in the middle of the nineteenth century.

By the way, another thing that always struck me about this is, some of the main pioneers in this were women. And the reason, I think, is that the traditional mathematicians are mostly male, but when something new like this comes along, then the really talented women go into it, and they don't have to put up with what you might call persecution or something by the existing authorities.

01-01:21:53
Burnett: I think the historians of science would agree with you on that. So these are new domains for you. This is at Roosevelt University. So you graduate from Hyde Park High, and you attend, is it 1947 or eight?

01-01:22:15
Telser: Let's see. It was 1948. I was seventeen.

01-01:22:20
Burnett: Right. And so, this new world of mathematics was introduced to you in high school, and you develop it further, and you go into these courses that are all new vistas for you. And in terms of the teaching, was it connected, was it the abstract nature of it, or was it the fact that they would say, "You can use this to understand something in the real world?"

01-01:22:50
Telser: Although I think people think of me as sort of an abstract theorist, that isn't the way I think of myself. I think of myself as using this stuff to understand the real world. The emphasis is on understanding reality, not just on playing around with the symbolic relations. And it was a big breakthrough for me to realize that it would be possible to understand things. For example, the fact that it would be possible to see some kind of logical structure in business relations and in economic relations using these methods was a revelation to me that these tools made possible.

And one of the things that's also, I think, important here is that, in some of the traditional economic theory, there is a tendency to use mathematical tools that are not really appropriate to the problems that we face. That is, they're tools that were used to study physics, but they are not good tools to use in studying economic relations. The math that you need to study economic relations is not really calculus and things like that. In economic relations, there are what we call combinatorial problems, discrete problems, and things of that sort, where calculus doesn't work.

Now one of the points that von Neumann made, which I think is kind of funny: he said, "Well, you can really make progress in the combinatorial problems if you can somehow convert it into a continuous version." Well, sometimes that's true, but in a lot of important cases, that has not been true. And if you try to do that, you're sort of trying to bend a tool to do something that it really wasn't meant to do, and you're liable to be frustrated. Sometimes it works, but often, it doesn't.

01-01:25:32

Burnett: So, if I understand things correctly, your exposure is to these forms of mathematics first. You had exposure to the world of business through your family, but in terms of formal economics, like, let's say, neoclassical economics of late nineteenth century, you had this other math first. Or was it coterminous?

01-01:26:00

Telser: No. I had the usual sequence of math, and the only type of math that I had that was modern, you might say, was in matrix algebra. That was a course much later at the U of C. But the early math courses at Roosevelt, with the exception of the course on symbolic logic, and the fact that one of the instructors, Norman Rudy, was a student of Jimmie Savage, who is certainly a modern mathematician—and it was from Norman Rudy I got my first exposure to game theory—by and large, most of the math that I had at Roosevelt was pretty traditional. Differential equations and calculus and analytical geometry and all of that, that's really the standard math that is used in the physical sciences.

01-01:27:24

Burnett: So, *Theory of Games and Economic Behavior*, von Neumann and Morgenstern, is '44, and then there's that second edition in '47. You enter Roosevelt in '48, and you are exposed to game theory as a teenager.

01-01:27:46

Telser: No, no. Well, no, I was exposed to game theory at Roosevelt. Well, I guess I was eighteen. I guess, technically, I was a teen—

01-01:27:57

Burnett: Technically a teenager.

01-01:27:58

Burnett: My point is that you're very young, and that game theory is also very young. This stuff is published. Economists first become aware that this might be useful for their field the year that you entered Roosevelt, basically. So the timelines are very, very tight, in terms of your initial exposure to a field that is brand new. So that's interesting.

01-01:28:34

Telser: Yeah, right. No, that's true. The original, the first edition, was in 1944, and the edition that I got is the second, in 1947. And, economists knew nothing

about—it's not only that they knew nothing about game theory, they also knew nothing about other things that von Neumann had done earlier that turned out to be exceedingly important in business and economics. For example, the minimax theorem, which von Neumann published in 1928, is extremely important in the development of linear programming. And if you were to ask, what was the gain in the cost of producing and refining oil that resulted from linear programming? You're talking about billions and billions of dollars. It became possible to do things that were literally impossible before.

So these are huge advances, and then there's another result that von Neumann had, in a lecture that he gave at Princeton in 1932 on economic growth. He delivered the lecture at Princeton in German. Now, you have to appreciate the fact that, before Hitler, most American mathematicians studied in Germany, they published in German, and somebody could come to Princeton and deliver a lecture in German, and the audience would sit there and listen to it in German. Dickson, for example, his grandson, Harlow Higinbotham, Harlow Higinbotham is a grandson of Dickson. Dickson is from Texas. Dickson studied in Germany. He published his dissertation in German, and published articles in German, and you don't think of some guy from Texas doing all of this stuff in German in mathematics. And Dickson, by the way, was at the U of C. He was an algebraist.

All of this was really very new. All of these developments were really very new, and the results, the von Neumann article on economic growth, first delivered in 1932. Then the war intervened. It was published in English in *The Review of Economic Studies* in 1946, translated and published in English in 1946, and it had to have another article in English by Champernowne to explain, in more down-to-earth terms, what von Neumann was talking about and why it was important.

But see, this isn't game theory that von Neumann is talking about. This is a model of the economy, and it has a result that I don't think anybody had thought of. The result is really quite astonishing. What von Neumann showed was that the maximum rate of growth of the economy is equal to the minimum real rate of interest. Nobody had ever dreamt of this, as far as I know, before. So that's straight economics. You're not talking about game theory, but in order to get that result, he used the minimax theorem. And so Champernowne had to try and translate it so that the readers could understand what von Neumann was talking about. It's only a nine-page article.

01-01:33:43
Burnett: There's a story about a letter that Jacob Marschak—

01-01:33:50
Telser: Marschak.

01-01:33:50
Burnett: —yeah, wrote to Michael Polanyi where he's telling a story about von Neumann in 1926.

01-01:33:57
Telser: In 1926?

01-01:33:59
Burnett: I will check my records on this.

01-01:34:01
Telser: Gee, I didn't know that. Yeah?

01-01:34:04
Burnett: In Germany, and that Marschak was talking about supply and demand curves and equations. And von Neumann got exercised a little bit and got up from the table and started walking around the table, and saying almost to himself, "You must not speak of equations. You must talk about inequalities, that the seller will take at least a certain amount and the buyer will pay at most a certain amount." It's a folk story that gives you an inkling of how he was thinking differently.

01-01:34:50
Telser: Oh, yeah. That's amazing.

01-01:34:52
Burnett: Yeah, and I'll find the reference for you.[2] And that's really early.

01-01:34:58
Telser: As far as I know, but it's interesting, the first person to construct a model of the economy with inequalities was Abraham Wald, and that was in Vienna around 1937. But this was a seminar that von Neumann attended, so what I don't know is, to what extent Wald was influenced by von Neumann.

01-01:35:29
Burnett: I might be a decade off. It could've been '36, but we'll verify. [laughs] And if you think of transmission, if you think of scholars who end up at Chicago who

---

[2] letter, Marschak to Michael Polanyi, July 8, 1973 Michael Polanyi Papers. University of Chicago Special Collections. Cited in Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science.* (Cambridge: Cambridge University Press, 2002) 102.

"Yes, I remember those encounters in Berlin, 1926, most vividly. Of the participants, I cannot identify the Indian physicist. The others were yourself, Szilard, Wigner, Neumann. We were sitting at an oval table and I recall how v Neumann was thinking aloud while running around the table. And I remember the issue. I was talking, in the 'classical' Marshallian manner, of the supply and demand equations. Neumann got up and ran around the table, saying 'You can't mean this; you must mean inequalities, not equations. For a given amount of a good, the buyer will offer *at most* such-and-such a price, the seller will ask *at least* such-and-such price!' This was (later?) pointed out by another mathematician, Abraham Wald, perhaps in the 'Menger' seminar in Vienna, and certainly in 1940 in the USA."

had this contact with von Neumann, and von Neumann has this interest at Cowles, that there's this early transmission of knowledge right there in Chicago. And it's happening right there, when you're exposed in college to some of these ideas.

01-01:36:04

Telser: Well, now, there is another interesting thing that I only learned much later. I had a course on *Finite Dimensional Vector Spaces*, and it was taught by Paul Halmos, and it's published by Princeton. And you may have heard, these were paperbacks with orange covers. They had a notorious reputation, these orange-covered paperbacks. And so I had this course, and Halmos was also very interesting in the way he did it. When he would give us a problem, he wouldn't tell us, "Well, you solve this." What you were supposed to do is, either you prove it or you give a counterexample. You can't assume that he isn't fooling around, but the point is, Halmos was von Neumann's research assistant at the Institute of Advanced Study in the thirties, and this lecture, this book, is really based on work that von Neumann was doing, and as it turns out, it's the work he was doing for quantum mechanics.

Von Neumann, he did not have a very high opinion of quantum mechanics. He wrote a very famous book about it, and then, at one point, he said, "And then a miracle happens. You get a result. It's sort of arbitrary. It isn't a logical implication of the theory." But that's not the point. The point is that the influence of von Neumann, certainly, on me—and I was in that course in 1952, and Halmos, presumably, was a research assistant in the late thirties. So there is this relation. And the notion that you should use inequalities, and equations are wrong, that's very important, very important.

01-01:38:59

Burnett: And it's interesting. It's a fairly accessible way of understanding how he's thinking.

01-01:39:06

Telser: Now there's another thing that's also related to this and related to Albert. George Stigler wrote a very famous article, I think it was in 1946, on a minimum-cost diet. In my opinion, it's one of the most important things that George ever did. And what he did in that article was to take seriously the recommendations of the Department of Agriculture. They issued a statement about what people should eat, the minerals and the vitamins and all of that stuff, so that a good diet should have certain amounts of all of these things. So what Stigler did was to say, "Suppose that you tried to satisfy these requirements at the least possible cost."

Now, these requirements are inequalities, and he didn't know how to solve it. So he went to Albert. George told me this story and I'm sure it's correct. He went to Adrian Albert and he said, "How do you do this stuff?" And Albert said to him, "It's trivial. There's only a finite number of inequalities. Go away and don't bother me." [laughs] Now, I also had another experience directly

with A.A. Albert. Albert was the dean of physical sciences. My experience with him was: when we wanted to hire a mathematician, we had to get the blessing of somebody in the math department, so I was sent as the emissary to Albert, who was the dean. We wanted to hire Benoit Mandelbrot, who I knew pretty well.

So I went to Albert to get his blessing for Mandelbrot. So what Albert told me was, "Oh yes, Benoit Mandelbrot, I know his uncle very well, Shlomo Mandelbrot. You know? He's a descendent of the Vilna Gaon." That had absolutely nothing to do with the credentials. I mean, to be told that the uncle of this guy is a descendent of a very famous rabbi has absolutely nothing to do with whether we should hire him, but that was Adrian Albert. I didn't say anything to him. His son and I were pretty good friends. But anyway, this is the second example that Adrian Albert somehow misses the point. [laughter]

01-01:42:08

Burnett: There's another piece to the story at Roosevelt [University]. During the Great Depression, there're all kinds of efforts to figure out whether and how the government might plan its way out of the Great Depression. And given what was seen at the time as a business failure, the failure of the business community to invest, and restart the economy, that the government was going to have to step in, and how might it do that. So Keynesian economics comes to the United States. And there are his representatives in the United States, like Alvin Hansen, and there are other advocates, as well. There's also a Roosevelt connection there, isn't there?

01-01:43:12

Telser: Oh, yeah! Abba Lerner is an English economist originally, and he was a student at the London School of Economics, and he spent, I'm not exactly sure how long, but it was at least half a year and possibly a year at Cambridge as a pupil of Keynes. So he was one of the very first, you might say, disciples of Keynes, and, in the original exposition and dissemination of Keynesian economics, Lerner is very important. He wrote a number of very important articles. He also wrote another book on *The Economics of Control*, which has to do with a planned economy. But on the Keynesian stuff, he is very important and he was certainly a direct influence on me. And of course, I also had direct influence because I took Alvin Hansen's course at Harvard.

One of the general things, when you've been around a long time, as I have been, you ask yourself, well, I took this course and what do I remember of the course? And on a lot of these courses, if you ask me, "Well, what do you remember about this course?" I'm blank. I don't remember anything. But on some courses, I certainly do remember. And I had Alvin Hansen's course after I had had Lerner's courses at Roosevelt. This is in 1951, and Hansen was very bothered about the fact that we had come out of the war, there was a huge increase in savings, a huge increase in the money supply, price controls had

been removed by Truman, so why didn't prices go up? What's going on here? We don't understand this.

And what really impressed me about Hansen: Well, first of all, this is an important question, and I don't know the answer—and I don't know any economists that I would trust, or would appreciate, let's say, the answer that they would give me—but I think it's very important to have somebody say, "Well, here is a big problem, and we should think about it and figure out what's going on." Now, as a matter of fact, right now, as we speak, there is a huge increase in the money supply. I think it's four trillion dollars, and in the paper today, today's *New York Times*, there's some consternation that the latest price report shows that the increase in prices is much less than the Fed had expected. So the question that Hansen raised, here it is in 2017. They still don't really know what's going on. And I think, in general, that you have to think about serious questions, and try to figure out what's going on—not in terms of dogma or theology or anything, but in terms of what really is going on.

01-01:47:25
Burnett: So was he upset, in part, because, according to the dictates of Keynesian economics, prices should have risen, or economics across the board?

01-01:47:38
Telser: According to the quantity theory, prices should have risen. I don't know what Keynes would have said about this. Certainly, according to the standard theory, or according to Friedman, for example, prices should have risen, and they didn't, or not very much. There were some other stories that we heard. Here's one of the stories: Automakers didn't want to raise prices because they were afraid that this would bring the government down on their heads. So that, what they would prefer, is to let the dealers raise the price, because they're not going to start throwing 10,000 dealers in the slammer for raising prices, but they can certainly harass Ford, General Motors, and Chrysler. So that's one kind of story. You can believe it or not. It's a story; I wouldn't say it's a credible explanation.

01-01:48:58
Burnett: Yeah. What's clear is that it was an extremely unusual situation. The economy had been effectively taken over by the government, in the sense that market prices were suspended for the duration. It was a planned economy until '46, right? And then, I think, they had a planned release of the controls, but the economy had grown by a third, an extraordinary amount in that short period, and then there's the demobilization. So there are all these factors that are so unconventional, and so unprecedented, that I think that may be part of the story that makes it a case study worth revisiting.

01-01:49:53
Telser: Yeah. Well, I have to say, in fairness to Milton, he did say that trying to figure out the relation between output and the price level is a very important

problem, and he mentioned that many times. So I think, to be fair to Milton, he was not doctrinaire quantity theorist, and he was aware of the problem. The problem, I think, isn't with Milton. I think it's with a lot of the disciples, and that also applies to the disciples of Keynes. They're very uncomfortable with questions that they can't answer, and if they can't answer, I think they'll either give you stories or they'll evade them.

01-01:50:44

Burnett: [laughs] Well, what's clear [at the time] is that it's not a left or right thing; it's not a market socialism or a capitalist thing, the debate is within economics that is properly capitalist. There are concerns about whether the economy can be administered, not planned, centrally. There are organizations like the National Planning Association, the Committee for Economic Development, that are made up of scholars, business leaders, so on, who are trying to figure out: How can we plan for a market society? Which sounds like a contradiction in terms, but that gives you a sense of what's in play there, that the possibility of planning is in question at that time, and that seems to be the kind of governing impetus behind a lot of economics across the board. Is that a fair statement?

01-01:51:49

Telser: Yeah. The trouble is that certain slogans become popular, and as a result, they may distort understanding of what the real problems are. We need certain rules. The word "regulation" has become a pejorative word, so let's not talk about regulation, but what you do need is appropriate rules, and this is a very hard problem. You can see how hard a problem it is if you simply study the evolution of formally organized markets over a period of about 200 years and how the rules have varied and changed, and how they have dealt with the problems that came up. And some of them were good rules, and some of them were bad rules. But to say that "regulation is bad" is simply stupid. What you want to know, what you're trying to do, is to figure out what rules should be. But then, I think it's also important to recognize that there are circumstances when you have to rely on people's judgment, and no rules will work.

I can tell you one example of this. I wouldn't say I was close to Frank Knight, but we were on pretty good terms, and Frank Knight, as you may know, did not have a high opinion of Hayek, to say the least. He did not like the Austrian economics. And there are two occasions when, as it happened, I was walking with Knight and we were going to a lecture of Hayek. On one, it was just Knight, and on another, Ted Schultz was walking with us, and all the time we were walking, Knight was mumbling to himself. He had a tendency to mumble to himself when he was annoyed. He felt that Hayek is an important economist and it would be rude for him not to attend the lecture, so he had to go, so there was a certain element of coercion involved in this.

But what Knight really disliked about Hayek, more than anything, is a naïve belief in the rule of law, that if you have the rule of law, then everything is

going to work out. And that's just simply not true. I can think of a number of important examples where we avoided trouble because the person in charge had good judgment and knew what to do. The one leading example of this, which we'll probably talk about later, is Black Monday in 1987 when the market crashed, and I was on a task force looking at it. And Monday night, Alan Greenspan, who was the chairman of the Fed—now, the Fed is not allowed to make direct loans to non-banks. Their clientele is only the member banks in the Federal Reserve System. But according to what I knew on reliable terms, what Greenspan did is, he was on the phone that night calling around to the leading banks and telling them, "I don't want any clearinghouse to fail. I don't want any brokerage firm to go under. You lend to the clearinghouses whatever you need, and we're going to back you."

Now what he said, and the promise he made, well, let's put it this way: it was not legal. He did not have the authority to do this sort of thing, but I'm sure he would've done it. And whatever happened, on Tuesday, the market recovered, and we did not have the kind of meltdown on Tuesday that would've followed. And you have to understand that on Monday, the prices fell, I think it was 24 percent, and the only other time we had that kind of experience was in 1929, except it took two days in '29 to do it. On Black Monday, it happened in one day. You can't survive that sort of thing. You have to have people who have the guts and the wisdom to do what it takes. And to talk about rule of law is just rubbish.

01-01:57:40

Burnett: Well, judgment implies an ability to understand relationships among things beyond rigid prescriptions of "this is this, and this is that," and so that's something that you're suggesting is very important. Well, all of this is going to be in play as we move into the next session, where we're going to talk about your time at Harvard, and moving into the Chicago period. And, all of these questions about how economies are organized, how markets are organized, are going to be front and center.

Interview 2: July 11, 2017

02-00:00:15
Burnett: This is Paul Burnett interviewing Doctor Lester Telser for the Economist Life Stories Project of the Science, Technology, and Medicine Series, for the Oral History Center at the Bancroft Library. It's July 11, 2017, and this is our second session. And, we're now at the point where you are attending Roosevelt University for your bachelor's degree.

02-00:00:42
Telser: Except that, we wanted to start mentioning how I got into Roosevelt and why I started there. In high school, I was pretty ignorant of what the procedures are to get into a reasonably good college. But I had a high school teacher, I remember her, Miss Currie, and she was very smart about this, and so she sort of groomed me and did things that definitely helped me get a scholarship. So, for example, there was an honor society at Hyde Park High School. I didn't know anything about it; I had nothing to do with it. But she fixed it so that I would be not only a member of the honor society, but the vice president. So that looked good on the record.

Now, the other thing that was also very helpful—and in general, to make sure that I would be an interesting addition to the student body at Roosevelt—but of course, the other thing that helped was that I was very active in the newspaper. So I wasn't just an ordinary student that did all the standard stuff. It was more interesting because of all my experience in the newspaper, and as I mentioned last time, especially with Russell Bensley.

I should also say about Russell Bensley: He became a professional journalist. We were very close friends the whole time I was in college, and before that, of course, in high school. And Russell became the associate producer of CBS, Fred W. Friendly news program. He was not drafted. He was Four-F, but an unfortunate thing that happened is that his television team went into Khe Sanh in Vietnam, and he was hit by a mortar shell and very severely wounded. So that was, of course, in the seventies, something like twenty years or so later.

So Russell would have risen even higher, I suspect, in the journalism business, but then was put out of action as a result of severe wounds. We used to go sailing, Russell and I, on Lake Michigan. He had a Star, and we would sail almost weekly, weather permitting, from early summer, late spring, to pretty late fall. So we were out almost every weekend on Lake Michigan.

02-00:03:57
Burnett: So you remained friends with him. You were lifelong friends with him?

02-00:04:02
Telser: Yeah.

02-00:04:03
Burnett: So this is an advantage to staying, more or less, in the same city, and you've got these childhood friends that you've remained in touch with.

02-00:04:13
Telser: Yeah.

02-00:04:13
Burnett: Ah, that's fantastic. So being involved in extracurricular activities, they say today that that's a must if you want to get into college, but you had someone who was able to mentor you and give you advice about how to be the best candidate you could be.

02-00:04:31
Telser: Absolutely.

02-00:04:33
Burnett: And, what did she counsel you to do? As I understand it, money was tight for you, and so the focus was on a scholarship. You needed a scholarship. Is that right?

02-00:04:46
Telser: Right.

02-00:04:47
Burnett: And so, did you apply to a number of schools? What was the strategy?

02-00:04:52
Telser: No, actually, I only applied to Roosevelt. It didn't occur to me at that time that it would be a problem getting into college, and I was completely ignorant of the process. In order to get the scholarship, you were interviewed by a committee of the faculty and so on, and they actually did not offer very many scholarships, so I was fortunate in getting one.

02-00:05:30
Burnett: And there would have been pressure, too, because there were returning GIs.

02-00:05:34
Telser: Oh yeah, there were, absolutely. I would say that more than half of the students were veterans.

02-00:05:45
Burnett: What was that experience like?

02-00:05:48
Telser: Well, it was a very unusual group of students. They were older, and I was friends with some of them, and I remember one in particular. I think I mentioned in the other session that he had been in an infantry platoon from D-Day to the end of the war, and he said that, of his seventeen in the platoon, there were only three survivors, so that you got an idea that this was very

serious. And then of course, while I was in college—which is going a little bit ahead—on June 25, the Korean War started.

We should mention that there was a special program that was instituted, unlike in World War II, so that they made sure that they wouldn't sort of drain off the whole college population. There was, in fact, the problem that, at that time, the college population was relatively low because during the early years of the Great Depression, the birthrate was low, so the pool of candidates was lower. And to prevent the same kinds of unfortunate things that happened in World War II, there were tests given. To get a college deferment, you had to take a test, and then the draft board, on the basis of how well you did on the test, the draft board would decide whether or not you would get a deferment.

Now, for example—this is sort of going ahead a little bit: There was a special program in World War II for college students, and in 1944, when things got very hot, the program was stopped. And there were three from the U of C that I came to know very well—Manning Nash, Harry Roberts, and Morry Janowitz—and they were all three drafted and were in the Battle of the Bulge, and, were not really adequately trained to survive. Harry Roberts was taken prisoner and marched to Dresden. There's a famous novel about it called *Slaughterhouse Five*. So he is one of the prisoners that was marched to the prison facility, which was a slaughterhouse before it was used for other purposes. And Manning was a second lieutenant, and Morry Janowitz was a master sergeant. So these three guys were college students thrown into combat, and in our case, we had these exams, and I did okay, so I got a deferment. I was not only able to finish college, but I was also able then to attend graduate school, and I was not drafted until after I had completed my PhD, which was in the summer of 1956.

02-00:09:36

Burnett: Summer of '56, so you were just at the cusp of aging out, because doesn't the draft end at twenty-six?

02-00:09:43

Telser: Yeah, except that if you got a deferment, then the age limit did not apply, so you were still eligible to be drafted. I don't remember until what age, but that age limit did not apply in my case, because I had been deferred from 1951 on.

02-00:10:08

Burnett: Right, right. And so you were in this extraordinary circumstance of having so many veterans back and people who've been through a lot and have been around the world, and that adds a dimension to it. I think Al Harberger talked about the veterans returning, and that they were very serious. U of C, for example, is a serious place to begin with, but it added another layer of maturity and seriousness to the operation.

02-00:10:40

Telser: Yeah. It was not a "Joe College" type of thing at all. And of course, there was the background. The other important point to be made about Roosevelt [College] is, it had been founded as a memorial to FDR, and Eleanor Roosevelt was one of the major, I would say, sponsors of Roosevelt, and had a continuous interest in the progress. Now, the faculty at Roosevelt was a very high-level faculty of people who had not been able to get college jobs elsewhere because they were left wingers, black, Jewish, women. I guess it was 1947 is when they officially started, so you might say this was sort of the prototype of what we now consider to be okay, but at that time, it was definitely not okay.

And I had outstanding teachers, I think, precisely because of this. I could mention some. For example, one of them, one of the first teachers I had, had been a Rhodes Scholar. I remember him: Mr. Taylor, who was not at all well off. One of the things I remember about him is, he did not wear a sports jacket, but he wore a suit, and the jacket and the trousers didn't match, which was, of course, not what you're supposed to do if you're wearing a suit. But the main thing about him is the way he taught the course. He taught us how to write essays on particular subjects. So he would assign topics, or places to go to, where we would, for example, go to a lecture at the Adler Planetarium, and come back, and write an essay describing what was said in the lecture, and so on, things like that. So he was very good.

And then another one who had a major effect on me was Norman Rudy, who was a statistics instructor and a graduate student at the U of C under Jimmie Savage. And I was in his statistics course, and he gave a special lecture, I remember, on the minimax theorem of von Neumann. That was my introduction to the theory of games, and that had enormous effect on me in arousing my interest in economics. I also remember a philosophy teacher who was a black minister, Mister McGee, and he taught a course in philosophy and it was a terrific course. And the usual image, of course, is, well, if you're a black, you're a criminal or something; there's something wrong with you. And this guy was the polar opposite of the stereotype, and he made a very important impression me, and there were others.

The math teachers, they had outstanding math teachers. I had an outstanding course in symbolic logic, a new subject that I had never heard of before. I remember that was by, his name was Lionel Ruby. So that was a new experience for me, to learn how you could make logic into sort of a mathematical discipline. In my case, another thing that happened is, because I needed money, I had various jobs to grade papers, be a research assistant, and the faculty they were very helpful for me. For example, I worked for Sarah Landau, who was writing a book—I don't even remember on what topic—and so she hired me to type her manuscript, which was very helpful.

I had another interesting job. One of the students, apropos veterans, was blind. He had been a gunner on a B-17 and he was blind. So I was hired. The government provided for readers who would read the reading assignments to him, or to others like him, and I read the reading assignments. He was taking a course in American literature. I'm afraid it was more important for me to read the literature carefully than to pay much attention to what it was actually about. But of course, they were the famous American classics, so I was, you might say, exposed to and paid for reading the American classics [laughter] to this guy. Oh, and he had a seeing-eye dog, which the government paid for.

Then I graded the calculus course, so, and in fact Morrie DeGroot—one of the students in the course whose paper I graded—ended up eventually as the chairman of the statistics department at Carnegie Mellon. So we had some pretty good students at Roosevelt. Morrie DeGroot is a particularly interesting case because he helped a student from Taiwan who was in trouble with the Chiang Kai-shek regime in Taiwan at the time.

Oh, and Walter Weisskopf taught the "Economic Principles" course and "The History of Economic Thought." It was an excellent course. I learned basic economics very well. Now Weisskopf was a refugee from Vienna, and he had a problem because his brother, Victor Weisskopf, is a very famous physicist who became chairman of the Physics Department at MIT and then head of CERN in Europe, and his sister is a very famous psychiatrist who had been analyzed by Freud himself. So poor Walter Weisskopf was between these two famous siblings, so he had to live up to being really good as an economist, and he did.

02-00:18:25

Burnett: We spoke in your last session about your growing up in Chicago, in Hyde Park, close to the University of Chicago. You're surrounded by the children of professionals and academics, and, going to Roosevelt, you can see that there are literal refugees from other middle-class and upper-middle-class environments. So you're exposed to people who are enormously talented, both in your own family, in your neighborhood, and at this college, and that gave you, as a young man, windows onto these worlds. It must have been electrifying for you. Do you recall what was so interesting about the minimax theorem or the exposure to game theory? Was it that it was so unlike the math and the other formal work that you had looked at before?

02-00:19:24

Telser: Well, it was partly that, but I would say that the main surprise to me is that somebody would look at games and think about whether it would be possible to figure out the best way to play the game, to turn it into a mathematical problem. Now there had been attempts made. There are famous attempts of building automatic checkers players and chess players and things like that, but as far as I know, nobody before von Neumann had actually attempted to describe a general optimal way of playing a game. And in fact, it's also

interesting that—I'm trying to remember. The first paper that he published on the minimax theorem was in 1928, and at that time—I think he was born in 1903—I think he was twenty-five when that paper was published. And there was a very famous French mathematician, [Emile] Borel—I think it was Borel—and he said, "It's wrong." Although von Neumann would never get involved in controversies with people, he avoided it, and only later were there articles written by Jimmie Savage to defend von Neumann against Borel.

I think, in many countries, there was an attempt to think the scientists in your country are somehow in conflict with the scientists in another country with whom you had been at war, or rivalry or whatever. I would say, perhaps France more than Germany, this sort of attitude, I would say, kind of poisoned the atmosphere that would often exist. Now, for example, there was a French mathematician, [Henri] Poincaré, who had some kind of an animus toward Einstein, and wrote some articles criticizing Einstein that did not do any credit to Poincaré, despite the fact that he was a highly regarded mathematician. But you just don't do this sort of thing. Anyway, the minimax theorem, it was completely new that it would be possible to do this sort of thing.

02-00:22:39

Burnett: And so these instructors were introducing you to cutting-edge stuff. Game theory is brand new at this time, and it has this Chicago connection, as we'll see.

02-00:22:58

Telser: And of course, [Abba] Lerner, who was a very prominent economist and disciple of Keynes, also. I didn't know very much about macroeconomics, except in having read Lerner's articles and listening to his lectures. I was not aware of the hostility to the Keynesian theorems. But there is an important point I should mention about Lerner and Keynes. The most important thing that I learned from Lerner was not the macro models that were popularly taught. There was a famous article by J. R. Hicks—I think it's called "Mr. Keynes and the 'Classics'"[3]— and it has two curves in it called the IS and LM curves. Every student in economics had to learn about IS-LM curves, and the multiplier and what happens if you increase investment—income goes up and unemployment goes down—and so forth.

So this was sort of the standard way of teaching the Keynesian model, but what Lerner taught me was no, that's not the major contribution that Keynes made to economics. The major contribution is a chapter in the *General Theory* which I remember very clearly: "The Essential Properties of Interest and Money."[4] And that chapter, which is largely inspired by an economist, Piero

[3] J.R. Hicks, "Mr. Keynes and the 'Classics': A Suggested Interpretation" *Econometrica* 5:2 (April, 1937): 147-59.

[4] J.M. Keynes, *General Theory of Employment, Interest and Money*. London: Palgrave Macmillan, 1936.

Sraffa, who was a refugee from Italy, sort of a left-wing refugee, one of the most brilliant economists in the twentieth century, and Keynes was his patron. Every Saturday, Keynes, when he was in government, he would make sure that he would come down to Cambridge and spend the day with Sraffa visiting the bookstores in Cambridge.

However, anyway, in this article, it explains very clearly the portfolio theory of the demand for money, long before Friedman thought of it. So I learned about this before any exposure to Friedman and his sort of a recasting. But I think that Keynes did a better job of it.

02-00:26:10
Burnett: Well, this is in the air, isn't it? Because as we discussed last time, Keynesian economics was filtering into the United States rather slowly, and there were domestic economists, Gardiner Means, and business leaders who had an idea of centrally planned market economy. And the United States had been through a world war where there had been a planned economy, effectively, and it didn't fall to pieces immediately, and so, that emboldened a lot of economists to think, well, we can make this more or less permanent, and we can look to have a kind of market-socialist system. And then there are others who, very predictably, reacted against that. In some sense, that kind of summarizes what we're entering this period, that we're going to talk about in detail.

And so you majored then in economics and math at Roosevelt. And so, at that point, had you decided that you wanted to be an economist?

02-00:27:32
Telser: I think I did. I was very interested in math, but I didn't think I would be really good enough to be a good mathematician. I was pretty good, but not really. In math, it's like being a composer. There's Mozart, and there's everybody else; and in mathematics, it's sort of similar. There are half a dozen in the Mozart class, and there's everybody else. There's a very nice saying by a famous mathematician that kind of summarizes it, Kolmogorov, a Russian mathematician. And Kolmogorov said, "Every mathematician thinks that he's better than all the other mathematicians, but he's too smart to say it in public." [laughter]

02-00:28:36
Burnett: You felt it wasn't quite your scene.

02-00:28:43
Telser: Right. The mathematical end of it came through in statistics, and in fact, at one point, I think I was, in a way, kind of interested in becoming a statistician, but then I decided that really isn't very interesting. If you're a statistician, what you are really doing is helping other people; you're not really doing stuff on your own. And so I drifted away from statistics, but the economics became very important. The simple notion that motivated me, in living through the Great Depression, is very simple: How could things get so screwed up? It

bothered me that it seemed to me that it ought to be possible to figure out how to do things better, and so first you have to understand, how did things get so bad, and second, what you should try and do about it. And this seemed to me much more interesting than just doing math or statistics or something like that.

02-00:30:12
Burnett: And, I imagine you're just part of a whole generation, because that would seem to be the narrative, right? That you've—

02-00:30:20
Telser: Oh yes, definitely.

02-00:30:21
Burnett: —been through that and the Great Depression was a puzzle, too, because it wasn't a weak economy. It wasn't a particularly unstable economy. All of the basic fundamentals were there, and yet, it tanked, and there was this puzzle that had to be resolved and wrestled with. And so, that animates you as you're going through your undergraduate. And at a certain point, did you have a mentor who said, "Now you need to think about graduate school"? Or, were you animated at that point, you knew where you wanted to go and what you wanted to do?

02-00:31:00
Telser: Well, one person that influenced my choice to go to graduate school is a cousin, my first cousin, Lester Seligman. By the way, we are both named Lester after a maternal grandfather who died very young of cancer in 1907. And in fact, my mother always made sure to tell me that her father died when she was three years old, so that the date when he died was always very clearly impressed in my memory. But anyway, Lester Seligman was an instructor at the University of Chicago. He had a PhD in political science, and so this was another person who made me aware of the fact that there were academic disciplines apart from the formal sciences, chemistry and physics, or a profession like medicine; that it would be possible to become sort of an expert after you finished college.

I was pretty close to Lester. They lived, actually, only about a block away from us, and I visited very often, Lester and Judy, his wife. So we were pretty close, and he was the one who advised me about graduate schools, and of course, he knew about the U of C, and he had friends who had been graduate students at Harvard. So that seemed to be the two choices. So in fact, I did apply to both Harvard and Chicago, and Lester said that "you would get better faculty supervision at Chicago than at Harvard."

And I think that one of the influences—I shouldn't say "I think." I'm pretty sure that one of the influences that inclined me to Harvard was Lerner, because of Alvin Hansen. And certainly, there was nobody at Chicago who would [chuckles] give you the time of day as a Keynesian, whereas, at Harvard, there were at least people who were very friendly to Keynes. Of

course, there were others there who were extremely hostile, but there was a different atmosphere. So, I applied to both places, and the main factor that influenced me was, where would I get a fellowship? And the answer was: I got a fellowship at Harvard, but I did not get a fellowship at the U of C. So I went to Harvard.

02-00:34:35
Burnett: Well that's a smart economic choice, for sure.

02-00:34:38
Telser: Yeah. And by the way, and Al always said, but this was later, he said, "Always take the money." [laughter]

02-00:34:48
Burnett: Oh, this is Al Harberger. That's borne out of his own career, yeah. So you were thinking then about Keynesian economics because, did that promise the avoidance of another collapse, because you could manage aggregate demand, and thus forestall—so the causes of the Great Depression were then understood in those terms, and Keynesian economics was the way to manage the economy?

02-00:35:22
Telser: Leaping ahead. And then, for a long time, I wasn't really doing much in macro economics, and then there was a big shock, and that was Black Monday in 1987 when the market crashed. And one day, when the indexes fell, I think it was about 24 percent—and the last time that happened was, of course, in 1929, and that really stimulated, or revived, I should say, a very strong interest which continues to this day—this event, which I didn't think would happen, did happen, and that's another puzzle that we have to try and understand. Why do these things happen and what can we do about it? But that's way ahead of where we are here.

02-00:36:22
Burnett: So, you apply and you receive a fellowship. You must have done very well at Roosevelt. This is Harvard, after all. And so, you're admitted, and you pack your bags and move out there?

02-00:36:42
Telser: Yeah. Now the other thing that is also interesting: How did I get there? Well, I got there on a coach, by train, sitting up. So I was not in—

02-00:36:59
Burnett: A berth, yeah.

02-00:37:01
Telser: —a roomette or anything like that. So I sat up the whole time; got to, I guess it was Boston; the train stopped at Boston. You take the subway to Cambridge. I got off the subway, and at that point, when I got off the train, I did not have a place to stay. And here I am, I'm twenty years old, with my suitcase, and I go to the Cambridge Commons and I'm sitting on a park bench. And as it turns

out, the man sitting next to me, as I found out later, was Sumner Slichter, who is a very famous labor economist at Harvard. It was obvious to him that I was newly arrived, and I said, "I'm looking for a room." I didn't know who he was or anything. I wasn't in awe. I think we did talk a little bit so he knew where I came from and so forth, and I think he certainly knew about Lerner. And so he told me, "Well, why don't you go down this street?" And on Irving Street, I found a room. So the first semester I was at Harvard, I was in this room on, I think it was Forty-Six Irving Street.

Now one of the—that I was lucky about, is that Harvard had just finished constructing new dorms for graduate students. So a lot of these rooming houses that had been catering to graduate students had a lot of empty rooms, and you could get a good deal. So by sheer dumb luck, I stumbled into a good one. And as it turns out, the people who ran, or the owner of the rooming house, they were old New Englanders. I think they had come from England in the seventeenth century or something, and they were—let's see. What would you call them? In the Merchant Marine, they were professional sailors, and one of them, I think he was a second mate or something at that time, on a freighter. So I stayed there for the first semester. And you would not think, today, that an entering graduate student would do that sort of thing, that you would not think that one of these hovering—what do you call them? The parents.

02-00:39:54
Sylvia Telser: Helicopter.

02-00:39:56
Telser: Helicopter parents, would allow their kid to go under these conditions without having a place to stay, or anything like that. But there I was, and I was too naïve to think that it was even a problem. It seemed perfectly natural to me that you would do that. You go there, you find a room, and fine! [laughter]

02-00:40:24
Burnett: And I suppose there was a kind of *in loco parentis* attitude at colleges, but not at the graduate level so much. So, socially, how were you integrated when you arrived there? What were the graduate students like at Harvard?

02-00:40:45
Telser: Well, at Harvard, see, now this is my first experience with graduate work. Well, first of all, it was a pretty big graduate class. As I recall, it was on the order of sixty or so. So there were sixty graduate students, and I remember the first meeting was with the chairman, a guy named Arthur Smithies. I think he's an Australian. He did not make a good impression. Here we were, the entering class, and he comes in with this big, black dog and he's in the front of the room and we're sitting there, and it's sort of terrifying. Here is this chairman with his big, black dog, and we're sitting there as new students coming into this department. And I don't even remember what he was talking about, but all I remember is him and his big black dog.

Oh, and I should've mentioned, by the way, that one of the main reasons that I thought of Harvard in the first place—turned out to be disappointed—and that was when Schumpeter was there. The one economist at Harvard that I knew about before I arrived and was truly outstanding was Joseph Schumpeter. But Schumpeter died, I think it was about a couple of months after I had applied. So I was left, you might say, with the standard faculty. [laughs]

I don't want to make any derogatory comments, but I think, and I don't know, but Harvard has, of course, a big reputation, and it's had a big reputation for a long time. As a matter of fact, I even got an email today, because they consider me a member of the Harvard community. And you may have heard that the president of Harvard, after, I think it's twelve years or something, she's resigning, so they're looking for a replacement to be the president of Harvard. So they sent me the letter, not just to me, but to all the members of the community, to make suggestions about who should be the next president. I have not yet decided whether I'm going to put it into Trash or not. I suspect that I will.

Anyway, so, here we were. So now you could say, "Well who were their faculty?" The students were very impressive, and of course, there were a lot of veterans among the students, quite a few veterans among the students at Harvard, and I became close friends with some of them. In fact, one of them was not a graduate student in economics but in physics. I remember him. His name was Sunderland, and he was a lieutenant in the Royal Canadian Navy, and we became very close friends. One factor that influenced me is, he had a car. He had a Meteor, so we could drive around and see the sights before classes started officially, and then we would also meet fairly often afterwards. And I will not report what he said about the Physics Department at Harvard, but it would not be very dissimilar to what I would say [laughter] about the economics department.

Anyway, so I met these guys, and I became close friends with some of them. I remember, later on during the Christmas break, I went to New York with one of them. But now, coming back to the faculty, and the tests. Are there any courses that I took where I can remember what was said? And one, especially, and I think I mentioned this: Alvin Hansen made an impression. He posed a problem, and I think I already talked about it: Why was there this huge increase in the money supply, and no commensurate increase in the price level? And that bothered me, and that made a big impression on me.

I also had the price theory course with [Edward] Chamberlin, who was an author of a very famous book, *Theory of Monopolistic Competition*,[5] which was the chief rival to another famous book called *Imperfect Competition*[6] by

[5] Edward Chamberlin, *The Theory of Monopolistic Competition: A Reorientation of the Theory of Value,* Cambridge: Harvard University Press, 1933.
[6] Joan Robinson, *The Economics of Imperfect Competition*, London: Macmillan, 1933.

Joan Robinson, who was at Cambridge. And so the final in Chamberlin's course was the question that we called "the differences," and the question was: What are the differences between monopolistic and imperfect competition? And I presume I must have done pretty well. I think I got an A in the course, but about fifteen minutes after the final, if you asked me, "What were the differences?" I couldn't tell you, and I certainly can't tell you now what the differences are. [laughter]

But there was one thing that Chamberlin did in the course that did have an impression, and in fact, it had an even bigger impression on another student, Vernon Smith. And what Chamberlin did is, he had us all gather together in a big room, and each of us had a little card, and it either said "B" or "S": B for buy, S for selling a number. And if the number was a B, it meant that was the highest price you would be willing to pay, and if it was an S, it was the lowest price you would be willing to accept. And so you go around and you try and make a deal, and then you come back to me and you report the price.

And the message was that this is no way to run a market. And since Chamberlin was a proponent of the argument that the economy is no good, this experiment was supposed to demonstrate that, but most of us concluded it wasn't that the economy was no good, but that this is not the way a market runs. In the case of Vernon Smith, it led to his research on the structure and operation of real markets in the actual economy. So you could say that it had an effect, but the opposite, I suspect, of what Chamberlin had in mind.

Then another course I have to mention. It was a course in statistics by Guy Orcutt, who had been an officer in the US Navy, and the only thing I remember from that course is that the US torpedoes were terrible. They had shorter range than the Jap torpedoes, and they often wouldn't go off at all. And in fact, you could say it was because of these crappy torpedoes that John F. Kennedy survived to become president of the United States. He was in a PT boat, fired one of these crappy torpedoes at a Jap cruiser which blew the PT boat out of the water, and Kennedy swam, I think, two or three miles. Anyway, so I learned about torpedoes from Orcutt.

Oh, and [Wassily] Leontief taught a price theory course, and he was an interesting case. He apparently would not remember what he had talked about in the previous lectures. So the current lecture, half of it is what he said before, and then he would remember and then he would continue on. So what we got, in effect, was a half a course, [laughter] because he always started in the middle of what he had talked about, at the beginning of what he had talked about in the previous course, and never really finished a complete lecture in a single lecture. The best parts of the courses at Harvard were taught in the third hour by the instructors. They were usually very good. And in fact, one of them, Alan Manne, who was Chamberlin's instructor, was really very good, and turned out to have a distinguished career in economics.

Now Gottfried von Haberler taught a course in international trade theory. He was an anti-Keynesian, and I am now more sympathetic with the fact that he was very hard of hearing. I didn't appreciate it at the age of twenty. Anyway, you'd ask him a question. He was very tall, very bald. He would come over to you, stand over you, put his hand around his ear, and say, "Eh?" And then you would have to repeat the question in a loud voice. What did I learn about international trade theory from him? Very little.

02-00:51:10
Burnett: Okay. [laughter] And, it sounds like there's a lot of the new economics floating around Harvard at the time, and John Kenneth Galbraith was there, I suppose.

02-00:51:25
Telser: Yes, he was.

02-00:51:26
Burnett: And some more traditional economists.

02-00:51:29
Telser: Carl Kaysen. Yes.

02-00:51:32
Burnett: J. D. Black, a well-known ag-economist.

02-00:51:33
Telser: Right, in agricultural economics. Seymour Harris. There's a joke about Seymour Harris. He wrote a lot of anthologies, and Haberler's joke was, "There is no need for me to introduce Professor Harris, for those of you who have not read his books, have at least written them." [laughter]

02-00:52:00
Burnett: I hope he had a good sense of humor about that. So, I'm getting the impression that, overall, you were feeling like Harvard didn't agree with you.

02-00:52:14
Telser: For the kinds of things that I was interested in, they really had practically nothing. There was a certain part of the market they were catering to, but I wasn't in that market. Let's put it that way. So it was not a good fit.

02-00:52:44
Burnett: Right, right.

02-00:52:46
Telser: And then I mentioned in my email, Otto Eckstein and I spent a lot of time playing bridge. Otto became—well, he was one of the smartest guys among the students there.

02-00:53:04
Burnett: And so, you finish out the year, as it's your first year as a graduate student at Harvard, and at what point do you decide, well, it's the summer, so what are your options then?

02-00:53:20
Telser: There's another factor that entered into it. They did not renew my fellowship, and the deal was that I would be a teaching fellow. Well, if you're a teaching fellow, you're there. Now at Harvard, you only actually had to pay tuition for two years. So in effect, the deal was: I paid nothing in the first year. I would have to work for the second year, and then after that, to make a living, I would presumably have to be a research assistant. But I would not get any more. Well, I suppose, if I were a research assistant, I suppose I might get a little bit more than as a teaching fellow.

And the guy who had a huge horde of students working for him was Leontief, who was constructing these big input-output tables of the US economy, which, aside from collecting a lot of statistics and so on, I did not think was a very useful project. What is the point if you have these input-output tables? What would you do with them? And would you believe any of the results that you would get if you ran data through these input-output tables? Now maybe Leontief would believe it, and others, presumably, but I certainly wouldn't take it seriously.

02-00:55:11
Burnett: And so, do you remember how much tuition at Harvard costs in 1951?

02-00:55:17
Telser: I'm trying to remember. I think that my fellowship in the first year was $600, but I don't remember if it was a semester or for the year. The number that sticks in my head is $600, but I think it might have been for the year.

02-00:55:47
Burnett: Right, right. Even with inflation, that's still—

02-00:55:52
Telser: Well, let's see. So the minimum wage back then, I think it was about twenty-five cents an hour, so that's pretty high. It's a pretty stiff price.

02-00:56:10
Burnett: Yeah. So getting that waived—not waived, but getting a fellowship to cover that, would have been essential, in your case.

02-00:56:18
Telser: Oh, yeah.

02-00:56:18
Burnett: So, it was a nonstarter for you that the teaching assistantship did not seem interesting, and if it had been, you still would have contended with the fact that you weren't particularly engaged in what was happening there.

02-00:56:33
Telser: Yeah, right.

02-00:56:34
Burnett: Okay. Were you thinking then of quitting economics or quitting graduate school period?

02-00:56:40
Telser: Well, I don't think so, but what I was thinking, because I, of course, learned about economics—for example, one of the books that I read on my own, very carefully, from beginning to end, was Kenneth Arrow's book, the famous book that he wrote on the voting paradox [*Social Choice and Individual Values*, 1951]. And Ken had done this stuff, most of it, at Cowles, at Chicago. And I should mention that one of the teachers at Harvard, John Chipman, had been a post-doc at Chicago. He was the only mathematical economist at Harvard.

So I already knew that if I wanted this, if I was interested in this, that the place to go would be Chicago. And so, when I returned to Chicago, I did get a job as a research assistant to George Tolley. And the topic was interesting, because at that time, Ted Schultz, in particular, thought that agriculture was in difficulties, because, or as he put it, "We have agriculture in an unstable economy," so that he was interested in understanding more of the economic relations between agriculture and the rest of the economy. Agriculture was, of course, relatively more important then, by far, in terms of the percentage of national income, than it is now, but it was already on a downward trend in 1950 or so, or '52, agriculture was still a considerably smaller fraction of the whole economy than it had been in 1900.

I was working on this project that involved how much inventories were being held of agricultural commodities, and to what extent was it appropriate, relative to the demand for the product, and so on. Or, another way of saying it is, was this part of the economy responding appropriately to economic conditions? It's sort of on a piece with talking about what would lead to the Great Depression, or to explain what happened in the Great Depression. And then, somehow, I don't remember who suggested it, somebody, maybe it was Tolley, suggested that I should go and talk to Schultz, who was the chairman of the department.

Now there's one thing that I should mention that is very important that I hadn't mentioned before, and that is, at Harvard, it was almost unheard of—in fact, I would say it *was* unheard of, that a student would walk into the office of a professor and talk to them, or even to make an appointment to talk with the professor. The professors were up here, and the students were down there, the peons, and there was practically no relation between the two. And at Chicago, it's completely different. For me to have gone to talk to the chairman of economics, and that's this guy Smithies, at Harvard, it was inconceivable.

But [at Chicago] it was perfectly well understood, "well sure, you go and talk to Schultz and see what he has to say!"

And we spent a fair amount of time, it seems to me, something like an hour or so, talking. Of course, he did know a little bit about me, because I had applied in the previous year to be a graduate student. But anyway, we talked, and he persuaded me to transfer to Chicago. Then, he also, apparently, must've talked to Tjalling Koopmans, and at the Cowles Commission, they were looking for a research assistant for Houthakker, Henk [Hendrick Houthakker], who was starting his second year.

Houthakker had come to Chicago from Cambridge, Department of Applied Economics, in 1951 to do a study on organized futures markets, and the project, that particular project, the idea of it came from Jan Tinbergen. Anyway, he was the one that recommended this project to Houthakker, and it was supported by the Rockefeller Foundation. And, the previous research assistant had kind of bombed out, and so they were looking for somebody to replace him, and I was chosen.

And I remember getting a phone call from Koopmans asking me if I would be—well, what he asked me was to come in and talk to him about becoming a research assistant, which I did. And then, he, in effect, said, "You're hired." And it was a somewhat similar experience, by the way, that Al Harberger had before me. Al also was a research assistant at Cowles and also had the interview with Koopmans, and Koopmans would ask Al a couple of questions, and he asked me a couple of questions. I guess our answers were okay, and we were hired. And this put me in a pretty privileged position, relative to the other graduate students, because if you're a research assistant at Cowles in Chicago, that was a pretty coveted spot.

02-01:03:46

Burnett: Can I ask a point of clarification? You mentioned earlier that Schultz knew of you before because you had applied to be a graduate student, but when you applied in the summer of '52, you applied to be a research assistant—

02-01:04:09

Telser: Well, not really, no. In the summer of '52, I was really applying to be a graduate student. I was a research assistant, so it was sort of implicit that it would be good if I could be a research assistant, but I was simply applying to be a graduate student.

02-01:04:31

Burnett: Okay. This is probably during the school year at Harvard, you had decided you were going to apply to Chicago.

02-01:04:38

Telser: No, I applied to Chicago at Roosevelt.Yeah, no, no. It was while I was at Roosevelt that I applied to Chicago and Harvard, and so that information would have been at least a year old, more than a year old.

02-01:05:02

Burnett: Right, it was because they didn't have the fellowship to give you. Okay, now I understand. Okay, so, you had worked with Tolley and now you are working with Houthakker on commodities futures, but the key piece is that you're at the Cowles Commission now. Can you describe the types of research that were being undertaken, and the people that you met there in 1952?

02-01:05:41

Telser: Well, the first thing, and actually, I have been interviewed on this several times, on this particular topic several times before, and it's in several books. And the way I would put it, and the way I did put it, is that in physics, the leading research operation was in Copenhagen, and it was run by Niels Bohr. And so, it was the center of cutting-edge research in physics. And I would say that the Cowles Commission was the center of cutting-edge research in economics. Since the days when it started at Chicago, which I think was around 1938 or so, until it left in 1954, it was the unrivaled capital of research in economics. And even after when it went to Yale as the Cowles Foundation, it was nothing like the way it was at Chicago. And I was at Cowles at Yale, so I can make that comparison.

And, one of the things that happens is that almost all the leading economists would visit, sooner or later, Cowles. So I actually met practically everybody. If you were to name, let's say, the twenty-five or thirty leading economists in the world, I probably met every one of them while I was at Cowles. I wasn't the only one, of course. And then, there was a certain atmosphere. Once a week, every Wednesday, there was a luncheon, and at the luncheon, there were the Cowles people and the visitors. So you would meet all of these guys.

The workshop system, the seminar system, was at Cowles. At Harvard, for example, there was practically nothing. But at Chicago, the Cowles seminars, the agricultural economics seminar, the money workshop that Milton Friedman ran, and then, when Al came in 1953, his workshop, and then there was a labor workshop, but I wasn't involved in that. But there was something going on just about every day, a seminar or something, going on every day at Chicago. So you were exposed to all kinds of things.

Then of course, in addition to the visitors coming through, there were the permanent visitors, for example, Leo Törnqvist from Finland, who spent a year, and was, by the way, very interested in game theory. Offhand, I'm not sure, but the people who were there, the regular people, and those working on research projects that I got to know very well: Chris Winston, from Oxford; Martin Beckmann, who was a post-doc, brought here by Ted Schultz from maybe Heidelberg, one of the German universities. So Martin Beckmann,

Chris Winston, Bart McGuire. I think Bart was from Berkeley. I think he was an undergraduate at Berkeley.

02-01:09:47
Burnett: Roy Radner, was he there?

02-01:09:49
Telser: And Sigbert Prais from Cambridge, also Department of Applied Economics, and Roy Radner. Then we talk about veterans: Martin Beckmann was a veteran in the Wehrmacht, and Chris Winston, who is English and lived through the Blitz, he wouldn't speak to Martin. So that whenever there was necessary communication between Winston and Beckmann, I was the carrier, or somebody was the carrier, but I often was the carrier of the message. Martin Beckmann, he was lucky. He was very lucky. He learned how to run at Stalingrad, and he was wounded, so he was evacuated before the German army was surrounded and destroyed by the Russians. So Martin was a veteran of the Wehrmacht.

Chris, he was not in the armed forces in England, but he did do war work. I don't think he ever actually told me, so I don't know what he was doing. Bart McGuire, I think, was in the Air Force. Roy Radner was in the Army and he was in the Pacific when the war was over. Roy's job was to dig up the bodies and have them transported back for burial in the US, which I think is a sort of a—no comment. I think those were the ones that I knew best. And, Chris was a statistician, so I learned about statistics from Chris.

And Houthakker, he was an econometrician, and later, Houthakker won the [John Bates] Clark Medal for the work that he had done while he was at Cambridge, and some of the work he had done with Jim Tobin, and Jim Tobin was, I think, well, he was the head of the Cowles Foundation at Yale. And Jim, by the way, was offered the job as director of Cowles Commission, and I remember, there was a party at the Quadrangle Club. The one who was really hostile to having Tobin come to Chicago, I would say, was Milton Friedman. So he kind of killed it, and that's unfortunate, because I think Jim was a very, very good economist and a very nice person who helped me a lot later, but we'll talk about later.

There's one very important point, though, that I should make, and that is, when I came to Chicago, I was with a group of people who were really outstanding intellectuals. I had never had that experience. In fact, perhaps not even since, had I ever been in a group of such powerful intellects concentrated in one place as was true in the economics department at Chicago and at the Cowles Foundation. It was really quite astonishing. These were really very, very smart guys.

02-01:14:13
Burnett: Well, Cowles has an interesting history, doesn't it? And it's at this time that Cowles Commission is founded and it is supported by the Cowles family, basically?

02-01:14:31
Telser: Right, right. Well, mostly, Alfred Cowles.

02-01:14:33
Burnett: Yeah. And funding sort of dwindles near the end of the war, and it's on the ropes a bit for awhile, and then you enter at that moment. I think it's 1950—

02-01:14:49
Telser: I entered in '52.

02-01:14:51
Burnett: Yeah, but just prior to that, there's an injection of funding from RAND, right?

02-01:14:58
Telser: I think it was Ford, but I'm not sure.

02-01:15:01
Burnett: Okay. The historical sources that I've been looking at have been saying that it's RAND money, starts to come in as Koopmans takes over, right?

02-01:15:22
Telser: Yeah, no, yes.

02-01:15:24
Burnett: There's a context for a kind of transformation of the mission of the Cowles Commission it seems, because during the war, there's Oskar Lange. There's this search for a way to calculate a kind of market, to make market simulations, so that you could have a kind of centrally administered, market-socialist system. That is a major project at Cowles, to start with.

02-01:16:00
Telser: Yeah, I should have mentioned, and you reminded me, and that's important: Marschak's project was financed by ONR, the Office of Naval Research. And the project that Winston, McGuire, Beckman, and Koopmans were on was sponsored by the RAND Corporation, and that was a study of the railroad system of the United States. So, in both cases, I think it would be fair to say that the Cold War was an important factor, and the only really non-war related research was Houthakker and me. There were no implications about defense or anything in what we were doing. But all the other, most of the funds that came in, either were ONR or RAND.

02-01:17:15
Burnett: Right. And so that's basically operations research, right?

02-01:17:19
Telser: Right.

02-01:17:19
Burnett: Can you talk about what operations research meant in those days? What did the military want out of a bunch of economists or physicists? What was going on? What was the end game, or was there an end game?

02-01:17:38
Telser: Well, I think, on the railroad stuff, what they were trying to figure out is, what is an efficient way to run a transportation system? For example, to leap ahead, when I was at the Presidio Monterey, the Sixth Division was stationed there. And of course, Monterey is a harbor, and one of the exercises was how long would it take to move the Sixth Division to Germany, the whole thing: tanks, artillery, trucks, jeeps, everything, ammunition, everything. And I think it took three days. Those were the sorts of questions, and in order to do that sort of thing requires research in what, at the Cowles Commission, was called "activity analysis," but it's really linear programming. And one of the major inspirations, or purposes, of linear programming, was initially inspired by the Cold War.

So, for example, there is somebody we'll talk about a little bit later, Alex Orden, a mathematician who came to the business school [University of Chicago Graduate School of Business], and he had been involved earlier in linear programming for the Berlin Airlift. And Tjalling Koopmans, as I found out later, he had a very high opinion of him, and Alex actually did some important work in perfecting linear programming. So linear programming, which was one of the areas of research at Cowles, was certainly inspired—I wouldn't say inspired—*supported* by the military for its own purposes. Contrary, let us say, to the Henry Simons school where you hope that things will work out and all of that, by themselves, there was the view that, well, you really have to think about what you should do. You have to use rational thought and planning, and you have to understand things in order to get all of this stuff right.

Now it's interesting, for example, that in the economics department itself, there was a certain amount of hostility towards linear programming. It was considered non-economics, and Milton Friedman, for example, in the stuff that I did on "Safety First and Hedging," I was using quadratic programming, essentially, quadratic programming, to solve finance problems. And let's see, Harry Markowitz. I think I mentioned it in my talk in October, there was one of the students before me at Cowles and at Chicago who wrote about portfolio selection, and Milton said, "Well, that's not economics." It's hard to understand. There was this attitude that there are certain topics that you should consider to be economics, and there are other topics that's really operations research. So operations research deals with problems that are not economic problems, and I think that's wrong.

And then, to come ahead a little bit ahead of where we are, on my original dissertation committee, [Jacob] Marschak was one of my thesis supervisors

when I was working on my thesis proposal. And, what I was working on was inventory problems, which is very closely related to economic problems on the demand for money, and closely related to the chapter, let us say, in the *General Theory* that Lerner, "The Essential Properties of Interest and Money." For example, there was the notion that, for every commodity, there is an own-rate of interest, and how are these own-rates of interest related, and how does it depend on inventories and things like that? And Marschak was interested in that sort of thing.

And in my thesis seminar, I was describing what I was doing along those lines, and what I don't remember is what triggered an argument between Friedman and Marschak, but the argument had to do with the suitable way of studying inventory problems. I just stood there and the two of them battled it out. And the result was, well, I think what Marschak wanted was a more specific approach. Instead of looking at the problem in general, he wanted me to look at specific commodities, or specific applications, and not deal with the general problem. Milton wanted me to deal with the general problem, because the general problem would be more closely related to the demand for money; whereas, if you're looking at wheat or corn or whatever, there are special problems for these commodities that require a special treatment, and Marschak thought it would be a good idea to look at this so we could learn more about what's going on. But what Milton really wanted was the more general stuff for the demand for money.

02-01:25:08
Burnett: This was not just a regular argument. It came to a head, didn't it?

02-01:25:15
Telser: Yeah.

02-01:25:16
Burnett: And so, one or the other of them had to go, if they were going to continue to move forward.

02-01:25:21
Telser: Yeah.

02-01:25:22
Burnett: Right. We'll return to that in a moment, but one historian who wrote about Cowles and this period, in particular, I think the suggestion in that history was that Milton Friedman's opposition to Cowles, and others' opposition to it, may have been an association with the theory of socialist planning from Lange and others, that they were dreaming of planning. And operations research is, in some sense, about planning, and if you're going to do planning in a capitalist society in a Cold War context, the only context in which it's kind of allowed would be in military applications, right?

02-01:26:29
Telser: That's right.

02-01:26:30
Burnett: And that's the point that they make in this history. So, that may have been at play.

02-01:26:39
Telser: Yes. I think that's right.

02-01:26:42
Burnett: And so there emerges a bit of a schism between Chicago economics, as it's coalescing in its postwar form, and Cowles economics as it's emerging in its Cold War form.

02-01:27:01
Telser: And now there's another area of research that actually was not being pursued at Cowles when I was there, but was earlier, and that was in econometrics on general models of the economy, Keynesian models of the economy. So for example, one who was doing this is Carl Christ, who had left, had finished his research at Chicago, and I guess Carl was at Johns Hopkins. Anyway, so this was another source of dispute between Cowles and Friedman. I think this point, that planning and operations research and Keynes and all that, that we're really talking about various forms of socialism. It's a departure from a free-market economy.

And I think that one of the problems that comes up when you're in game theory, you are opening up the model so that you're allowing the kinds of relations that are not generally allowed in models of a free market economy. And again, to leap ahead, this is a factor that explains some of the difference between what are called non-cooperative games and cooperative games. Non-cooperative games are supposed to represent, more or less, free market competition, whereas cooperative games don't. They are supposed to be more, you're not competing; you're working together. Instead of saying "cooperating," you could use a bad word, like colluding, or conspiring, or something like that.

Now the thing that's very interesting about this is a person that I don't think I've really mentioned very much before, and that is Frank Knight. Knight wrote a very famous book, *Risk, Uncertainty, and Profit*, and in the first section of the book, or the first part of the book, he describes the essential logic of a free market economy. What do people assume when they're talking about a free market economy? What are the assumptions they make about a free market economy? And some of these are very critical assumptions, that there's perfect certainty and perfect knowledge. So, there's no uncertainty, and everybody knows everything. You don't have to search for prices because everybody knows all the prices. I'm not sure about this; I would have to check whether he goes so far as to assign, or say that the standard models assume constant returns to scale.

The basic idea is that you want a model where there are no monopoly returns, that all you have are competitive returns, no profits, no losses; everything is working smoothly. And there are some people who think that you can construct rules for an economy that will bring about this result, but I think Knight makes it fairly clear that that's nonsense, and that the purpose of explaining this logical structure is to point out why it doesn't work; why, when we're looking at the real world, there are certain changes that we have to make in the way we think about the real world that are departures from this very abstract model.

And then, one of the problems that you run into when you start following these lines, is that, for example, in geometry, when you draw a picture of a triangle, it's not to be taken seriously. It's not a real triangle. A true triangle doesn't actually exist in the real world. It only exists in the imagination of a mathematician. So then you get into this "as-if" stuff, and you say, "Well, we can draw a picture of a triangle because it'll help us deduce relations about true triangles and true this and true that," and it works, even though we know that, in reality, the things that we're looking at are different from this.

But then, then the way I think of it, is that you really shouldn't think of economics as a science in this sense, that economics should be thought of as engineering. And in engineering, you do have to take into account friction, and the fact that the triangles that you're working with are triangles in the real world. They're not perfect triangles in your imagination, and if you want things to work in the real world, you have to take this into account.

02-01:34:12

Burnett: To take that analogy further, are economists also engineers, in the sense that they, themselves, as economists, are in the world, suggesting rules, advising governments, and through their actions as advisors and as aides of leaders, are shaping economies and introducing things in new economies, and having an effect in that sense, as well?

02-01:34:40

Telser: Absolutely. Then there's another famous thing that Keynes said. He said, "Well, when the facts change, I change my advice. What do you do?" [laughter]

02-01:34:57

Burnett: Well, I want to come back to something that we were talking about before, with respect to Cowles and the friction between Friedman – and perhaps, you could say, Friedman's camp – and the Cowles folks. In one sense, there is this legacy of the search for a kind of market socialism and a mechanics of market socialism, but perhaps what was more thorny for Friedman and others was Koopmans's and others' search for general equilibrium. My understanding from the historians is that Friedman and the Department of Economics at Chicago was much more partial equilibrium/ Marshallian economics, taking that into the postwar era, whereas the Cowles Commission was looking for a

Walrasian general equilibrium by using the tools of game theory. How does that sit with you?

02-01:36:04

Telser: Yeah, no, that's true, in fact, there's an important person on this that I hadn't mentioned so far, Gérard Debreu, who I knew reasonably well. And Debreu was very much interested in general equilibrium, and it's also interesting that, in a lecture that Debreu gave, he began by describing the Knight description of the classical economic model. And then, he presented what he regarded as a mathematical model that reflected what Knight considered to be the essence of the classical economic model, and it's general equilibrium. And that was what Debreu focused on. And there were others besides, although they weren't directly in Cowles. For example, [Israel N.] Herstein, who is a mathematician, worked with Debreu on some of these questions, because they raised interesting questions for a mathematician on how all of these things fit together.

And it is certainly true that, at Chicago, among, let us say, the Friedman wing, there was hostility to a general equilibrium model. It's hard to justify or hard to explain what is the source of that hostility. Is it because they fear that a general equilibrium model, the purpose of it is to lead to socialism and a planned economy? If you're interested in economics as a science, and understanding how the whole economy works, then, it seems to me, you do have to consider a larger framework than just partial equilibrium.

There are certain topics that are considered outside the bounds in the classical, or, I wouldn't say in the classical model, but let's say, in the Friedman-Stigler model. They don't like to talk about externalities. When they talk about a commodity, they think of a commodity as something that you consume because of its utility to you. They don't think about a commodity that you consume because a lot of other people with whom you are friends, or whatever, consume it. It's sort of part of your group. I am not wearing knee pants, and I don't have a wig on my head, and I left my sword in the closet, and so on, and you are not dressed like a Roman senator, and so on. And these are pretty obvious things, but the standard economic models try to suppress it and avoid it.

And what I find interesting is that, actually, some of the classical economists, like Jeremy Bentham, going back to 1780 or so, they were very much aware of this sort of thing. Well, it's an interesting question to ask: why is it that this stuff dropped out, and the models got distorted? And it's very difficult. If you're a member of the profession, it's very difficult to try and reproduce these elements into the model without opprobrium. In game theory, where you're talking about coalitions and things like that, you are really forced to think about social relations and interactions.

02-01:40:53
Burnett: I guess it was [Freidrich] Hayek's line about the impossibility of knowing in its totality all the multitude of decisions [of individual agents]. It's not calculable, right? I think that's the general position of that camp. In Friedman's understanding, the market itself is an information processor, and that seems to be what takes over in the postwar era. I think one historian has put it, "The market is understood as a super-brain."

02-01:41:34
Telser: Yeah, whereas, for instance, that isn't the way I think of a market. There's another person I haven't mentioned so far, who was very important in this going back, and that's Leo Hurwicz. Now, Leo Hurwicz was the first person to write an article about game theory aimed at economists, and that article was actually reprinted in the *Readings in Theory* edited by [George] Stigler. I think it's Stigler and [Kenneth] Boulding. Hurwicz and I, by the way, were on very good terms, and, sort of a mutual admiration society. And Hurwicz has reservations about what prices can do. Under what conditions is it true that if you tell people prices, that they will make the right decisions? Is it always true? And the answer is no. There are lots of things that enter into the model besides just these monetary calculations, the way I think a lot of the economics was formulated, in order to make a convenient assumptions for certain mathematical methods that would simplify the problems.

Now there's nothing wrong to have simplified models, if you understand what they're doing and why you're doing it. Then, there's obvious examples that are used as debating points. When Galileo wanted to show that gravity is an important factor, and it's not different for a ten-pound weight than it is for a one-pound weight, the story goes, he goes to the top of the Leaning Tower of Pisa, he drops these two weights, and they hit the ground at the same time. He did not take a ten-pound weight and a feather. Although he knew goddamn well that they would not have hit the ground. [chuckles] Everybody knew that, but that's not the point. So you could say it was a simplified model. But there are circumstances when you really do have to take into account the difference between a feather and a cannonball.

02-01:44:30
Burnett: Right. Absolutely. And so, suffice it to say that this is one of the key portals of entry for game-theoretic concepts to enter the profession of economics. RAND is one; MIT is another, I suppose; and it's Cowles at Chicago at that time. So you are there when that work is being done. Did that shape you in your approach at the time, or were you focused on your project as it was emerging? Let's talk about your work there.

02-01:45:12
Telser: Well, I think there's an interesting fact, that after the argument between Friedman and Marschak, and I'm working on my dissertation, the issue doesn't really come up too much about methodology, and I wasn't really doing anything in mathematical economics. And then, when I came back out

of the Army to academia, I was doing work on advertising, and in doing this work, I had to develop some statistical models to deal with cross-sections of data—in other words, data where you have a sample of firms observed at intervals of time, the same firms at intervals of time. And I found a way of getting efficient estimates of the parameters in these models.

And I was presenting the paper at a meeting of the Econometrics Society—in fact, I think it was at Northwestern—and Al Madansky was in the audience. And in the process of doing this, I was using a certain kind of iteration, and then Madansky said to me, "Well, how do you know that it converges?" And you see, for a long time, for maybe six or seven years, from the time I was working on my dissertation and two years in the Army and so on, I hadn't really thought about this sort of thing. So he said, "Well, you have to prove that your method converges," and that is what really brought me full force back into mathematics. It was Al Madansky's question.

Up till that time, I would describe myself, unfortunately, as a faithful disciple of Milton Friedman, hostile to mathematical economics and general equilibrium. And Al Madansky's question set me, what some would say, on the wrong road of going back to game theory and mathematical economics. You can't just say, "Oh, well, I know this is true." You have to prove it, and to prove it, it takes work, and he started me off that way. I'm embarrassed to say, I wrote a discussion of a paper of some guy at North Carolina State—this is when I was still in this period—and I criticized him for using linear programming. "No, that's not economics." I would like to withdraw that from the record. [laughter] I know better.

02-01:48:46

Burnett: Right, right. You could be forgiven, I think, for being in such a heady atmosphere for so many different fundamental approaches, that, in some ways were in their infancy, because even in the case of Friedman-Stigler, it's not neoclassical economics; it's a revitalized neoclassical economics with new hypotheses, new research, new models, and the Cowles group is importing von Neumann's approach. Von Neumann actually had an influence, didn't he, because he was at RAND? He knew Koopmans and he was a mentor. He helped Koopmans, and he gave some of his initial talks about the von Neumann-Morgenstern book, *Theory of Games and Economic Behavior*, during the war, in 1943 or so, at Cowles. He was a force, wasn't he?

02-01:49:59

Telser: And actually, not only that, but—this is before my time—before he became sick, he was a consultant at Cowles. I think he came once a quarter, on the fourth floor, 401, I think it was—yeah, I think it was 401—and he would sit, and there would be a secretary sitting, and people would come in and tell him about the problems they couldn't solve. And von Neumann would sit there. And then, after a couple of minutes, he would dictate the solution and the secretary would write it down, and the next one would come in.

The point you have to keep in mind, when we talk about somebody who is a genius: von Neumann really is a genius. In fact, yesterday, the book I was looking at is by Stanisław Ulam. It's called *I Am a Mathematician*. It's a very good book. He's a very good writer, and among others, two of the people, I should say, that he describes at length, are Enrico Fermi and John von Neumann, and they are both geniuses. And it's very interesting to read Ulam's description. Now, for example, von Neumann had a prodigious memory. I never met him so I don't really know, but I'm only telling you what I have heard, that, a photographic memory. If he read, let's say, *David Copperfield*, you could sit there and he would just recite chapter one, chapter two, chapter three, et cetera. He's that kind of a guy.

02-01:52:11

Burnett: Completely eidetic memory, yeah, just photographic. And so, well, let's pause for now, and we'll start next day to talk in earnest about your dissertation project and the sequelae of that, the articles that you published as a result, and the story of how that unfolds.

Interview 3: July 12, 2017

03-00:00:18

Burnett: This is Paul Burnett interviewing Dr. Lester Telser for the Economist Life Stories Project, for the Science, Technology, and Medicine Series, for the Oral History Center at the University of California, Berkeley. And we're here in Hyde Park, Chicago, and it is July 12, 2016—2017. So, Doctor Telser, we are embarking on a discussion of your dissertation project at Chicago, and I'm wondering if you can pull together some of the threads of influence, intellectual influence, and the types of questions that interested you, and the types of thought leaders who inspired you in your work.

03-00:01:16

Telser: Well, first of all, of course, I started on thinking about the thesis topic because I was research assistant to Henk Houthakker, who was studying commodity futures. And what particularly intrigued me about it as a thesis topic is the idea that today, here are people thinking, or trading, in a market in terms of things that are going to happen in the future. It boggles the mind that you have people trading, let's say, wheat futures contracts in, let's say, October, that won't mature until July in the following year, and similarly for the other commodities they trade, cotton and so on. And it seemed incredible to me that there would be some kind of a rational operation or economic transaction that would take this into account.

Now of course, borrowing and lending depends on the future, and you're sort of more used to that sort of thing and you don't really think about the future aspects of it, at least not at the age of twenty-one. But the notion that there is this active interest in things that are going to happen later, I found astonishing. And of course, there were some other important aspects to the problem, and that is the question of whether the markets are functioning adequately, whether they are a source of stability or instability in an important sector of the economy. So there were policy aspects. And of course, one of the major policy aspects at that time was that the commodities were supported by a price support program that had started in the early 1930s under the Hoover administration. They started in about 1933, I believe, but it is not well known.

But it is, in fact, true that the price support program is actually before FDR. Herbert Hoover started the price support program in a different way, I believe, around 1930 or '31, and in fact, in Hoover's case, it directly used futures markets as an attempt to stabilize commodity prices. So it had a lot of interesting different angles to it. And of course, there was another. It's related to Keynesian economics. It was an important element in Keynes theories and thinking, so that was another aspect that influenced me, but all of these things put it together. But if I had to say what was the most important single factor, it was the notion that people would be trading today with an eye on events in the near future or fairly distant future.

03-00:05:22
Burnett: Well, as you mentioned, there's an elaborate set of government programs to manage the agricultural markets for certain commodities and not others, and there's a whole long history to that. And there's also, I guess, a question about the role of certain economic agents in these kinds of transactions, so, the role of speculators, for example. And, so can you talk about what the first steps were? Did this emerge from working on Houthakker's research project?

03-00:06:22
Telser: Well, the first step in the project was to get an idea about the numbers, the sorts of things that were actually going on in these markets. There was no readily available series of prices, and so one of the first things that we had to do was to put together price data on these commodity prices that we were interested in. And, for example, trading in cotton futures began in New York, I believe, around 1820 or the early nineteenth century. And on one wall of our office, which was in the Social Science Building, 403—was a pretty big office. In the office, by the way, there was Henk [Hendrik Houthakker], Chris Winston, and me. So there were three people in the office, and the wall was about, I would say, thirty to forty feet long, and we had, on that wall, a graph showing cotton prices in New York, I think monthly, from 1820 or so to about 1950.

It was a laborious job to put all of that together, and I remember that, occasionally, Milton Friedman would come in just to look at the chart. And you have to remember that, in those days, we did not have personal computers. The computers were pretty primitive. Graphing was a big job, and so we almost had a monopoly on the visible availability of cotton prices. Well, that was one part of the problem. Then, of course, another part of the problem, in addition to finding out about prices, is to have data on the production of the commodity, the size of the harvest, consumption, and so on, in the case of wheat, and also, in the case of cotton. There are a lot of complicated characteristics for an individual commodity. How is it graded? What are the delivery points? Who does the trading? Who uses the cotton? How is it used? Who grows it? And so forth. So, all of this involved not just looking at the futures prices alone, but at looking at the whole commodity from planting to, let us say, the final output: sheets, towels, shirts, and whatnot.

03-00:09:40
Burnett: Wow. That sounds like a tremendous job. And who physically made the graphs? Did you make the graphs?

03-00:09:47
Telser: Well actually, the graphs themselves were made by the research assistant who was my predecessor. So those long graphs were not my production, but what I did get involved in later were certain parts of it that dealt with certain aspects of what was going on in a commodity. Well, the direct issue that came up was: how much were the futures markets influenced by the government programs? How did the effects show up on these prices? And so on. So my

involvement, really direct involvement, really began to start with looking at how the actual government programs affected what was going on in these markets.

The way these programs operated was that the government did not actually buy the commodity from the farmer. What the government did is "make a loan" to the farmer at a stipulated price, and the collateral for the loan was the commodity. But what would always happen, and it was understood it would always happen, is that the loan, in a sense, would be defaulted, and the government would take ownership of the collateral, so that it was a device that was used to pacify certain prejudices of politicians to make it look like a legitimate commercial enterprise, rather than as an outright subsidy of the government to the farmers. Although, other things began to enter into the programs later, for example, that farmers could get a payment if they withdrew acreage from production, and so on. And that sort of thing would attract more criticism for certain newspapers and politicians, but that didn't really start until later.

The other part of it was the fact that really it was officially a loan program, and one of the interesting aspects to it is that the government did not make it easy to find out the size of their inventories and exactly what was going on. In effect, it was public information, and there was a certain amount of freedom of information, but a freedom of information did not imply a zero cost of getting it. So that was an important factor. And then, it's also interesting to go a little bit ahead. I started working on the dissertation, I believe it was in 1953, and it was completed by the summer of 1956. And in the first part of the project, it was part of Houthakker's research project, but in the later part, when the Cowles Commission left the University of Chicago in June of 1954, I was literally without support.

And at that point, Ted Schultz stepped in and was extremely important [in advocating] on my behalf, so that I would become a cooperative agent of the US Department of Agriculture, and that had the considerable advantage that it made it much easier for me to get data that was not readily available publicly. So I had data on the size of the loan programs and what the CCC—it was called Commodity Credit Corporation—what the CCC was doing, how much they had lent, the size of their inventories, monthly, and all of that I was able to obtain because I was an official employee of the Department of Agriculture. So it was much easier. And it was very important the conditions that Schultz laid out, or required of the Department of Agriculture before he would accept this arrangement as a cooperative agent. They could comment on the dissertation, and they could criticize it, but they were not allowed to prevent publication or to hinder the research in any way whatsoever. So they could talk, I could listen, but I didn't have to pay attention to what they were saying if I didn't like what they were saying.

03-00:16:01
Burnett: I'm sure it was public knowledge among the graduate students why Theodore Schultz would have wanted that kind of protection.

03-00:16:12
Telser: You're alluding to what happened at Iowa State earlier.

03-00:16:17
Burnett: Yeah.

03-00:16:17
Telser: Yeah. I can tell you a funny little thing about that. When I started teaching at Iowa State, Ted came one day to deliver a lecture. And people are very friendly in these small towns, and a next-door neighbor had come over to chat with me, and I said, "I'm sorry, I have to go now. I'm going to hear a lecture by Professor Schultz." And he said, "Oh," he said, "that's a familiar name. Whatever became of him?" And I don't know to this day if he was serious or not. [laughter]

03-00:16:58
Burnett: So yeah, he resigned from Iowa State under some pressure to change the conclusions of some of his research assistants for a pamphlet that was being prepared for the USDA, and he resigned as a matter of principle because he did not want anyone to put political influence on, to change the results of what he regarded as scientific research, which needed to be independent.

03-00:17:31
Telser: And it was very important in my case. It was very clear what the limits were. The limits were almost, for the USDA, insurmountable. And then there was another interesting element here, and that is my access to people at the department. How should I put it? Each commodity had a USDA expert who kept careful watch over what was going on, and, there would be a weekly, I think it was a weekly publication: the *Wheat Situation*. So one person put out the *Wheat Situation* every week, and described what was going on in the international wheat markets as well as in the US. So there's the *Wheat Situation*, *Cotton Situation*, and so on. So I knew all of those people, and then there was another person who was in charge of the watching the commodity markets, I remember: L. D. Howell.

So I would meet all these people, and then, in one case, the major benefit was via Earl Hamilton, who is a very famous economic historian. And it turns out that Earl Hamilton and W. Edwards Beach were fellow students at Harvard. So this, by the way, shows what you might call, I don't know, the elite influence or something. W. Edwards Beach was the head of the Commodity Exchange Authority, which was the government agency that regulated futures markets. And it was not usual for a twenty-two-year-old graduate student to have an appointment and spend a couple of hours talking to W. Edwards Beach. But, because I was at the U of C and Earl Hamilton recommended me, maybe because I had been at Harvard or whatever—anyway, so I did spend

time with Beach, and this is a separate point: he was a chain smoker. And one of the things that distracted me is, I would watch the cigarette as the ash would get longer and longer, and as soon as it fell off, he would stub it out, and put another cigarette in his mouth and start smoking again. And, I should add, he eventually did die of lung cancer.

03-00:20:37
Burnett: Wow. And, these officials, the commodity folks, they're at the USDA, and were they based out of Washington, or were they local?

03-00:20:48
Telser: All of these officials were in Washington, but they were all in contact with the experiment stations in the state colleges throughout the country. And of course, in the Commodity Exchange Authority, they had direct supervision over the futures markets in Chicago and New York and elsewhere, wherever there was trading in these things.

03-00:21:18
Burnett: So you were kind of doing some anthropological research. You were learning the culture, the practices, of the federal government, and its efforts to know and manage the farm problem.

03-00:21:35
Telser: I would say it distinguishes me from some of my colleagues who have had no direct experience with what happens in government, and government agencies. And they have a tendency to denigrate government agencies, but it's not based on any direct evidence. It's just a prejudice. I learned too much about what actually goes on to share those views.

03-00:22:19
Burnett: So you have more respect for the government officials in charge of extremely complex economic processes, and regulations, on top of that.

03-00:22:29
Telser: Yeah. And in fact, in one of my books, instead of going into this sort of thing, the example I chose was the Manhattan Project, where everybody, if they know anything about what was going on, they would have to understand that this was a government-run project almost without precedence. I shouldn't say almost, but without precedence in world history. And it was very successful.

03-00:23:01
Burnett: Do you think that was the influence of Schultz—I don't know if it's his background in ag economics—that there had to be a really practical, and almost like a practicum to learning about any economic problem? You had to be in the trenches, a little bit, at least.

03-00:23:23
Telser: Oh yeah, yeah. I think there were several people—he was one of them, and then later, in the business school, Allen Wallis and Jim Lorie were others—I won't say they were insistent, but they certainly encouraged me to be involved

with economics in the real world, to meet people, to meet business people and others so that I would learn how things actually worked. And this was certainly true in a lot of the consulting projects and more practical kinds of things, or whatever you want to call it, that I did in the course of my career. So I did have a lot of exposure to the real world—I don't want to mention any names, but in contrast to some of my colleagues, who don't really know anything about what's going on. Of course, Al Harberger is an example of someone that's very knowledgeable about the real world.

03-00:24:38

Burnett: With also a commensurate respect for those who have to deal with the day-to-day challenges of operating, government policy or operating a business concern, or operating a financial operation. He would have a tremendous sympathy for the struggles that they faced and would try to help them. I suppose you could also say that Milton Friedman and Allen Wallis came out of the Statistical Research Group at Columbia, and they were involved in war work that had very practical applications. It had the opposite effect for Milton Friedman. He became very disappointed with government agencies, I think, as a result.

03-00:25:24

Telser: Well, he's a very complex person, very hard to understand. Now I should add, in addition to the work, let me put it this way. There was a very close relation to what my dissertation, which had to do with the relation between inventories, the stocks held of the commodities, and futures prices, and what we call the demand for money, like the demand for a product. So this was one of the major reasons, I think, for Milton's interest in the dissertation, because it was very closely related to his major interest, which is in monetary economics. He's very quick, very smart. He was very helpful, but then he would also have very strong views on certain things. And there were occasions when there would be conflict for reasons that I really couldn't understand, and I still don't understand, for example, his hostility to the work that I did on hedging, the attempt to understand hedging. I think I should say something about it, and you raised that issue a little while ago.

There are essentially two kinds of people trading in a market, in a certain sense: the hedgers and the speculators; but then you could also categorize them in other ways: professionals and amateurs. If you're a tourist and you want to visit the Chicago Board of Trade, it's a dog and pony show. So there's a standard thing that you would get. So you go there with a group of tourists, and there's somebody there who's explaining how the markets work: and there are no bad people in the market. "All the traders are good. The hedgers, of course, are good because they're buying price insurance, and the speculators are also good because they're the ones who are selling price insurance. So the whole function of the market is to eliminate or reduce risk for the legitimate purposes of the economy, namely, food and clothing, and

what could be better than that?" And so there's no real understanding of what actually happens in a market.

Now, the big guru on futures trading, especially on wheat futures, is Holbrook Working. He was at the Food Research Institute at Stanford. How should I put it? Our swords crossed on several occasions, and we did not agree on many aspects of futures trading. But it is to his credit that Working was very much aware of the fact that what goes on in the futures market does not really coincide with the canned story. And the first thing to understand is that when you are trading in futures—and this goes back to my first interest in the problem—there are people who are trading on price differences, the difference between, let's say, the May contract and a July contract. And that really doesn't have anything to do, or not really—it isn't useful to think of it in terms of price insurance. It's more useful to think of it in terms of a kind of a gamble or speculation that you would make, because the variability of the spread is considerably smaller than the variability in the prices themselves.

Now that doesn't mean that speculating in a spread is safer than speculating on the level of the price, because what happens is that, if the variability is lower, then the leverage is higher. The leverage means the amount of money that you have to put up, in order to buy and sell futures and different maturity dates, is a much smaller fraction of the total value of the contract than it would be if you were doing a straight speculation on the price. So there are people who are specialists in spreading.

Now, the ones who buy plain futures prices, who are supposedly the ones selling the insurance to the buyers, are typically the least informed traders in a market. And in my view of it, the professionals live off of them, because you have a constant turnover of the dentists, the doctors, the lawyers, et cetera, who think they're smart, and they go in, they take an open position in the contracts, they lose money, and they get wiped out or their wives stop them. I mean, "Enough already. We've already lost a hundred thousand dollars; how long is this supposed to go on?" So they come out, but then new ones come in, so the supply is always replenished.

And this is a way of looking at the market that is very useful. And one of my initial prejudices against the standard view, which is based on a modest amount of exposure to the way real markets actually work, is, if you are a nervous type, the last thing that I would recommend that you do is take open positions in any commodity future, because you'll either get ulcers or a heart attack. And so the guys who are doing this, the professionals, they're a pretty hardy type. And they are not doing this on the basis of some kind of a social service or whatever that people sometimes think of; they are doing it on the basis of what they think they know that the other people in the market don't know. And the hedgers, the ones who are the shorts in these markets, they typically do better.

Now one of the things, going back to what I was doing at Cowles—so we circled around; I think we're coming back to where we started. One of the questions that we were interested in is the gains and losses of the traders. If the standard theory is right, then what you should see is, the hedgers lose and the speculators gain. Well, the fact is that that is not what you see when you look at the data, and in order to look at the data correctly, what you really need to have are the accounts at the brokerage firms. And of course, they are not readily available. They're not going to tell you how their traders are doing.

However, in the early thirties, there were studies made of the records of actual brokerage firms, and some of those results were published, and I used them in my dissertation. And Henk tended to be critical of them, because he said, "Well, the reason that the government got these accounts is because the brokerage firms failed, and the speculators turned out to lose money in those records because they were taking the advice of the firms." I think that's a lot of nonsense, and there's nothing substantive to really support it, but that was his way of trying to rescue the standard theory. The facts show that it is the professionals, the people who really know what's going on, who do better than the amateurs who don't really know what's going on.

And there's another implication of this argument that's important, and that is, if the standard theory is correct, it implies that there would be a trend in futures prices. So the way a speculator, who's on the long side of the market—that means he bought the contract—the way he would make money would be if the price later is higher than the price at the time he acquired the contract. So there should be an upward trend in futures prices, and one of the main things that I did in my dissertation was to see if, in fact, there was such a trend, and the answer is no. Whatever "trend" there is in futures prices is simply reflecting what's going on in the economy as a whole. So if there's a recession, prices are going down, and if there's a boom, prices are going up, and that's got nothing to do with anybody selling price insurance to hedgers.

03-00:37:11
Burnett: So in your analysis, you had to control for the changes in prices in the commodities market by comparing it with the general price level.

03-00:37:24
Telser: Yeah, in what I would say is a relatively crude way, but it was enough to show that if you're looking for a trend in futures prices, you're wasting your time.

03-00:37:38
Burnett: So I just wanted to ask about [a few things]. There are four articles that come out of this dissertation in the late fifties, and one of them is about the conventional wisdom that comes out of Keynes' and J. R. Hicks's theory and research, and that's the claim that commodities prices rise as they approach maturity. Is that part of that conventional wisdom? That is it?

03-00:38:18
Telser: Yes. I didn't mention it, but that's what I call, and others, the Keynes-Hicks hypothesis, about futures trading to support the view that the speculators are selling price insurance. And the only way they can do this, on average, at a profit, is if there is this trend in futures prices, upwards trend in futures prices. So it's the Keynes-Hicks version of this theory.

03-00:38:52
Burnett: And if it were true, according to your argument, then you would expect speculators would be making more money, on average, than hedgers.

03-00:38:59
Telser: Or at least not losing money.

03-00:39:01
Burnett: Right, right. I think you mention it in one article, but it's most clearly stated at the beginning of an article by your colleague, Harold Demsetz, who—

03-00:39:21
Telser: By who?

03-00:39:22
Burnett: Demsetz, who says there's a longstanding folk wisdom that speculation is taking advantage, so there's a moral opprobrium associated with speculation and that that colors some economic research. Is that what you found? Do you agree with that position?

03-00:39:51
Telser: Well, I think that what Harold says about the views of speculators is correct, that they are held in opprobrium. And this brings me to another article. In fact, I wrote this article when I was in the Army, and that is, does profitable speculation stabilize prices? And this also, by the way, it gives support to Milton Friedman. Now the reason for believing that profitable speculation stabilizes prices is actually very simple. If you are a speculator, let's say, on the long side, the only way you can make money is if the price at which you buy is lower than the price at which you sell. And if you're a speculator on the short side, the only way you can make money is if the price at which you sell goes down after the time you bought it. So in either case, what happens is that the price sort of goes in the opposite direction to the position that you initially took.

So if it goes in the opposite direction to the position that you initially took, with a very modest amount of common sense, it becomes obvious that profitable speculation—meaning that you always, on the average, that you came out ahead—the effect of your actions must have been to stabilize the price. And now, there was an article that Will Baumol wrote, that was published in *The Review of Economics and Statistics*, in which he claimed that speculators destabilized the price. Now, aside from the simple logic of what I just described, there was an unfortunate error in Baumol's article. He had a

differential equation in it which he did not solve correctly, and in fact, the differential equation is what we call, in mathematics, unstable. That means it would go off into the stratosphere, and in economic terms what it means is, you would never actually be able to close out your position.

So, yes, you would be destabilizing the price according to his model, and the price would just keep on going up, but you would never be able to sell your contract. It would just go on up, indefinitely. So in my article that appeared in *The RES*, I pointed out this error. Now what happens when you do that sort of thing is, you earn a lifelong enemy, and that is what [laughs] happened in this case. I remember, actually, later, we were both at a conference at Stanford. I remember we were at a swimming pool. He was barely polite to me. Baumol was at Princeton, but our relations were not friendly thereafter. [laughter]

03-00:43:51

Burnett: Well, you mentioned something important, and you state that it's obvious that the market is kind of auto-equilibrating, almost like a self-regulating system, because there are people who buy and there are people who sell, and it all kind of balances out, right? But that's partly what was at stake after World War II, wasn't it? In the sense that there was a consensus that the market doesn't work all by itself, that it requires intervention. And so, go back to 1916, you have the Grain Futures Trading Act where it outlaws certain kinds of trades, and you can't trade by telephone, and all this kind of stuff. So there are all kinds of regulations precisely because people argued and convinced government officials to regulate and manage what was considered to be an unstable market.

03-00:44:56

Telser: And this was actually particularly prominent, I believe, to this day, in Europe. It is very, very difficult for somebody in a western European country to trade in a commodity market. It's almost impossible. It's much easier in the US, and there is this sort of paternalistic attitude in Europe to prevent it. Now the idea behind these regulations is that, I think there are two elements to it. One is the belief that most people are dumb and you have to prevent them from doing dumb things, and the other part of it is that there are a lot of crooks, and so you have to prevent the crooks from taking advantage of these poor, innocent people.

So the purpose of the regulations is to hamper the crooks and to prevent the sheep from being slaughtered. In describing these amateur speculators, I said that many of them do not do well, and unless they are very foolish or very wealthy, somehow they pay for the lessons that they learn. And so the question is, given that this sort of thing, in terms of the previous argument that I gave, tends to destabilize the price, and so you have another group who make money by offsetting their effects — is it better to leave it to them, or should the government intervene directly as sort of the helicopter parent, to

use the term yesterday, [laughter] to keep these poor kids from being screwed?

03-00:47:18
Burnett: Well, I guess another aspect of it, with respect to commodities, is that an important justification for price-support programs was that they stabilized prices. And you had something to say about that, too, didn't you?

03-00:47:35
Telser: Well, part of the problem in that case was that some people who studied it thought that the government did not succeed in stabilizing prices, and in my article, what I pointed out is that they didn't do the test correctly. The fact is that the government price-support programs actually did stabilize prices. And there is a problem, especially back then, because some of the farmers who would benefit the most from stabilized prices were not able to do so. It was very difficult. In fact, farmers, in general, typically were not able to sell futures contracts the way a merchant could sell futures contracts, and this is for a number of reasons. First of all, if your crop is growing, you don't really know what quantity you should sell. You don't know how it's going to turn out, what the yield is going to be. So that's one difficulty.

The second difficulty is that typically, in those days, a farm was relatively small. In Iowa, for example, a rather successful corn farm would be about 300 acres, and in terms of these contracts and the way these markets operate, that's sort of peanuts. The way it would have to work would be if the merchants to whom they would sell their stuff, if there was enough competition, if something is going on in a market that would enable the farmers to take advantage of the competition among the merchants, so that they could obtain some of the benefits of an organized market. And it would be costly to do that. So that, basically, the way I would interpret these government programs is that they were trying to supply a service to farmers at a lower cost than the free market was able to perform.

03-00:50:20
Burnett: I think that was the idea, yeah.

03-00:50:22
Telser: Yeah. I think that was pretty much Henry Wallace's idea. I should mention, by the way, that Henry Wallace, and T. W., as far as I could see, were on pretty good terms if not friends, and Zvi Griliches, whose name I don't think has come up so far, was doing his dissertation on hybrid corn, and would give seminars at the ag workshop. Well, this was, yeah, in the middle fifties. One day, Henry Wallace came in. One of the things that was impressive, that here is a guy, I must say, with charisma. As soon as he walks into the room, you know it. He was really a consummate politician. I was enormously impressed by Henry Wallace.

And of course, he is one of the major geneticists or agronomists in the development of hybrid corn. So, the main purpose that he served was to tell Zvi a lot about what was going on in the development of hybrid corn. It's also interesting to note, by the way, that most of the important research for hybrid corn was not done in experiment stations in either Iowa or Illinois, the major producing states, but in Connecticut, as I recall. I may be wrong, but that's my impression. Oh, and another thing I remember that Henry Wallace said, that "in the fairs, the emphasis on which corn farmer won was not based on the yield of his crop but about how good-looking the ears were." [laughter]

03-00:52:23
Burnett: Yeah, that's true. They had beautiful family contests, too, so that extended to people and hogs and all kinds of things. [laughter]

03-00:52:33
Telser: Yeah, it's all very nice.

03-00:52:35
Burnett: Schultz, I think one of the things he got out of that research was perhaps an initial spark to human capital research. He was an ardent defender of the state college system in providing innovation and extending that innovation to farmers. It was an engine of scientific research and the dissemination thereof to people who could use it, and private innovation notwithstanding; there's certainly that, as well.

03-00:53:09
Telser: Yeah, the whole idea of human capital owes a great deal to T. W.

03-00:53:14
Burnett: Oh certainly. And, I think I have one more question about that, and it also involves your primary chair, which is D. Gale Johnson, who was a student of Theodore Schultz's and came with him to Chicago. In the forties during the war, Schultz was interested in this concept of forward prices for agriculture, and it's something he actually advocated as a substitute for the price-support programs that were in place at the time, which he argued were perverse and providing the wrong incentives. So, just briefly, the idea would be that the government, for basic commodities, would set prices one season in advance based on all of its information-gathering power, and farmers could then react to the price, and that would at least be better than what existed. I don't think he thought that was ideal by any stretch. And D. Gale Johnson elaborated this into a whole monograph called *Forward Prices for Agriculture* in 1947. Now that's happening in the background before you even get there. Was there any conversation about that plan, or what happened to it, or did any of their questions for you reflect that history?

03-00:54:51
Telser: Not while I was working on my dissertation. The only time that this general issue came up, which did have a very major effect on me, was at my final orals. And I think I mentioned it in one of my articles that T. W. asked me,

"Why are certain products, commodities, traded on an organized market, and similar commodities not traded?" And I said, "I don't know." And I think it took me about twenty years or so [laughs] after that, in a paper with Harlow Higinbotham, when we looked at it systematically and we tried to explain the differences, to some extent, I suppose, in terms of the costs of organizing markets and the benefits of doing so, so that contrary to the usual idea that an organized market is a zero-cost enterprise, it really isn't. It's very complicated, very difficult, and needs very elaborate rules.

Now I want to come back, apropos that, to the point that Sylvia had raised about our venture in speculating. Now David, David Meiselman and I were very good friends, and David was working on a problem that's very similar to futures prices. His problem was on the term structure of interest rates. In other words, what is it that determines the relation between short-term and long-term interest rates? So that is very similar to the problem of what determines the relation between short-term futures contracts and long-term futures contracts. And David's principle thesis supervisor was Milton Friedman, and David and I were both in the money workshop. So in addition to the agricultural workshop, I attended the money workshop weekly, and so we knew about what we were doing.

Anyway, it turns out—and this goes back to the question of spreads—that there was a huge backwardation in the soybean market. Backwardation means that the near-term futures price was way above the long-term futures price. The usual argument is that, the difference is that the distant price should equal the near price plus the cost of storage. That's a terrible hypothesis. It doesn't work that way at all. It's much more complicated. However, in this case, the backwardation was huge. I think it was something like a dollar a bushel, which was unheard of. So David and I discussed this, and we decided it can't last; it's going to turn around. So what we did was a spread, so that we sold the near-term soybean contracts and bought the long-term soybean contract, and I think our margin was about $500.

Now, China is a major producer of soybeans, and so one of the arguments that the Chinese government made, which is the traditional argument of these places, is that the reason that there was a lower supply of soybeans is because of bad weather. We've heard that often, almost infinitely often. So, as things were going well, and then all of a sudden, news came out that a freighter had left Shanghai or someplace loaded with soybeans, and bang, the spread collapsed. So we got margin calls, and I think, I don't remember, we needed more money; we had to put up more margin. In a futures market, by the way, the broker never loses money. It's the customer that loses money, and if you can't add to your margin—and usually, you only have about a couple of hours or whatever to do it—then the broker just sells you out then that's your tough luck; you lost money. So that's what happened to David and me. So we lost money on that contract.

Now, later on, I was also involved in a spread, but this time, it was with Gale Johnson, who is much more knowledgeable about commodities than Meiselman and me. I don't know if we want to go into this, but it had to do with the fact that the nutrient content of a bushel of wheat is higher, bigger, than the nutrient content of a bushel of corn. So that means that the price of wheat should never go below the price of a bushel of corn, aside from whatever may be happening to the output and whatever. So the spread was too narrow, and the point is that D. Gale knew what the minimum spread should be.

So the two of us, what we did is, we sold corn and we bought wheat. And in that case, the thing that was really funny about it is, it took months before the spread began to widen, except, of course, in that case, there wasn't a problem of not being able to meet margin calls. And actually, I don't think we had any margin calls, but if we had, we would have been able to meet it. So anyway, it took a long time before the market finally reacted efficiently. And what I used to do is tease Stigler. "How come these markets are so screwed up? Why is that happening?" [laughs] It took months before they turned correctly. But anyway, both Gale and I made money on that spread. [laughter]

03-01:02:12

Burnett: Well, there, you got some practical experience in the work that you were doing. We talked a little bit about this before, but there was a bit of a changing of the guard on your thesis committee.

03-01:02:27

Telser: Yeah, sure. Marschak went off and Friedman replaced him.

03-01:02:32

Burnett: Yeah. And can you talk about Friedman's influence on the dissertation and his feedback on it, and what his issues were with it?

03-01:02:44

Telser: Well, the particular issue that led to the argument between Marschak and Friedman was that Marschak wanted me to deal with the details of storage, in particular, commodities, and Milton wanted a more general approach, and the two of them argued, and I just stood there silently. Well, first of all, I think Milton was right, and the reason for Milton's position is because of his interest in the relation between the demand for inventories, or the supply of inventories relative to the spreads in futures prices, that is very closely related to the theory of demand for money. And so he was interested in a general approach, and not in getting involved in the details of particular commodities.

Although, as it turns out, in order to do this, I did have to learn a lot about particular commodities. As an amateur psychologist, I would say, I don't think that this was really the cause of their disagreement. I think that this happened to be the current argument; it wasn't really based on any substantive issue.

They were—how should I put it—hostile in strong disagreement on many economic issues, and this just happened to be the current battle.

03-01:04:56

Telser: Then, what was Milton interested in, in the dissertation? Well, one of the things that he was interested in is rational expectations. That is, to what extent is it true that the futures prices accurately reflect the subsequent spot price? And the prevailing argument, at that time and for a long time, was actually in Hicks, in his book *Value and Capital*, where he argues this. So this was an attempt to give some kind of empirical substance to the Hicksian argument about, are these prices reasonably accurate? And later, it became, in a more popular version, it was called *Rational Expectations*.

03-01:06:13

Burnett: So, is that related to Milton Friedman's 1952 on "as-if" reasoning? So that the agents—that is derived from the piece in von Neumann and Morgenstern, the addendum in 1947, the expected-utility theory. Is that part of that lineage that comes out of that? Then Milton Friedman writes that up in '52; that's what becomes rational expectations theory?

03-01:06:47

Telser: Well, it's sort of interesting. The first article, I think the one you're alluding to, was a joint article with Jimmie Savage.

03-01:06:58

Burnett: Yeah, that's right.

03-01:06:59

Telser: Yeah, so that, it was a Friedman-Savage article, and that was the article that had the biggest impact on economists who were interested in utility theory. And now I can add something as a sort of a footnote. But I was a visitor, or a fellow, at the Cowles Foundation, 1964 or '65, and by then, Jimmie Savage was at Yale no longer. I think Savage went from the U of C to Michigan, and from Michigan to Yale. Anyway, at that time, he was in Yale, and we sort of knew each other. I was not a close friend or anything, but he knew who I was and my relation with Friedman. On one occasion, when we met, what he said to me was that he was very sorry he got involved with Friedman on this expected—there was a second article, by the way, that came out. And he said he's very sorry he got involved with Friedman on this, because he doesn't think it's right. He thinks the whole thing is wrong, [laughs] and he regrets that he got involved in it in the first place.

Now, of course, Savage—this is on a different topic—Savage was one of the major proponents of subjective probability, and the way I would describe it is that you cannot really draw a sharp distinction between subjective utility and subjective probability. Probability is a very difficult concept in the first place, and what Savage, the way he thinks of it, is that the only objective way you can really think about probability is in terms of, how much money are you

willing to pay in order to back up your opinion on something that may or may not happen? Well, once you put it in those terms, the distinction between probability and utility sort of disappears. And the fact is—and this is an aside, maybe we'll talk about it later—probability theory has always bothered me, and it bothers me because it isn't really a mathematical concept. There is no rigorous definition of probability, at least, in my opinion, and actually, I think, in the opinion of some other people. So that really bothers me, and the whole business about expected utility bothers me. I was thinking in what, I think, are more common-sense terms.

03-01:10:33

Burnett: So, I think there was a concrete consequence, at least in terms of the dissertation, because there's a piece of it that Friedman did not like, because he wanted you to use cardinal utility, right? And that becomes a reason—you agree, I guess—to excise it, because he asked you to. [laughter]

03-01:10:57

Telser: If your thesis advisor says, "I don't like this," you're not going to argue with him, and, yeah.

03-01:11:05

Burnett: So, you took it out and you published it as a separate article, which is when your first—

03-01:11:10

Telser: Right, and in fact, not only was it published as a separate article, but it was the lead article in one of the major journals, in *The Review of Economic Studies*.

03-01:11:22

Burnett: "Safety First and Hedging," and, I don't know if you want to talk about that paper and what's different about it.

03-01:11:33

Telser: Well, sure.

03-01:11:35

Burnett: Apart from what we've talked about already.

03-01:11:38

Telser: When I think about these sorts of things in terms of risk, I think of it in terms of what you can gain and what you can lose, which seems pretty obvious to me, and so you're willing to take a chance on a gain providing you don't think that you're going to lose very much if it doesn't work out. So when you put it in formal terms, it becomes a problem sort of in contradiction to what I just said. You say, "I'm going to do something, or choose a portfolio, that will maximize my expected return, providing the probability of losing more than this does not exceed a certain level." So it becomes a constrained maximum problem, and it can be shown by some people — in fact, it was shown by John Chipman — that this does violate the axioms of von Neumann and Morgenstern.

So then it leads you to a question, and since von Neumann is very smart and he probably thought about this: What's going on here? The *Theory of Games*, it's a very difficult book, and I don't know of anybody who has actually read it from cover to cover. So when I get stuck on something, I look at it and see if they had something to say about it. Well, in this case, there's a fairly complicated appendix about this, and I don't think that von Neumann takes the theory too seriously. He uses it because, in certain mathematical problems, where the objective you're looking at has sort of a funny shape, that one of the ways you try to handle it mathematically is by smoothing out the bumps and the wiggles.

Well, the way you smooth out the bumps and the wiggles is by convexifying it, as we say. Now what that really means is that you introduce linear approximations that could be interpreted in terms of probability, or inserting linear segments so that you act as if the pieces in between have a certain probability of occurring. I think, in my interpretation of von Neumann, that should be interpreted as a way of handling these ugly problems. It's not really realistic. But on the other hand, there really isn't any good alternative to these things. And, it comes up—we don't want to get into it—in quantum mechanics, where a lot of people are very upset about all of this.

03-01:15:25

Burnett: Now I'm struggling to recall if that is the part of the paper where you're talking about the derivative of a slope on a curve, and the ways in which that doesn't necessarily tell you where the curve is going.

03-01:15:39

Telser: You're quite right, that's a very pertinent element here, that in general, what happens is, depending on how well you understand mathematics, you may approximate a function so that it will satisfy certain convenient mathematical conditions. Now, and in this particular case, when it is applied to investment problems, in one version, when you make it a smooth function with a continuous derivative, and you get a nice Euler equation and all of that, it is important to understand the economic meaning of it. Now, the economic meaning of a derivative is that, at least in the neighborhood of what you have done, there's perfect foresight that you know what's going to happen, just as, in the future, you know it as well as you knew what happened from the past, that it goes continuously in this way.

But if you don't use differential equations to approximate it, so that there's these jumps and so on, then in a random walk, you know where you are, but you don't know where you're going. And that's a very big difference, and there aren't that many economists who know—I don't know. I shouldn't say it. I don't know how many of them know this, but there are examples of what—mathematicians, in the old days, it made them very unhappy that there are curves that are continuous and nowhere differentiable.

I think that Weierstrass gives an example, and it's actually in a textbook. It's reproduced in a textbook by L. M. Graves, who is the father of Bob Graves, my co-author. And in fact, the L. M. Graves textbook was one of the textbooks in a course I had on analysis. Anyway, the point is, that you have to understand that the tool that you use should not distort the fundamental aspects of the problem that you're interested in. And sometimes it does, and that's very unfortunate, because it leads to very bad economics.

03-01:18:40

Burnett: The historian, Philip Mirowski, wrote this book called *Machine Dreams: Economics Becomes a Cyborg Science*, and one of the stories that he focuses on is this 1947 addendum to *Theory of Games and Economic Behavior*. He argued that the addendum to *Theory of Games and Economic Behavior* in 1947 is that piece of von Neumann and Morgenstern that gets picked up by Friedman and others, which becomes expected utility theory, basically.

03-01:19:33

Telser: Well, [laughs] see, when you say "addendum," the addendum is in two parts. There's one part that's in the main text, and the other part is an appendix. The appendix is very complicated. The main text is much simpler, and I don't think that they properly appreciate the subtleties of the appendix, where von Neumann and Morgenstern, I think, felt freer to express their real feelings about the problems that this creates.

There is a very serious problem, because if you really think that you're going to have a mixed strategy—so, you're introducing probability into choices, deliberate choices, and you're saying that, well, what you're going to do is something that you yourself do not know in advance, and so the results of your own actions are not completely predictable—knowing that, how does this affect what you are going to do? When you leave the arena of writing a book and you're talking about a real problem, as was the case at the RAND Corporation—when we had to talk about real problems—then it's pretty tough and you can't weasel around. You start talking about doing things that can have huge, unfortunate consequences.

03-01:21:39

Burnett: So, Mirowski's point was that that revision of von Neumann and Morgenstern, they never intended that piece of it to then stand for economic rationality.

03-01:21:51

Telser: I don't think so.

03-01:21:52

Burnett: Yeah, and he said that that's where the uptake from cybernetics into the economics profession, at least in that particular sense, was much narrower than cybernetics as a project, or operations research as a project as a whole,

which was about elaborating different forms of reasoning, and different forms of decision-making theory applied to different contexts.

03-01:22:19
Telser: Yeah, I think it was. Now let's see. Von Neumann died in 1957, and I think that, by 1952, he was already gravely ill, so that he wasn't the same person in the last five years or so that he had been before. It's very upsetting to read about this. It's another example that, sometimes, the disciple is a very pale copy, or an imperfect copy, of the master. These are very hard problems, and we're facing them right now, and there are very hard choices that have to be made, and there aren't very easy ways of doing it.

03-01:23:26
Burnett: Well, I want to save all of those problems for the end of our talk, and just for the sake of our timeline, something else happens in the mid-1950s on a personal note, on or around 1955 or so. Is that a good time to talk about that, or should we finish talking about your dissertation?

03-01:23:50
Telser: No, no, why don't we? I think we pretty much finished the dissertation, yeah. So what did you have in mind?

03-01:23:58
Burnett: Well, there's someone named Sylvia. Can you talk about her a little bit?

03-01:24:03
Telser: Let's see. Sylvia came from Montreal to Chicago, and was living with her aunt and uncle for a short time, and then moved to Hyde Park and had an apartment with several other young ladies, and one of them, Claire Friedland, was a pretty good friend of mine. She was a graduate student in economics. As a matter of fact, D. Gale was her thesis supervisor, and Claire's dissertation was on sweet corn. She never finished it, which neither I nor Gale could understand.

Anyway, so they had a party, and I was invited, and that's where we met. So it was in October of 1955, and I was smitten. [laughs] And I was smitten for two reasons: first that Sylvia is very beautiful, and second, even more important, she's very smart. Well, she went to a relatively small high school, and I think that in Quebec, they used to have competitive exams for the whole province, for the high school students, and she came at the very top in the year that she took the exam. And never mind the test score—she is very smart. She had a lot of interests in things that most girls had no interest in, including, in her case, she was very interested in economics and in social sciences and so on, and so we had a lot in common.

So, the relationship, I guess it flowered, and then, in December of 1955, I still had not finished my dissertation but I left Chicago for Ames where I was an assistant professor. So then the relationship sort of continued, somewhat long-term. We visited each other pretty often during that period, and got to know

each other better. And I think I proposed pretty early, sometime, I don't remember exactly, maybe, I think it might have been in December, as early as December. So it was a very short time between our first meeting and when we decided to get serious. And by June, the date had been set, and so we got married on June 24, 1956, and we have been, I think we're best friends. We have been best friends ever since. I would say that, intellectually, we're very congenial, and, in all kinds of ways. Of course, we disagree on some things, but I think on all the important things, we agree.

But then I was drafted in September, and Sylvia had been working as a social worker in Chicago. So, after I was drafted, I was in—well, they say, technically, I think it was what, six or eight weeks? That isn't really true. Basic training is about three months, in fact. So, by the end of December, I had finished basic training; I had been transferred to a research unit. It was originally at Fort Ord in California, and then, it's a short, I think, several miles or so from Monterey. And so in December, I was in California, then returned to Chicago around the middle of December, and we spent two weeks or so together, and then we drove across the United States in the winter to California.

We had some interesting adventures. I think that for anybody to drive through the Rocky Mountains in January shows that they're recklessly ignorant. I remember, at one point, Sylvia was driving and we hit ice, and the car skidded, unfortunately, stopped at a—what was it—one of those things at the side of the road, that stopped us from getting all the way across.

03-01:29:58
Burnett: You touched the barrier?

03-01:30:00
Telser: Yeah, we touched the barrier.

03-01:30:00
Sylvia Telser: Well, the barrier was covered with snow.

03-01:30:02
Telser: It was covered with snow, yeah.

03-01:30:04
Burnett: But still. [laughs]

03-01:30:05
Telser: Yeah. But yeah, we touched the barrier. If there hadn't been a barrier, we wouldn't be here now. [chuckles] And then the other thing I remember that we did that was kind of stupid is, you were required to have tire chains to go through Donner's Pass. So we didn't have tire chains, and so we figured that if we went at night, nobody would stop us. But it's a pretty stupid thing to do, to drive through Donner's Pass at night in the winter without chains, but we did it. [laughter] So there were a lot of things that we did. And even before that, I

think, we did some pretty stupid things. When we went to Montréal to visit Sylvia's mother, and brothers, I had a '55 Ford with a V8 engine, and at one point, we wanted to see how fast it would go. So I went up to about ninety miles an hour, which I don't think is a very smart thing to do.

And then, in Monterey, because Sylvia, she was a Canadian—and in California, they had very rigid rules, especially Monterey. In Monterey, there's the Navy Postgraduate School and the Presidio of Monterey, and Fort Ord. So it's a very military type thing, and the civilian jobs were mostly with the state of California, and Sylvia couldn't work for the state because you had to be a citizen, and she wasn't. But, as I say, she's very smart, and she got a job with a very famous psychiatrist, Eric Berne. You may have heard of—no? Well, anyway, so she got cases from Eric Berne, and in addition, she did something else to make money. I'm blocking on what it is. She sold, I forget what it was, some kind of—

03-01:32:45
Sylvia Telser: Avon cosmetics.

03-01:32:46
Telser: Avon cosmetics.

03-01:32:47
Burnett: You were an Avon Lady?

03-01:32:51
Telser: Yeah. And I think also, at one point, the *Encyclopædia Britannica*, but that was a pretty big bust, so Sylvia was an Avon Lady. And the other thing that I did to make extra money, I rose in rank, so that after about, I don't know, six or seven months or so, I was a Spec Three, which is the equivalent of a corporal, but you do get an allowance, but still the pay is very low, so I got a job teaching statistics to high school teachers at the Monterey Peninsula Junior College. And so, I think I taught one semester based on a statistics textbook by Wallis and Roberts. The students were very nice; they were the high school teachers. But then I lost the job because, in order to continue, you had to qualify in visual training or something. You had to be able to run a camera or a TV or something, and I was not qualified, so I was fired.

Oh, and then there's another interesting fact about that, and all the while Sylvia was helping support the family. The pay I got as an instructor was five dollars and twenty-five cents an hour. And Gary Vescelius, who was one of my comrades at the unit, he was an anthropologist, and he did not have his PhD, and he only got five dollars an hour. So I got a monetary measure of the value of a PhD: it's twenty-five cents an hour.

03-01:35:00
Burnett: We talked off camera about this, but I am astonished that the PhD, especially in a quantitative field, did not automatically translate to officer class with

officer training. I guess if you're drafted, you don't get your pick of things, they just shunt you where you're needed.

03-01:35:27

Telser: The US Army is different. I don't know, but my impression is that the Canadian Army and the US Army are very similar in this respect. European armies would do exactly what you are describing. If you are a college graduate or if you have an advanced degree or whatever, you're automatically considered officer material. But in the US Army, that is not the case. In order to be an officer, you really have to be, really, a professional military, not somebody that happened to get through a college or whatever, and especially once combat has finished, so that there isn't the same pressure or need for officers and so you don't have OCS [Officer Candidate School]. It doesn't really matter.

They do try to use you usefully, and in my case, after basic training, I certainly was very well treated, and in fact, wrote two articles that were published in professional journals. And the project I was on was a real research project until it was cancelled, but you don't get any special rank because of that. There's one little minor point, though, that I always thought was interesting about the Army, and that is: it's the first time, just as I was drafted, that you're measured and you get tailor-made shirts. I had never had tailor-made shirts in my life before, except when I'm inducted as a draftee into the US Army, and all my shirts were tailor-made.

03-01:37:43

Burnett: So it wasn't fatigues; it was the dress browns.

03-01:37:47

Telser: Yeah, no, fatigues are not tailor-made, yeah. Some of them were more dressy than others, like with epaulettes and so on, but then, just the regular shirts under your regular uniforms, those were tailor- made.

03-01:38:03

Burnett: I don't know how much we can talk about this. It was a psychology unit that you were working with?

03-01:38:14

Telser: We could talk about that, sure. Well, of course, the fighting in the Korean War had ended by the time I arrived, but before that, one of the things that the Army learned in World War II and presumably before, they learned a great deal about becoming professionals, so that it wasn't just "charge and over the top," or anything like that. For example, one of the projects that they had was, in fact, I believe it was by a historian, S. L. A. Marshall. And S. L. A. Marshall was assigned in 1944, I believe. He was attached to US Army units, and he was supposed to interview soldiers immediately after combat. Not so that you didn't rely on somebody telling you all kinds of fancy stories about

what happened, but he would interview them and find out exactly what happened during the events of the preceding day.

So, one of them, I remember, was on the island of Kwajalein. Now, all these battles with the Japanese, they were very bloody, hand-to-hand combat, so that I think it was also similar to that against the Nazis, but it was pretty bad in the Pacific. And they learned something very interesting that I don't think was widely known, and that is that only 15 percent of the men in combat actually fired their weapon. Yeah. And so this sort of thing had a tremendous effect on training, and in the unit that I was in. There's some I can't talk about because it's classified, but there were other projects that were studied that were related to improving the ability of the Army to perform well in combat. By the way, the Army that has a very high firing rate, way above 15 percent, is the IDF, Israeli army. I mean, firing at somebody is a hostile act, so a lot of these guys—and it's not just that it's a hostile act—they really didn't know what to do. So the effect of this was to change the training.

One of the projects that I was on was, the advisor was Leon Festinger, who is a very famous psychologist at Stanford, and it was about, the main problem, it's called post-cognitive dissonance. And for example, Festinger found out that people are more likely to look at the ads of something they already bought than to look at ads when they're considering what they should buy. To me, as an economist, that came as an astonishing fact. So I was used as an economist in this stuff, and the problem that the Army was interested in was whether the junior officers would behave in a sort of a rational way.

So they did experiments with captains. In a simulated situation, a captain would make a decision to do something, and then they would be fed information, and the information was that they made a mistake and that what they did was wrong. And so what the Army wanted to find out is, how would these guys respond? Would they get the hell out and change, or would they just go ahead? And they were very happy to learn that, by and large, the captains were pretty good.

And so the result of it, however, was, in my case, that after about two months, they decided the results were so good, they just stopped the project. But that was actually good for me, because then I was left alone, and most of the time, not always but most of the time, I had an office. I would come to my office, and I would sit there and, for example, one of the papers I wrote was about "profitable speculation is stabilizing," and I also wrote a couple of articles based on my dissertation. And by the way, in the morning, we played bridge, and during lunch hour, shuffleboard. So I did work; my own work I did in the afternoon. And then once, the only time Colonel Francis P. Mulcahy took me—I'm trying to remember the other guy. The one from this other—

03-01:44:15

Sylvia Telser: Mac Richards.

03-01:44:16
Telser: Oh yeah, Mac, Mac Richards. Richards, right. Yeah, Mac Richards and I. Mac was a psychologist. Most of the people there, by the way, were PhDs, or on the verge: Gary Vescelius, Yale; and Mac Richards, Vanderbilt; and other, like that. So anyway, so Colonel Mulcahy took Mac and me with him to Hunter Liggett, which is—well, you know where San Simeon is?

03-01:44:54
Burnett: Yeah.

03-01:44:54
Telser: Okay, well, San Simeon, I don't know, is it open to the public?

03-01:44:59
Burnett: Yeah.

03-01:45:00
Telser: Oh, it is. At that time, San Simeon was the officer's quarters for the Hunter Liggett military reservation, but that was a long time ago, sixty years ago, actually, I guess. Anyway, and so we went down there and we observed the training exercises. No, they weren't training exercises—small-unit tactics. Oh yes, and Colonel Mulcahy wanted us to be given quarters in the officers' quarters, but they refused. We were Spec Three's. We're non-comms. "You can't be with the officers."

So, anyway, so Mac and I were in a barracks in a regular infantry platoon. And that, I must say, is an interesting experience. The guys who were in regular infantry, you have to be a pretty tough character. They were very nice, and so on. And I remember, they were staying up till about midnight or so listening to the radio, then, I remember, Mac and I saying, "Don't you guys ever go to sleep?" [laughs] It was quite incredible. That is sort of an interesting experience in the process of becoming a professional economist that I had.

Oh yeah, I should mention one other experience I had, which was fortunate and I was lucky the way it ended. One of the things in basic training is to have simulated combat. And that meant that we would go on a battlefield where there would be pop-up targets behind rocks and trees and all that. Everybody in the platoon is on a line. We have our loaded M1s with eight rounds in a clip, and you're supposed to stay in line, and if you see something, shoot!

Well, the guy next to me in the line, I think he must have been an imbecile or something. Anyway, his rifle exploded, boom! And I was lucky that it was sufficiently far ahead of me so none of the pieces came near me, but that could have been a very serious or fatal accident. He survived, and what I remember is that three or four of the cadre ran over to him and one of them said [affects deep accented voice], "If I was your mudder, I would'a choked you when you was a baby." [laughs]

03-01:48:15
Burnett: They concluded immediately that it was his fault.

03-01:48:18
Telser: Yeah, oh yeah.

03-01:48:19
Burnett: He must have jammed the barrel or something?

03-01:48:22
Telser: I don't know what he did. Anyway, then, he disappeared; that is, he was removed. You can't have anybody in a platoon who's an idiot, and that guy was taken out.

03-01:48:37
Burnett: Goodness. Well, I guess we can say, about the draft—because Arnold Harberger has talked about this stuff, and his time in the war, and George Shultz, as well—is it pulls you out of your bubble. [laughs] If you want to have some diverse experiences, you're going to have them, whether you like it or not.

03-01:48:56
Telser: Oh, definitely. Yeah, I certainly had diverse experiences. Oh, and I remember once, with Milton—when was it? Yeah, I remember. It was about the Yalu [River] when the Chinese were coming across. So, and afterwards, I often would walk home with Milton and we would talk. And he said, "Well," he doesn't understand why we didn't bomb the bridges across the Yalu. And I said, "Milton, you know how long it takes to build a pontoon bridge across a river? I don't know, maybe five, six, seven hours. That would be a very wasteful way of using defense money." [laughter] So Milton, although he had been at the Statistical Research Group [war-research project at Columbia University], there were certain practical things I don't think he was aware of. [laughter]

03-01:49:58
Burnett: Well, I want to back up a little bit, because one thing that we moved over quite quickly was your time at Iowa State as an assistant professor. That's your first job as a professor, and Iowa State is a remarkable institution. It has ties to Chicago. I'm wondering if you could talk about your time there.

03-01:50:21
Telser: Well, I taught a price theory course, and I was enormously impressed by the quality of the students. Now this was an undergraduate course, and they were really superb students. And in fact, I remember, there was one young man who was very, very good, and I tried to persuade him to go into graduate work, but unfortunately, after college, he had to return to take over family farm, because his father had died, so he would be the mainstay of the family. And there were other aspects. Statistics, for example, at Iowa State was first rate. They had probably one of the top three statistics departments in the country. The research that I did there was on the price support program, and I

would give data to the statistics computer group and ask them to run some regressions, and bingo, I would get the results back almost immediately.

My colleagues were very good. Karl Fox was the chairman of the department, and he was a very distinguished agricultural economist. And, I arranged some seminars. People came. In fact, Zvi Griliches was one of the people that came to give a seminar at Iowa State, and I had invited him. And I shared a house with three graduate students: one from Jamaica; a black, a very smart guy, I think, Walter Miller, I remember his name; and then there was another one whose family owned an orchard, I think it was in either Washington or Oregon. And so we had our own cooking and all of that, and it was very amicable. Oh, Gerhard Tintner was at Iowa State. He was a very distinguished econometrician. But, I wasn't really there long enough for any major things to develop.

Now if I hadn't been drafted, of course, then we would've returned to Ames at Iowa State and so on. One of the things that I remember: I would get up Sunday morning and go out to get *The New York Times*. Everybody else was going to church, and I was unshaven and going to get *The New York Times*. On the other hand, here in this small town, [laughs] *The New York Times* was readily available, and also, there was a very good program for plays and so on. I remember I saw some very good plays there. It was an interesting experience. And of course, Sylvia visited very often, [laughter] so that helped a lot.

03-01:54:36

Burnett: I'm sure it did. You were not trained as an agricultural economist; you were a Chicago economist who happened to work on problems of commodity trading. And so, in a sense, inadvertently, because they finished up that project in Monterey, you could use part of your time there to publish, and it was almost like a partial post-doc, in a sense, and you could get your new articles into journals, and you can prepare yourself. Now, did you go on the market, or were you planning to come back to Iowa State at that time?

03-01:55:27

Telser: Well, initially, I was planning to return to Iowa State. In Monterey, there were two serious alternatives: one was Berkeley, and one was the business school at Chicago. We'll talk about that later. The business school at Chicago had virtually died, and it was being revived by Allen Wallis, but I was very interested in Berkeley. It's sort of funny. I don't remember who it was, one of their prominent professors, said to me, he said, "Well, we are willing to hire a student of Milton Friedman, but not Milton Friedman." And I don't think he was aware of the fact that for him to say that to me would kind of kill off my interest [chuckles] in going to Berkeley. I don't know why he said that to me. You can interpret it in different ways, but I was seriously thinking about accepting an offer from Berkeley.

03-01:56:45
Burnett: So Milton Friedman had this kind of reputation at that time, by 1955, that he was considered to have certain preconceptions. What was the reason for his disapproval of Milton Friedman?

03-01:57:02
Telser: I'm not really sure. My guess is that the main reason was his hostility to Keynesian economics. I don't think it had anything to do with politics. But, at that time, well, there were people at Berkeley who were doing sort of the traditional monetary stuff, but the macroeconomics was largely Keynesian. And it is still sort of a puzzle to me. I'm not really sure. I couldn't tell you the reason for their interest in me. Andy Papandreou was very cordial, and there was Roy Radner, who was a very close friend of mine at Cowles, was at Berkeley, and in fact, I remember, I think, we had dinner with them when we were visiting, when Sylvia and I visited.

So, I think the main reason I went to Chicago and did not go to Berkeley is because the Chicago offer was simply a better offer. There wasn't any ideological thing about it at all. And the person who was mostly responsible for my offer from Chicago was actually Al Harberger, and Al Harberger had a simple principle. He said, "Always take the money."

03-01:58:52
Burnett: As opposed to the title, for example.

03-01:58:54
Telser: As opposed to the title. And even, in fact, then going ahead a little bit, after I returned to Chicago, I think it must have been 1960. By the way, I did have to spend one two-week period in the reserves at Fort Bragg, which is also interesting. We can talk about that later. But, Al Harberger, Carl Christ, and I had a triple offer from Johns Hopkins, and I think that was in 1960. Fritz Machlup, who had been at Hopkins, had gone to Princeton, and of course, Al had been at Hopkins and was at Chicago. And Simon Kuznets had left—

03-01:59:59
Burnett: And Carl Christ did, too.

03-02:00:01
Telser: I think Simon Kuznets was at Harvard. So they were sort of short, and they wanted to build up the department, so they made these three offers. And the result was that Chicago made extremely attractive counteroffers that certainly, to me and to Al—in my case, I was promoted and I became a full professor, which is pretty good. I think I was twenty-nine. So, and with two years out. I was probably one of the youngest full professors at Chicago. And Al, I think, got a pretty good deal, but there was no counteroffer, so Carl ended up at Hopkins, and Al and I remained at Chicago.

03-02:00:57
Burnett: So there's kind of a homing instinct it seems among a lot of Chicago folks, too, and it's not necessarily the pushing from them coming back, but the activities, the pull factors, the builders at Chicago really fighting to keep a set of projects together.

03-02:01:20
Telser: Yeah, no, that's absolutely correct, that this was a very lively place, and there were people here that I had worked with very closely. And the workshops were very active and so on, and we attracted very good graduate students, and you just couldn't find it anyplace else. And of course, I had experience. The experience at Harvard was nothing like—no workshops, although the students were pretty good at Harvard, but were, I would say, by no means comparable in their ability to the ones at Chicago.

03-02:02:03
Burnett: And there were perhaps some. Cowles Commission was extremely strong. It was this extremely generative set of research programs with people who were really, really talented in these different ways working together on projects.

03-02:02:23
Telser: But that was earlier. Cowles had left in '54.

03-02:02:26
Burnett: By '54, it's gone, and I'm wondering if that was a kind of lesson or a caution as to what could happen if you didn't keep things together. And then the business school was being revitalized, and that's an interesting story as well. Allen Wallis was this leader who made a bunch of hires: you, George Shultz, a number of others, and built up that program. Can you talk, with some hindsight, because I'm sure as a fairly junior, new person on the block there, you didn't get the full lowdown on what this project was about—from your sense though, what was the plan at the Graduate School of Business to build it up? What were the projects, the orientation of the school? What was going to be new about it that distinguished it from other schools of business?

03-02:03:31
Telser: Well, first of all, a very strong group in economics. Secondly, a very strong group in computer science. And of course, they had very good statistics, and they also had very good people that other business schools did not have, for example, in other fields: psychology, anthropology, sociology. So that it was a much broader array of academic disciplines and skills than any other business school in the country. And I think it did become, and remains, a leader among business schools. Well, of course, and George Stigler was joint between the Economics Department and the business school, and so he was sort of the key figure in economics. And they were very ambitious in trying to hire other people, sometimes with success, and sometimes without success.

For example, now, I think Hans [Henri] Theil, I think he was a joint appointment between the business school and the economics department. I

don't really remember. But anyway, Hans Theil came, and Robert Basmann in econometrics came. Then, in addition, coming to but not in the business school, but coming to Chicago, [Hirofumi] Uzawa. The departure of the Cowles Commission led to a very strong effort to reconstitute, and in some ways, the reconstitution moved in the latest new directions.

And here's an interesting fact also about Milton, and that is, he and I were the only two economists in a course that Bob Graves taught on computing. So we attended his course, and in fact, that's when I first met Bob. And also, by the way, to give you some idea of Milton, that when Bob would be writing one of these programs on the board, Milton could spot mistakes. [laughs] Now whether he had ever known anything about programming before, I have no idea, but this sort of gives you an idea of what kind of a brain power was there. And we can talk about some other aspects of that.

Anyway, but the point is that there had been problems and there were restorations. Now Nick Metropolis was brought in. Nick Metropolis was the chief computer scientist at the Manhattan Project; he was really involved at that. We didn't know that at the time, but he was really involved in the hydrogen bomb stuff. But he was brought in, of course. So the computer group was really very strong, and I was very close to Bob Ashenhurst. We shared an office, and Ashenhurst was an associate of Metropolis, so I learned a lot about the inside story of computing from Bob Ashenhurst.

03-02:08:02
Burnett: So was the Institute for Computer Research, was that inside the School of Business, or was it kind of affiliated with the School of Business?

03-02:08:11
Telser: I think it was affiliated. I don't think it was inside; I think it was affiliated.

03-02:08:17
Burnett: So Metropolis was this physicist; he founded that. He's also a co-developer of the Monte Carlo method?

03-02:08:24
Telser: Yeah. [LT: Stan Ulam was also an inventor of Monte Carlo].

03-02:08:24
Burnett: Or methods. And, I understand from George Shultz is that they built a mainframe in the School of Business, or perhaps it's just part of the Institute for Computer Research.

03-02:08:42
Telser: Yeah, well, Nick Metropolis, the way he was doing research required building a computer. Yes, and they did build a computer. Well, it was in another building, of course, but I'm trying to remember. It was in another building. I don't remember which building it was in.

03-02:09:04
Burnett: And they used that to develop the stock price database. They had this—

03-02:09:10
Telser: Oh yes. Larry Fisher was in the business school, who was a brilliant guy, but not a very good teacher. But the basic research on stock prices was primarily done by Larry Fisher with Jim Lorie, and their publication, I think, is simply invaluable and has never been equaled.

03-02:09:45
Burnett: So there's this understanding that computing and computational analysis is going to be very important. It was already shown to be important at Cowles, at RAND, at MIT, and these are the places where that future is being developed, and so the Graduate School of Business understood that. Allen Wallis understood this, and built that into the revitalization of the GSB at that time. Well, let's pick up next time and we can continue talking about the Graduate School of Business.

Interview 4: July 13, 2017

04-00:00:17

Burnett: This is Paul Burnett interviewing Dr. Lester Telser for the Economist Life Stories Project, of the Science, Technology, and Medicine Series, for the Oral History Center at the Bancroft Library. And we're here on July 13, 2017, in Hyde Park, Chicago, and this is our fourth session. So, we were talking about the ambitious shift into the use of mainframe computers by the revitalized [University of] Chicago Booth School of Business in the late 1950s, and one thing we want to keep track of is changes in work practice. And so, you've alluded to specific cases and instances in which you describe the kind of work you were doing physically. Can you talk about the physical work of being an economist? It's often described as a mental practice, but physically, what were you doing as an economist in the 1950s?

04-00:01:34

Telser: Well, one of the main things that economists did and that I did was to collect data. That means mostly numerical data, statistical data, from various sources, either from government agencies, so that we would use data on national income, or price indexes, and some of these indexes were developed over the past forty or fifty years by economists who were working for the government for this purpose. For example, the wholesale price index was started in 1913 by Wesley [Clair] Mitchell, and the national income statistics were begun on a large scale by Simon Kuznets working at the National Bureau of Economic Research when Wesley Mitchell began the bureau. And there was an enormous amount of data that assimilated from census figures of the government and used to construct these data. And it's not just a question of collecting the numbers, but there are very difficult, conceptual questions of what numbers you should collect and what do they mean, and what are the sources, and so on.

So that was a major part of the work, getting the input for empirical studies. And one of the major areas of interest for economists, especially in the early years and beginning in the late nineteenth century, was the study of business cycles, fluctuations in income, prosperity, depression, inflation, and the big problems like that. Now, in order to do the calculations, at first, in the early days, starting in the early 1900s, we had either punch cards—in fact, the first actual use of tabulating equipment to handle large bodies of data was for the census, I believe it was in 1890, and that led to the invention of tabulating equipments, punch cards, and the beginning of the company that eventually became IBM, International Business Machines. And, it's sort of interesting. When IBM started, they not only made tabulating equipment, but they also made scales, for example, for grocery stores and butchers and whatever. So it was literally a variety of business machines, but gradually focusing on just their punch cards and the tabulating equipment.

But these things were very expensive, and only the government and some companies' insurance companies found it economical to use this fancy equipment. So when I came on the scene starting as an economist, I suppose you could say in the late 1940s or early 1950s, in order to tabulate things, in order to calculate statistical results, linear regressions, et cetera, we used desk calculators. And the desk calculators that I used at one time or another were Monroe, Friden, and Marchant. And one of the things that's interesting that always intrigued me is that none of these companies seem capable of adapting to the computer industry. I can't think of a single one that actually shifted from making desk calculators to going into making digital computers.

And the first digital computer that we had at the university, I think it was a Sperry Rand computer, and it was enormous. It took up the whole basement of the administration building. It had thousands of vacuum tubes. The input came in the form of punch cards, and it took skilled engineers to run the thing. And if you got ten minutes of operation on the computer without it suddenly stopping, you were considered lucky. Well, the two groups I think that were doing most of the empirical work were either the ones doing work on agricultural economics: Marc [Leon] Nerlove, [Hirsh] Zvi Griliches, and me; or, work on monetary economics: Phil [Phillip David] Cagan, David [I.] Meiselman. And in some of those cases, we were calculating weighted moving averages.

So when you're doing weighted moving averages, it's a heavy computational task. It wouldn't be surprising to spend practically the whole day doing nothing but punching numbers into the calculating machine and writing them down and so on. So that took a lot of time, and it was tremendous change when, in the late fifties, 1958, '59, or early sixties, that we had a digital computer, which, by modern standards, is very, very primitive, but for us, it was a tremendous labor-saving device.

04-00:08:41
Burnett: Well, when you went to the administration building, you would basically register to have time on the machine, right? And you'd have to kind of turn in a chit or something to say that, "Here's the Economics Department, or here's the Graduate School of Business, and we need *x* amount of time for this operation." You'd get permission.

04-00:09:02
Telser: Yeah. You would get a block of time, yes.

04-00:09:08
Burnett: And, the people doing the operations in the basement, the programmers, were those mostly women at the time?

04-00:09:17
Telser: I would say so. Quite a few of them were women, yes.

04-00:09:20
Burnett: Yeah, because the computer was a job classification at the time. The computer was a woman, who would work these machines. That provided early entry into some fields for ambitious women. And I imagine it must have been hot, as well. These are tens of thousands of—

04-00:09:41
Telser: No. It was air conditioned, not to help the people. The machines were actually running very hot, and so you couldn't let the temperature get too high, so that the only place that the university, in fact, where there was air conditioning, was the home of the computers. And in fact, one summer, I think it might have been 1960 or so, I was teaching a course during the summer in econometrics, and it was taught in the same building on [South] Ellis [Avenue] where the computer was. The difference in the temperature between the computer and the outside was, I don't know, twenty or thirty degrees, and I got a terrible cold. [laughter]

04-00:10:38
Burnett: Well, I think it's important to underscore the role of data in the work of economists in the Post-War era, and it was very much owed to this earlier period of data collection. You need long time series, and that takes decades to accumulate those data, and, these were large organizations, and agriculture was one in which you had a real investment. The Bureau of Agricultural Economics was the fourth largest research bureau in the federal government. So large was its budget, in fact, that Herbert Hoover, when he was head of [the United States Department of] Commerce, was jealous and tried to get it shunted to his department without success. So, gathering agricultural statistics became an early opportunity to do the kinds of analyses that were being undertaken at Chicago. It's no wonder that research in agriculture was such a fertile area for this kind of highly quantitative research.

And so, there is so much going on with respect to your research, which is migrating at the end of the 1950s out of studies of agriculture, and so on. Oh, there's one more thing I'd like to ask you, and it's germane to what we just talked about, and that is: There was an article about the Bureau of Labor Statistics with William in 1960 called "Food Prices and the Bureau of Labor Statistics," and it was about precisely that, about the problems of interpreting the data, and the way in which the data is collected, what isn't collected. Is there an example that comes to mind, in general, of any example that comes to mind, of unique problems with data, the way it was gathered, and how that almost thwarted your efforts to develop a hypothesis and reach conclusions?

04-00:12:59
Telser: Well, the raw data was collected by field agents working for the Bureau of Labor Statistics, and they were not professionals. Their job was to go into stores—in the case of food prices, grocery stores—and they had a list of commodities whose prices they had to collect. And they would go once a month, they would visit, or the data were collected monthly, and they would

write the numbers down on a regular form, and it would get sent back for processing to Washington [DC]. And this raised a number of practical questions that you wouldn't be aware of if you didn't know anything about food retailing. So, for example, how do you handle things like fruits, vegetables, meat products that are not standardized, and that are not branded items, where it's very difficult to tell the difference, or to determine the effect of quality on the price?

Then there was another problem that came up, and that is, do they want the regular prices or do we include sales prices? And the rule, in general, was to use regular prices and not use sales prices, which imparted a bias to the statistics. And in fact, we made a point of this in the article, the point that the official price statistics understated the variability of actual prices in the course of a business cycle. Then there the question of, what stores do you choose? What cities appear in the sample? And so on.

And this created complicated questions. Some of it was political. A congressman would like to have stores in his district appear in the sample. And this did create problems for the BLS [Bureau of Labor Statistics], and there were some important foods, retailers, that would not cooperate, and typically, these were the chain stores. So for example, there were no food prices collected from A&P, [The Great] Atlantic and Pacific [Tea Company], which, at that time, was the largest national chain. And there were very few chains that would allow people to come into the store and collect data. So this was another problem.

Then there's the other problem, aside from the weights and how you put the indexes together and so forth and so on, but then there was the problem of, how do you handle brands? There's a confidentiality problem. Companies that are selling a branded product don't want to have easy access by the BLS or by consumers to make price comparisons between themselves and their competitors. So there are all kinds of very practical problems that came up in these data collection efforts. Now the other problem, a basic problem, was: Is the index supposed to measure price levels or price changes? As it turns out, the decision was that the index is supposed to measure price changes and not price levels.

And that, in fact, the confusion over that particular question, is what led Bill Kruskal and me into the whole problem of measuring the price index, because at that time, I should say, there was a company called S&H [Sperry & Hutchinson Company]. They issued Green Stamps, and certain stores, especially certain chain stores, gave out Green Stamps proportionate to the amount that you spent in the store. So the argument—we won't go into the economics of it—is that this somehow created monopoly power, so that the stores that gave out the Green Stamps could charge higher prices than consumers would be aware of. So in effect, the consumers were being cheated by going to these stores and thinking that they were getting lower prices or a

deal, because they were getting S&H Green Stamps, when in fact, that wasn't true.

Now the problem with that argument is that the price statistics couldn't tell you anything about that particular question. And we made a considerable effort to point out that these statistics measure price changes and not levels, and furthermore, the official price statistics did not include prices at the chain stores. So, it was a major exercise that we did to explore the actual accuracy of the statistics that were coming out of the Bureau of Labor Statistics that were—and those numbers were being used by economists who regarded them with much more awe than they deserved. And for me, in particular, and to some extent, I guess, for Bill—but he's a professional statistician, he was more sophisticated about this than I was—it was eye-opening to see all the things that were wrong with the way these numbers were being collected.

Our article was published in *The Journal of Business* and the commissioner of labor statistics wrote a nineteen-page, single-spaced letter criticizing it. And as it happens, Bill was then in Europe, so I wrote the rejoinder. [laughter]

04-00:20:42
Burnett: How long was the rejoinder?

04-00:20:44
Telser: It wasn't very long. I think it was about two or three pages. I should add that Bill and I, we read probably everything that had been written, every article about price indexes and the government statistics and so on, well over a hundred articles. Bill was a very thorough scholar, much more so than I am. And so, it was really quite an experience. I should also add that he was a professor of statistics who eventually became dean of the Social Science Division, and then provost of the university. And there was one considerable disadvantage because of my relation with him: When he was the dean of social sciences, he made me chairman of the Computer Committee for the Social Science Division, and then when he became provost, he put me on the university Computer Board. So I got these administrative jobs because of Bill Kruskal. [laughter]

04-00:22:16
Burnett: He knew you were the right man for the job, because—

04-00:22:19
Telser: Yeah, or he knew I couldn't say no.

04-00:22:22
Burnett: Maybe that's also part of it. [laughs] We talked about this last time, but I can't help but think that this intimacy with the data, with the problems of the data, informed your mathematical work, that it had an impact on thinking critically about what could be deduced from various quantitative hypotheses, and it made you epistemologically humble, I suppose, is a way of putting it.

04-00:22:57
Telser: That's correct, and very skeptical of fancy, econometric techniques and models. Now there was another source of data I didn't mention—this is separate—and that is, when I joined the business school in 1958, one of the projects that I worked on and spent a lot of time on was the problem of estimating the elasticity of demand for branded goods. And in order to do that, it was necessary to get detailed data on sales and prices of these branded goods. So, I had access to a consumer panel that was run by MRCA, Market Research Corporation of America. They had panels of households, and the households kept a diary, and in the diary, they would record what they bought, the brand, and the price they paid. And so that this was something that they wrote down immediately after they had come home from shopping.

There were several interesting aspects to it. Number one: Is a panel a good sample of the population buying this stuff? And there is a problem, because those who become members of the panel are typically not working mothers, that is, in other words, or not people with regular jobs, so that shopping and cooking and all of that is a small part of their daily activities. So you tended to get a different segment of the population than would represent the population at large. But it did seem that, in this way, you got a much more accurate estimate, let us say, of food prices than the BLS would get by their method of using field agents to go to the stores. And I used the panel data in studying the demand for certain highly advertised commodities: frozen oranges, concentrate; regular coffee; instant coffee; margarine. I think those were the products. And it's interesting that one of the founders of the company [MRCA] was Oskar Morgenstern.

04-00:26:13
Burnett: Really.

04-00:26:15
Telser: Yeah. And in fact, once, I visited Princeton [University] and I had lunch with Oskar Morgenstern, and to some extent, it was related to my use of the MRCA panel data. There were a number of interesting questions that you could answer with the panel data. Oh, and I should mention, the ones who bought the panel data, the ones who used the panel data, were the corporations that sold this stuff. They used the panel data to try and figure out what was happening in the real world to their products. So, for example, if a company had a special sale on their product, or gave out coupons or something, they could use the panel data to see what the effect this would have, how much would their sales increase in response to a certain percentage change in the price level.

And I remember, there was an interesting regularity that I found, and that is, it would take longer for a sales technique to have an effect on the market share of the product—in other words, it would take longer to go up than to come down. So it would take a longer time to get people to increase sales of the product, than it would take after the sales was over to let it taper down. Now

one of the main purposes of what I was doing was to see not only what is the effect of prices on product, but, in a sense, the effect of habit, or, do you build up some kind of capital, sort of advertising capital, for a company in selling its product? It was the problems connected with this that actually led me to work with [Robert L.] Bob Graves to look at formal, mathematical analysis of a product where the past history will have some kind of an effect on what will happen in the future. And it raises mathematical problems that were typically ignored in mathematical literature, because there, it's really geared to physics and chemistry. It's not geared to the kinds of situations that you encounter in business and economics.

04-00:29:28

Burnett: And in human behavior. And so, we should maybe amend our description of the Chicago School of Business in this period, because although there's this investment in computing and quantitative analysis of business data, there is also this very catholic incorporation of different social-science disciplines. So there are people doing psychology research, there's industrial organization, sociology, all of that kind of stuff, and so it's real-world analysis of human behavior, and getting data from it, thinking critically about the data, subjecting it to formal analysis, and investigating whether there can be formal or formalized outcomes from this kind of research. And so that's what leads you to working with Robert Graves.

So, before we get there, I'm wondering if we can back out a bit and talk about some of the leading questions in the economic profession generally in the 1950s, and particularly with respect to the University of Chicago, because it's during the 1950s that so-called problems of monopoly become important in society. Generally, there's the Celler–Kefauver Act; there's interest in investigating perhaps monopolistic practices of corporations. And, the University of Chicago responds to this in the Economics Department and the Chicago School of Business, but, there's a long and significant interplay between economics research and monopoly practices, whatever the institution, going back a long time in history. Can you tell us a little bit about monopoly?

04-00:31:46

Telser: Well, one of the main reasons that I was doing this stuff were the two big textbooks, or two books on the problem: the one by Edward [Hastings] Chamberlin, [*The Theory of*] *Monopolistic Competition*; and the one on [*Economics of*] *Imperfect Competition* by Joan Robinson. And, George Stigler had given a series of lectures, I believe it was at The London School of Economics, criticizing the monopolistic-competition work, because they had not actually presented any data to support their theory or to test their theory. So, what I was doing, in measuring the response of market share to price changes and to competitive prices, was evidence directly relevant to the criticisms that George was making of the theory of monopolistic competition. And there was a general feeling among many people that there was a lot of

monopoly in the economy, and that this was very bad, and that we should do something about it.

Now, you already heard from Al [Arnold C.] Harberger that he wrote a very famous article to measure how much harm, in fact, there was resulting from monopoly, and to this end, he used the famous Harberger triangles. And what I was doing, which did not become as famous as the Harberger triangles, was trying to measure directly the elasticity of demand, even taking into account the effects of habit and so on, on the returns to these companies. Now one of the things that I studied that was directly relevant to this was cigarettes. There was a big antitrust case against, I guess it was American Tobacco [Company] at that time, 1911, I believe, and the result was, it was broken up into three or four companies. The general approach that the government took to these practices under the antitrust laws is to take a big company that was accused of being a monopoly, and to break it up into little pieces. So the Tobacco Trust, as it was called, was one of them. The [Standard] Oil Trust was another, and so on.

So I wrote this paper on cigarettes, and the case of cigarettes, what was interesting is, there was data on the effect of prices on cigarette sales, and especially during the 1930s when ten-cent brands were introduced. It caused a huge drop in the sales of the regular brands. And it's sort of interesting to keep in mind: I'm not going to swear to this, but I think I remember the price correctly, that a regular pack of cigarettes in the 1930s was twenty-five cents, which was a lot of money. Twenty-five cents was a little less than two hours at the minimum wage rate, or about the minimum wage rate. So the price difference between a regular pack and a ten-cent brand was very great, and the response of the cigarette companies wasn't so much to cut prices, but to increase advertising.

So the purpose of the advertising, which somebody like Henry [Calvert] Simons would object to, was to give people the feeling that, if you are smoking this brand of cigarettes, then you're like whoever — the hero or heroine — is smoking this stuff. So then, advertising became very important and certain slogans became very important, for instance: "It's toasted." All tobacco is toasted, but the guy who thought of saying that his brand is toasted made a lot of money. I think it was Camels. And then another slogan to get women to smoke: It was generally considered unladylike to smoke a cigarette. So Chesterfields focused on getting women to smoke, and I remember one of their slogans: "Blow some my way."

So this was criticized, and what I did find was that, although there was a strong effect of habit on purchases, the price elasticity was very high, and there really wasn't competition on the basis of price. The competition was on the basis of relative advertising outlays, and then you could get into other questions about whether or not this is desirable. If you wanted to defend it, you could think of advertising as sort of a cut-rate psychotherapy or whatever,

self-esteem buildup, stuff like that. And of course, there's another aspect to cigarettes that does get into the antitrust problem, and I think the evidence on this seem to be pretty strong in the government cases, and that was not on the sale of cigarettes, but on collusion among the buyers.

The buyers of cigarettes, the big companies, would buy them at local auctions in the areas where this tobacco was grown, and there seem to be pretty persuasive evidence that there was collusion among the bidders. I remember one, in particular. We don't have to go into it. It is not true that there was no collusion and that we had perfect competition or any of the sort. There was somebody at A&P who inadvertently left a telegram in his files that got subpoenaed, and it was brought in as evidence in a case. And the telegram, in effect, said that, "Well, we're going to be bidding in this particular market, so why don't you go someplace else?" In other words, you had not oligopoly, which refers to the sellers, but oligopsony, which refers to the buyers, and the lack of enough competition among the buyers.

Along these lines, I could mention another big project that I got involved in. It was because of Aaron Director. One of the economists at the business school—in fact, his office was across the hall from me—was John [S.] McGee. And Aaron Director was running a major research effort in which he had various people looking into practices that were considered anti-competitive. And so the question that Aaron would raise is: Are these really anti-competitive practices, or can they be explained on other grounds than in an effort to reduce competition? And in my case, it was the antitrust case against General Electric [Company, GE] and Westinghouse [Electric Corporation] light bulbs, on resale price maintenance.

04-00:41:36
Burnett: It is interesting.

04-00:41:38
Telser: Yeah. The article was published in *The Journal of Law and Economics* in 1960, and that same issue had a famous article by Ronald [Harry] Coase. So I think the most-read article in the issue was Ronald's, and mine is probably the second most read, but it's got a lot of citations. And Ronald and I have exactly the same complaint. Ronald's complaint was that people only read the first part of the article, when he talks about what happens if there are no transaction costs, so they think that he doesn't believe that there are transaction costs, and they totally miss the point of the article. And in my case, I talk about free-rider problems in the sale of new products. You go to a fancy store where you get instruction or you're told about a new product or how to use it or whatever, and then, you go to another sort of cut-rate store or mail wholesaler or so on. There's a very good example nowadays of a big company whose first letter is A, but I won't mention it. And so the free loader takes advantage of the cost incurred by the other guy.

So I examined the data—not so much the data, but the briefs and records. I should mention that the way this stuff works is, there are nine members of the [United States] Supreme Court, and each one usually sends the briefs and records of big cases that they heard to the law school from which they graduated. So that the U of C [University of Chicago] Law School, I don't remember which justice it was, but they would get this stuff. And so Aaron [Director] had a collection of these briefs and records, so I got the briefs and records for the antitrust cases of GE and Westinghouse.

And I remember, I was working on this in the summer of 1958. I had just gotten out of the [United States] Army in June. We were actually living in George Shultz's house, because we didn't have any place to stay when we first got out of the Army, and the Shultzes were in Washington. He was in some big job; I don't even remember what it was. Anyway, so we stayed in their house, and let's see. I guess Sylvia [Ruth Telser] was about six months pregnant, and I was doing this stuff, reading the cases, and because it was voluminous, I was dictating notes. So the notes were transcribed as I read all of this stuff, and then Aaron had some law students look at similar cases, because when you write an article in a law journal, you have to have a lot of citations to all the other cases—which, I have to say, I never read those cases. The student looked at them, would write down the citation, a very brief summary, and I would look at the summary and that's it.

Anyway, the bottom line is what I found: overwhelming evidence that there was collusion; that the reason they were doing this, it did not have socially beneficial effect. The purpose of the resale price maintenance was to prevent GE and Westinghouse from reneging on their cartel agreement, so that one guy would be secretly cutting his price to cut into the sales of the other guy. And because of the way the light bulbs were sold, resale price maintenance at the retail level—plus one other thing, the retailers had exclusive sales agreements. So that if you sold GE light bulbs, you did not sell Westinghouse, and Westinghouse did not sell GEs. So it was a very tight, well thought-out cartel agreement. And the case was in the early twenties, and I don't think it was until the late 1970s that the justice of the department seemed to have figured out what was really going on and made them stop, because they were able to continue. They were found innocent, or whatever they call it, of doing anything in violation of the antitrust laws.

And then, as I mentioned, in the panel we had on the Chicago School in October of 2015, Aaron Director became a very close friend of the family. As I mentioned then, and I'll repeat it now: he was an incredible person, and I described him as the equivalent of Socrates. Aaron would never tell you what he thought. His method was to ask questions, and you would answer the questions, and sooner or later, sometimes later, you would realize that he really knew what's going on. And the purpose of his questions was a pedagogical method to let you understand what was going on so you would

think that you had really thought of it. But it was really his questions that led you to the right answer.

04-00:48:03

Burnett: So the purpose of some of this research was to recast a lot of the conceptions of monopoly and its effects in the market. And so with respect to resale price maintenance, it was thought that it was retailer collusion, that they were forcing this onto the manufacturers. But the manufacturers benefited from resale price maintenance to make sure that the retailers, for example, would do the right kinds of things to sell that particular kind of product. So in other words, there was a relationship between the nature of the product and the services that were required to sell that product properly in the manufacturer's interest. And that, in and of itself, was not monopolistic practice, according to the findings of your research.

04-00:49:02

Telser: And the point that Aaron made, that most of the people who worked on this seemed to have missed, is, the usual argument for resale price maintenance is exactly what you said. It is thought of as a cartel among retailers. But in this case, the resale price was imposed not by the retailers, but by the manufacturers, so that violates the standard argument. So why are they doing it?

04-00:49:31

Burnett: And in the case of advertising, was there an argument or set of arguments that advertising was being deployed by industries that had a monopoly, or companies that had a monopoly position already, and they were using it to restrict entry into the market, that the research on advertising was an effort to sort of complicate the picture?

04-00:49:57

Telser: Well, that's one aspect of it, advertising as a barrier to competition, and the other aspect of it was that, by building up advertising—that is, the purpose of the advertising was a barrier to competition. And then, the way you could build up this barrier is by, you had a financial advantage, a big powerful company preventing the little guys from going into the business. And this, by the way, illustrates another problem that I think I wrote about, and this is also a problem that was suggested by an antitrust case, the New Orleans Times-Picayune case.[7] Now, it turns out that the rates that advertisers had to pay was higher for national advertisers like General Motors [Company] or Ford [Motor Company], and lower for local advertisers like local retailers. And so this ran counter to the standard argument that the big guys like General Motors and Ford, they're monopolists, and so that they should be able to force the newspapers to charge lower prices for their advertising; whereas, in newspapers, exactly the opposite takes place.

[7] United States v. The Times-Picayune Pub. Co., 1952.

And I also have to say, and this is kind of a funny thing: One day, a group of lawyers came from the *Chicago Tribune* ["*The Trib*"] to see me, and they said they had asked the people at *The Trib*, "Why do they charge lower rates for local than for national advertisers?" And the answer was, "Oh, they've always been doing this." Yeah, because *The Trib* started in 1850, and they were always—I'm exaggerating when they started doing it, but they had been doing it for a long time and nobody really knew why they were doing it. It's just one of the things that you did.

Anyway, the answer to this came in research in a dissertation by Jim [James Milton] Ferguson. And the answer is—really, I hate to say it, but it's sort of obvious. If you drop the notion that somehow the advertising is a hypnotic control that the seller has over the buyer, the answer is pretty clear. The local advertising is telling the readers of the newspaper what's on sale this week, and what the prices are. They're getting useful information, and typically, this is the advertising that comes from the middle to the end of the week. Whereas the national advertising is sort of the general B.S.: "This is a great car," or whatever, and you're not really learning anything particular about an automobile. Well, a Chevy has four wheels. What other car can you think of that has four wheels? Et cetera.

So that, it attacks directly the notion that advertising is not informative, and on the contrary, it shows that it is informative. And not only that, but one of the things that Ferguson found was that you could look at the daily pattern of sales of the newspapers, and you would see that the sales go up on the days when the retailers are typically putting stuff on sale, and on the other days of the week, the sales of the newspaper, the circulation of the newspaper, is a little bit lower. So there's direct evidence besides the rates themselves about the role of advertising in a society. Of course, anybody who writes anything that defends advertising acquires a certain opprobrium.

04-00:54:40
Burnett: Did that happen to you?

04-00:54:42
Telser: Yeah, to some extent.

04-00:54:44
Burnett: Well, so, there's another article that you wrote, and I think it's '65, and we're talking, generally, between 1960 and '65 is when you're working on this stuff. And in 1965, you write an article about the relationship between market concentration of companies, and the percentage of their outlays that they spend on advertising. Can you talk a little bit about that, and how that relates to the conventional wisdom regarding monopoly?

04-00:55:17
Telser: Well, the usual argument, one of the usual arguments—I think we brought it up already—is, if you think of advertising as a barrier to entry, then that

directly implies that, in a more concentrated industry—in other words, in an industry where the big firms have a big share of the market, that is, assuming these are industries that are selling consumer products—that they will also be spending relatively more on advertising. So the argument implies as a testable implication, a positive correlation between the advertising-to-sales ratio, and the level of concentration in the industry, and it turns out that, that's kind of a tricky thing to look at. And the reason it's a tricky thing to look at is, as in many empirical problems in economics, there are many other variables that are involved, so that it's very hard to figure out whether this particular proposition can be clearly distinguished apart from the other factors that might be exerting a similar effect.

Now, the data that I—well, first of all, and it required getting special census data from the government statistics to look at this, because you needed to get concentration ratios for appropriate SIC, Standard Industrial Classification, groups. So we had that, and we also had advertising-to-sales ratios. I think that was from the IRS [Internal Revenue Service]. It was somewhat difficult to get the data. We got it. But, the other problem was the effect of the other variables. And I had data for several years; I had cross sections of companies that were important advertisers in different census years. And now, one of the techniques to use in studying this sort of thing is to see whether there are changes in the same industry from one census year to the next. So that when you look at the results for one census year, in what ways, if any, does it differ from what you would see in previous census years?

As it turns out, this raised a different statistical problem. So it led me into a certain area of estimation theory, and it led to an article that was eventually published in the *Journal of the American Statistical Association*.[8] It turns out, that technique is generally useful when you're dealing with panel data. Essentially, you're looking at the same observations over time, and there are missing variables, so it's a technique that enables you to take into account the missing variables that you can't observe directly, but, it controls, through their effects, so that you can see what's really going on in the relations that you're looking at.

Anyway, what I found was that the advertising is not really so much a barrier to entry as it is an avenue of entry. That is, if you're a new company and you want to break into an industry, the way you usually do it is to try and get people to use your product that haven't done before, and in order to do that, you have to advertise. So what you're really finding, in many cases, is the effect of success and failure. Well, basically, if you fail, you sort of disappear. It's like the story I was telling the other day about the missing bullets. To see what's really happening, you can't just look at the ones that are successful. You also have to look at the ones that are unsuccessful, and you can try and

[8] Lester Telser, "Iterative Estimation of a Set of Linear Regression Equations," *Journal of the American Statistical Association.* 59:307 (Sept., 1964) 845-62.

turn things around. As an example of this, one reason that Standard had a big market share—

04-01:01:12
Burnett: Standard Oil?

04-01:01:13
Telser: —Standard Oil—that one reason that Standard Oil had a big market share in the late nineteenth century was because they had relatively lower prices. It was relatively lower prices compared to their competitors that gave them the larger market share. So that the real question that this raises is: How was it possible for Standard Oil to have relatively lower prices than the other guys, although Standard Oil is considered the archetype of the evil monopolist?

04-01:01:53
Burnett: The Octopus Trust.

04-01:01:55
Telser: Yeah. And the same thing. Well, I have to say that, in the case of cigarettes, there are these other elements that I would not regard as genuinely competitive factors, so I am unwilling to say that they somehow owe their success to the fact that they had a better product than their competitors.

04-01:02:22
Burnett: Reading the article, so many things occurred to me. When you're talking about monopoly, there are different kinds, and there are ways in which things wouldn't necessarily show up. So in your list of industries that you were using that had the concentration ratio, the four largest firms has a percentage of the total market, and then percentage of total outlays spent on advertising. Just to take an example of two industries: one was sugar, where the concentration ratio was really high, but the percentage of outlays on advertising was .012 percent or something. Which makes sense, because no one, at least then, no one needed to advertise sugar. Sweet things were downstream. There were other products that were made using sugar and that's what you advertised, so it doesn't show up.

And then in the other category would be something like drugs, where there's a low concentration ratio of, say, 25 percent, but the percentage of total outlays spent on advertising is like 8.7 percent, and that makes a lot of sense, right? And then, going back out in terms of the complaints about amounts of money spent on advertising and how that might be considered part of a monopolistic practice, you can think about the criticism in the 1950s of the enormous influence of the drug companies, even then, right, and that's what [Sen. Estes] Kefauver was concerned about that. And so they [drug companies] are spending, compared to the sugar industry, a ton of money on advertising and they have all those kinds of specious relationships with doctors and so on. That kind of stuff was happening even back then, and there were complaints about it.

So, in that local context, with that particular industry, the argument about a monopoly in advertising can make sense; whereas, for sugar, it wouldn't apply. Sugar was a concentrated industry, but it was protected by the government. [laughs] It was, effectively, as close to a royal charter as you could have in the United States in mid-century. And so there are different kinds of monopoly based on custom, based on various kinds of protection, and so forth, that always have to be taken into consideration when you're analyzing these problems. It's enormously complicated, and I got such an appreciation for that, reading that article.

04-01:05:11

Telser: Yeah, there is a tendency to think of, the way we put it: monopoly is some kind of an exogenous variable. And the point that I'm trying to make here is that the relative size distribution of the firms in the industry reflects fundamental forces that are at work in that industry, and that explains why, in some industries, it's more concentrated, and in other industries, it's less concentrated. So that you don't think of monopoly as exogenous, but you think of it as endogenous.

And then there's also, of course, the argument: A monopoly is bad. "I don't care if it's endogenous or exogenous, it's bad, so we've got to get rid of it." A serious person should not seriously consider that sort of an argument. If you're going to make a statement about something, you should at least have thought about it, and say something reasonably sensible. Let's put it this way: there's a lot of loose talk, or there's comments that are made for reasons that are really unrelated to the actual topic in a conversation. There's some other point that you're trying to make, and you use this as kind of a spurious point to support the other ulterior motive. And I think it is useful, but see, once you have the notion that monopoly is bad, then it becomes difficult to try and explain to people under what conditions this results.

So one of the things, for example, that Alfred Marshall did to make this point: he introduced the concept of a natural monopoly, so that, for example, in the railroad industry, it would be foolish to have many railroad lines connecting two points, regardless of the amount of traffic, simply because the result would be more competitive. It's obviously foolish, because what you would be doing is raising the cost without getting any benefit from it. If you think of competition as a device to get efficiency, then it does say something about, what is the optimal number and size of the firms that should be serving a market? And you don't think of it in terms of monopoly, but you think of the means to an end, and the end is to try and produce whatever it is that you want, at the least total cost. And that may mean few firms or it may mean many firms; it depends on what it is you're talking about.

04-01:09:16

Burnett: And, were there efforts by economists to question what is a natural monopoly? The electrical utility industry, for example, and they were doing

analysis to study, is that in fact a natural monopoly? Does it require state regulation or state ownership to function properly?

04-01:09:42
Telser: Well, in both the railroad industry and electric utilities, you have a network, and the network connects vertexes that are different markets, and there is a certain efficient pattern. In the case of electricity, now I'm trying to remember—[Gustav Robert] Kirchhoff's law. There's Kirchhoff's law that describes how electricity will flow through a network of wires, and in fact, this is one of the things that the RAND Corporation was trying to figure out in their support of the study of the railroad industry in the United States in the fifties. There's a certain relatively efficient network of railroads, and so that there would be certain inputs, it would be wasteful to duplicate, and in those situations, you could say that there is a natural monopoly.

In other cases, there is a natural monopoly because the ability necessary to do this stuff is rare, and there are very few people who are capable of doing this supremely well. So, for example, you could say [Wolfgang Amadeus] Mozart is a natural monopoly.

04-01:11:27
Burnett: On doing Mozart compositions, yes, definitely.

04-01:11:30
Telser: Now there would be some argument about whether we like the music or we don't like the music and so on, but, in the market described by a certain criteria, you could say that there are certain people, certain composers, who are much better at doing this than the others. So, they have special talents. It's less controversial when you talk about this in sports, like basketball or baseball. So nobody is going to start arguing with you if you say, "Ted [Theodore Samuel] Williams had a natural monopoly." As far as I know, his batting average has not yet been surpassed.

04-01:12:17
Burnett: Well, there's certainly no end of complaints in the history of universities about executive compensation for coaches, for example, who get paid more than the presidents of the universities, [laughs] so it doesn't stop people from complaining if they don't like the monopoly price that they are able to command. And, there's another history, I think, to the question or problem of monopoly with respect to the University of Chicago, and that is that some of these actors are involved in a kind of political project that's outside of the University of Chicago, but closely tied with it because of the members.

And so, for example, Milton Friedman, Aaron Director, Friedrich [August] Hayek, and others, joined the Mont Pelerin Society in 1947, and that's its founding meeting, and they're devoted to the promotion of liberalism in society because they feel that it's under attack. And one of the things that's discussed is that it is important to defend liberalism as a political and

economic concept or principle, and to do so through a number of avenues—some of them were journalists and they would do it through the press—but to write serious, academic, scholarly work that would investigate the assumptions that people had about state-led society, for example. I don't want to put words in the mouths of people, but roughly something like that. And there's one other instance. I suppose it would be the Walgreen Fund, which George Stigler was in charge of. And the Walgreen family, one of the reasons for the fund is that they wanted to protect the University of Chicago student from undue indoctrination into Communist principles.

So there was a political valence to some of the extracurricular activities, if you want to call them, of some Chicago scholars that had an impact on some of their work. Aaron Director was supposed to write an American [version of] *The Road to Serfdom*, and reasons that are unclear, he didn't, and Milton Friedman ended up taking up that mantle independently in writing *Capitalism and Freedom*, which comes out in 1962, and which is a public intellectual book about the relationship between political and economic liberty, let's just say, to be succinct. And so, what was your experience or understanding of that activity? Some people just never talked about it and don't know anything about it. Some people knew more than others. What is your take on that history?

04-01:15:32

Telser: Well, I am sympathetic to having, in effect, freedom of speech, and allowing diverse views to be heard, and right now, it seems that there is a lot of pressure, especially in the university communities to suppress opposing viewpoints. One of my favorite magazines, by the way, is *Commentary*, and in the current issue, in fact, there's quite a few articles, about twenty-five or thirty articles, about freedom of speech. And one of them, I think one of the best, is by Richard [Allen] Epstein, who was at the law school at the U of C and is now, I think, at NYU [New York University], and he's one of my old friends, and I think he presents the argument clearly. Let me put it this way: You can be a protagonist; so, you can take one of the sides in a debate and say that you have a right to be heard. But, the attitude that I would take is not so much about this side or that side, but that you should have a framework that allows these opinions to be held.

And in fact, what Richard makes the point is, or the main point that he makes is, that there really shouldn't be coercion, that there are certain rules that regulate how these debates should be carried on, and that does not include violence or yelling or shouting or all of these other techniques that have been used to suppress people whose opinions you don't like. Freedom of speech and all of these things are subject to rules. And you have to ask yourself, what are the sensible rules to apply in these situations? One of the most famous is the one in an opinion of Oliver Wendell Holmes [Jr.]: You do not have the freedom to shout, "Fire!" in a crowded theater, and that sort of makes the point. There are circumstances under which you can express certain opinions,

and there are circumstances you are not allowed to express those opinions because of the harm, the clear case that harm would result. You have to allow people to discuss things.

Now what I'm not really sure about, let me put it this way: I think that what I'm describing now is the position that Milton and Aaron would take. Now, you have a third person that you mentioned: Hayek. Now first of all, I should say, this is sort of a personal thing. I happened to be, I think I was in Detroit or someplace at the time, and Hayek was coming as a visitor to the U of C. It was after he had left and had gone back to Europe. And Ronald Coase, who was in Hyde Park at the time, I think it wouldn't be inaccurate to say, he was stuck with being Hayek's host. And I remember, he phoned me in Detroit, "Hurry back," because basically, what would happen is that he shunted Hayek onto me. So I had to spend, I think, a day or two as the host of Hayek. And to some extent, Bob [Robert Emerson] Lucas shared it with me, but I did most of it, and Lucas did some of it.

And Hayek, I remember, gave a talk, and I think I might have introduced him, and it was about banking in the United States. And it was really very embarrassing, because he was quite ignorant of what had gone on in the early nineteenth century on free banking in the United States. It was very embarrassing. And so, I do not consider him in the same class as Aaron and Milton, even though I do think that he wrote a very important and useful book. But on his economics, he is not very good, and I don't think it would be unfair to quote Abba Lerner. There are two people who were in Hayek's courses at LSE: Nicky [Nicholas] Kaldor, and Abba Lerner. And I'll tell you what Abba Lerner once said to me. He said, "When Hayek would give his exams, I would answer the questions in two parts: one, the answer that Hayek wanted; two, the correct answer." [laughter] Now Milton, I think, actually said in print, so I'm not distorting anything, he did not have a very high opinion of Hayek as an economist.

Another thing is, I remember once, I was pretty good friends with Earl [Jefferson] Hamilton. In fact, we were very, very good friends, and I once asked Earl, "Why is it that Hayek wasn't in the department?" I don't think I'm divulging a big secret—and Earl said, "Well we didn't think he was good enough." So, I think that the attitude that Milton and Aaron would take is about freedom of expression, and that they would separate that from their own opinions on what they thought was desirable.

04-01:23:03

Burnett: And so, that dovetails to some degree with Arnold Harberger's understanding of the Mont Pelerin Society, and so that there was a participation and that they felt that it was good to support it, that the ideals of the organization were positive ones in their view, but it wasn't a star chamber of the elite planners of future society.

04-01:23:43

Telser: I have to be honest about something. I was asked to join the Mont Pelerin Society, and I refused. Merton [Howard] Miller, who was asked at the same time, he accepted, and Merton and I were very good friends. And I would say, I will say, the reason I refused was because of some people that are in the Mont Pelerin Society for whom I do not have much respect. And Jacob Viner was not a member of the Mont Pelerin Society, and although it is true that Frank Knight was, I think he is sometimes regarded as one of the founding members, and maybe he was, I don't know, but he certainly wasn't in that mold.

And the last thing I should say, to be honest with you, is, I wrote a book review in the *Journal of Economic Literature*— it was a couple of years ago. And it was a review of a book about the Mont Pelerin Society.[9] I'm afraid I don't remember the title of the book or the issue that it appeared, but I did not have a very high opinion of the book. It wasn't so much about the Mont Pelerin Society; I just thought that the guy who wrote the book did not do a very good job to a reader who would be interested in learning about the Mont Pelerin Society. But at least, I should say, that I did write a review of the book.

04-01:25:40

Burnett: Okay. Well, you didn't write a lot of political statements in your career. You see yourself: You're an economist, mathematically oriented; you're thinking about the thorny intellectual problems of relating formal, mathematical principles and tools to real-world problems.

04-01:26:08

Telser: Absolutely.

04-01:26:11

Burnett: One thing that I thought was interesting is, in 1965, you wrote an article for *Challenge* magazine, "The Case for Competition," and you argue that competition is necessary for political freedom, and that political freedom is necessary. "Without it, individuals," and I'm quoting here, "individuals cannot freely criticize government policies, and minorities cannot flourish." And I was struck by that, especially because of your recounting of your family history. Can you reflect on that a little bit? Is there something particularly poignant for you, for your family background? What's happened in world history in the twentieth century that makes the case for competition particularly acute for you?

---

[9] Lester Telser, "Reviewed Work: *The Great Persuasion: Reinventing Free markets Since the Great Depression,"* by Angus Burgin, *Journal of Economic Literature* 51:3 (September, 2013): 883-85.

04-01:27:11

Telser: Well, I think it's that my family suffered a great deal, originally, from being in a society where there really wasn't very much freedom, and life was very difficult as a result. For example, when World War I broke out, my mother was ten years old. So from the age of ten to the age of fourteen or so, she had to live through, to say the least, a very difficult period in a very difficult situation when the family, her mother, sister, and brother, moved from a border town in Germany into Białystok in the interior of Poland, then controlled by Germany, and there was always a feeling of constraint. There was always the feeling that you really didn't have much freedom and you had to be very careful.

And of course, with my father, certainly, until, well, I would say until the end of the war, after he was released as a POW [prisoner of war] into his sister's custody, and then later, when he and his parents went to Belgium. Belgium was, of course, much freer than Memel, which was then occupied German and occupied by France. Again, you had to be very careful. So he was also in a circumstance where there was a lack of freedom, and the change to the United States is just unbelievable. Even in our own experience, Sylvia's and mine, it may sound funny, but living in Belgium for a year. And Belgium is certainly a western democracy, but you do not feel as free in Belgium. In fact, I don't think you feel as free anywhere in the world where we have lived as you do in the United States. And so, it's something to be cherished, and it's extremely important, and our personal experiences reinforce it in general. Personal experiences, I think, are more important than reading about things.

04-01:30:15

Burnett: I think few people would disagree with that. In the other part of that article, it returns much more to the conclusions you've come to as a result of your research. One of the things you generalize—and this is based on several research projects you'd undertaken—that you had noted with some dismay the failure of competition to increase despite the growth of markets and opportunities for new companies to start. I think you state directly that antitrust laws have had no impact on competition, good or bad, and they were supposed to. They were supposed to increase competition, and you've concluded from your research that they don't have much of an effect.

And I guess the other area, when you're talking about competition, you argue that a solution to this state is that monopolistic union practices should be subject to antitrust prosecution, and that we should get rid of protectionism, and encourage foreign competition. That was your solution to this problem of the lack of competition or competitiveness in the United States. Can you reflect on that, what you were thinking at the time? And it certainly dovetails with what others at Chicago are writing. Was that a controversial thing to say at that time?

04-01:32:05
Telser: Well, certainly it wasn't controversial at Chicago, [laughter] but I think that there were certainly places, or let's say, economics departments, where that would be considered very controversial, and there were people who would disagree very strongly. And I can remember incidents. For example, well, once, at a dinner party in Cambridge that Henk [Hendrik] Houthakker had, I was a speaker at his dinner party and he had some Harvard faculty there, and they were pretty hostile. I don't think they knew very much about me, but they were just hostile.

Then I remember another occasion—if I may say so to a Canadian, but then of course, I do have a Canadian wife, so it's okay for me to say it—and that is: It was in Toronto, and I was at a conference. I was giving a paper. I still don't really understand it, but this guy was really very hostile simply because I was from the U of C. And, in fact, I remember what he did is almost childish. When I was going up to the stage, he tried to trip me.

04-01:33:37
Burnett: Are you serious?

04-01:33:39
Telser: Yeah. So, people do get sort of personally riled up about this. Now, in the case of labor unions, the problem—maybe it's not a problem now. I'm not sure, but there was a real problem at that point and earlier, that some of these unions had been infiltrated by criminals. And I didn't mention one of my cousins. I was very close with him, Stanley, first cousin. He was a doctor, and he had a very close friend, who was a lawyer, and he was a lawyer for a union, and he was murdered in a parking lot—literally, shot and killed in a parking lot. It was not an attempted robbery or anything. It was some kind of, I guess you would call—

04-01:34:55
Burnett: A vendetta or something?

04-01:34:56
Telser: Well, I think there was some kind of a conflict going on between different wings trying to take over a union, and he was on one side, and somebody on the other side hired a hoodlum to rub him out. Then of course, there are some very famous cases involving murder and violence and so on. So that the unions did not just operate as an attempt to certify ability, but some of them were really pretty criminal organizations. One of the problems I think we have now with unions, and this came up in Illinois just within the last month or so, is about unions of government employees, where there certainly is coercion that exists, that does not exist in private unions.

So that there are problems with this sort of thing. I think the general point is that we need rules, but it's very hard to have very good rules, and you need an accumulation of experience and sensible analysis of this, or understanding of

this experience, so that you can figure out what should be the appropriate rules. The experience that I have had along these lines is in the development of appropriate rules in organized markets. It's taken a long time to work out rules so that the markets function reasonably well with understanding. And sometimes, what has happened is, rules have been imposed that are very foolish and mistaken because of failure to understand what goes on in a market, and how they need to work. There are very simple things that have to be done to make sure that a market works reasonably well.

04-01:37:37

Burnett: You mentioned criminality, and there were some cases in the late fifties where that really came to the public attention, that there was infiltration of large labor unions by criminal organizations. But there are economic discussions about the benefit or cost of unionization of an industry, and that's happening during the 1950s too, and, if I understand it correctly, there were arguments for unionization that are incorporating Keynesian arguments about the stimulation of aggregate demand. You pay people more; they buy more goods and services and it stimulates the economy.

So it's a form of fiscal stimulus, in a way, to have an organized labor force that's able to exert a kind of cost pressure that not only stimulates demand—the analogy of the Ford employees purchasing Fords and that kind of thing—but also stimulates innovation because their high wages are a cost to the company that forces it to innovate and introduce labor-saving devices. So, was that part of the Chicago response to this, what Chicago economists understood as a problem of monopoly with respect to labor?

04-01:39:17

Telser: First, while I still have this in mind, I did find, for example, there is a positive relation between average wages and concentration ratios, that industries with higher concentration ratios generally have higher wages paid to their employees. And that's an interesting fact, and at least, it's not obvious to me how you can explain it. And then on the part of unions, I mentioned the fact that there are some unions that seem to have been taken over by criminal elements. But there are also occupations where it seems to me the workers were treated and maybe are treated very badly, for example, in coal mining. I think that John L. [Llewellyn] Lewis was very beneficial in helping the workers, the coal miners, from being exploited.

Here you are in a situation where you're typically living in a small town where the opportunities of other kinds of employment are not very good, so that there's a sort of a monopsonistic element on the part of the employer, and the market really isn't working very well. So there are circumstances when the response has been to try and make things better by having unions, and I think, in some cases, they were reasonably successful. And there are other occupations like this where the workers in the companies and so on did not have very good opportunities, and the only way they could try to improve is to

go someplace else and get a job in something entirely different and so on, and we should not underestimate the cost and the difficulties of doing that sort of thing.

So I think you have to have a sense, and be reasonable about what kinds of rules are justified and appropriate in different situations. So the fact that there may be bad elements in some cases should not lead you to conclude that the answer is to just get rid of the whole thing. It's more difficult than that. It's also difficult, or it's a mistake, to overstate how well rules can operate to correct problems. These are very tough situations. And actually, this is another one of the points. I had the benefit of having a really great teacher, Frank Knight, and you learn certain, general things that are not taught in an economics course. For example, I haven't mentioned this, and we're past the time when I was a graduate student, but let me say a couple of things about Frank Knight's course on the history of economics. I don't think I talked about—

04-01:43:15
Burnett: You haven't.

04-01:43:15
Telser: Okay. Now, one of the books he assigned—he didn't assign it. He asked us to read *The Brothers Karamazov*, and in particular, the chapters on the—what was it called? "The Grand," "The Inquisitors"—

04-01:43:35
Burnett: "The Grand Inquisitor," yeah.

04-01:43:37
Telser: The Inquisitor chapters. That was very important for us to understand what goes on in these situations, and certainly, there are few who could equal [Fyodor] Dostoyevsky's power of exposition. Another book that he recommended was a book by Edith Hamilton, *The Greek Way*, about ancient Greek civilization. Now, of course, they had slavery, yes, there were lots of things wrong with the ancient Greek civilization, but certainly, compared with what else was around, they are superior, way above a lot of the other things around, and Knight wanted to make sure that we understood this.

There is a collection of his essays on morality and ethics and so on, and when you are an economist, you should really know about these things. It isn't just a question of writing, bouncing a check or things like that, or stealing something from a grocery store, or whatever. There are very general principles of morality and ethics that we rely on, and some of these questions that we have, or that you have raised, don't have easy answers. You really do have to rely on having the right people capable of dealing with these problems. And if you don't, it's very bad.

04-01:45:49
Burnett: It seems that, when you were describing Frank Knight's course, it made me think of Robert M. [Maynard] Hutchins and his "Great Books" orientation, that he wanted to make sure that University of Chicago students were very broadly trained in the best that what he saw as civilization had to offer. I wonder if that influenced Frank Knight in his choices.

04-01:46:18
Telser: Well, it's sort of funny. Now, I would guess that Frank Knight did not particularly like Hutchins, and Ted also did not, particularly like— Hutchins may sound very good if you had no personal contact with him, [laughter] but he could be a real pain. Let me put it, he was not charismatic in the same sense, let's say, that Henry [Agard] Wallace was charismatic, but he did have good ideas. And there were the "Great Books" and so on. But that did not affect me as a graduate student in the Economics Department.

04-01:47:03
Burnett: Well, we're in the 1960s now, and we've talked a little bit about Robert Graves. Can you talk about the context for your collaboration with him, how that got off the ground? And it's around 1963, is that right?

04-01:47:28
Telser: I think that's right. I was in a course with Milton. I don't remember if I mentioned this. Bob Graves was teaching—

04-01:47:39
Burnett: Yes, you did.

04-01:47:40
Telser: Yeah, okay. So that was—

04-01:47:41
Burnett: The computing course.

04-01:47:41
Telser: —my first contact. Now the second point about Bob: He's an extremely nice person, very affable, a pleasure to be with, so he could put up with me. And for example, I'd be at the board and I would write down something and make a mistake. He wouldn't jump on it. He would say, "Um, now, uh, shouldn't that be such and such?" or something like that. So he was very good about it. I think the important point is that he was genuinely interested in applying the suitable mathematical analysis to real economic problems. I think I mentioned that George Stigler came to [Abraham] Adrian Albert with a problem about solving linear inequalities, and Albert said, "It's trivial." And often with these mathematicians, you don't get to first base with them. You tell them about the problem, and it's like talking to the wall. But with Bob, it was very different. He had a genuine interest in the problem.

Now, it turns out that these particular problems were very closely related to what he himself had been working on. I guess the first direct, professional

contact was in programming, when I was doing this stuff on market shares and prices and so on. It required writing some programs to handle these things, so I became involved with Bob at that point. The mathematical problems involved in studying the econometric relations are: What do you expect should happen? Or, if you treat this as a mathematical problem, what kinds of relations should you expect to see? It's a little bit in the class of differential equations. It's actually in an area that is called in mathematics functional analysis, because what you're looking for is a function which is a whole, a complete relation that is appropriate for solving a particular problem.

This comes up in something called integral equations, which happen to be one of the problems that Bob worked on in his dissertation. And the problem is, in economic terms, you want the present value of profits, let's say, from some sequence of sales or production or output, and there's a production function that describes the relation between inputs and outputs, and it requires choosing certain things or certain inputs at one point in time that will not emerge as outputs until a later point in time. So what you're looking for is the functional relationship between the inputs and the outputs.

Now, a crude version of this problem was in the Soviet Union in their five-year plan. A five-year plan is essentially an approximation to the solution of this kind of a problem. And now, if you try to make it precise by describing what the underlying constraints are—and so what you want to do is figure out the best strategy, taking into account the underlying constraints—you very soon get involved in some deep mathematical problems. And so this is part of the beginning of our relation. And then what was closely related to this is studying what is the optimal—well, it's related to, let's say, the growth rate of an economy. What is the optimal investment policy? What is the optimal path that the stock of capital should take?

And then a very funny thing happens. In the articles on this, some people describe the relations assuming continuity, and others describe the relations in terms of difference equations. And, when you do it in terms of continuous differential equations, then the solution says, "Well, if this is where you are, and this is where you want to go, then you should go there instantaneously." Whereas if you model the same problem using difference equations, it doesn't say that at all. It describes the sort of the slow path that will take you from where you are to where you want to go. And if you want to describe what's going on, what it means is, that in one form, the mathematics says, "It doesn't cost anything," that the rate at which you change your mind and you move instantaneously from here to there is free. You can do it as fast as you want. It doesn't matter. Whereas with the difference equations, it said, "No, no, no. You have to take into account the cost of this path versus that path." Well, once you start talking about what is the best path to take, you are talking about a problem in functional analysis.

04-01:55:35
Burnett: And is that what becomes the continuous and discrete time approaches to a maximization problem?

04-01:55:41
Telser: Yes. If you're talking about a real economy, the actual economy, you have to do it in terms of using the best tools, let's put it that way, or the correct tools.

04-01:56:01
Burnett: Right, right. And, it's a class of problems. A firm seeks maximum present value on its return over an infinite horizon at a given interest rate. That was what you were looking at in that, starting to do that. Now, you got some support for this, financial support, from an institution. Can you talk about that, and how that came about?

04-01:56:28
Telser: Financial, well, the support was from—

04-01:56:31
Burnett: From the National Science—

04-01:56:32
Telser: Yeah, now here's an interesting fact about the National Science Foundation [NSF], which goes back to your interest. We'll just briefly touch on it, because I don't really know that much about it. But I can say this, that one of the people, one person who was very important in getting NSF established was Ted [Theodore William] Schultz. You knew that.

04-01:56:59
Burnett: Getting the National Science Foundation established?

04-01:57:01
Telser: Yeah.

04-01:57:02
Burnett: I did not know that, no.

04-01:57:04
Telser: That is my understanding. I'm not an expert on this, but this is my impression, and it really came about partly in connection with the dissertations in Chicago. He wanted to get support for good research from a government agency based on the value of what they were going to do, and not with an eye on the Cold War, let's put it that way. And NSF, that was the first—I think this is true—it was the first institution, government agency, that had a broad goal for economic research. Now, I also have to say, I was a member of the panel, I think, for two years or so.

04-01:58:12
Burnett: Of the NSF panel on adjudicating economic research projects.

04-01:58:17
Telser: Yeah. I think the panel had, I don't know, six or seven or something people on it, and we would meet, I think it was about every, certainly every three months on that order. So we would meet in Washington every three months, and before that, we had gotten a huge number of research proposals, and we had read them, and when we came to the meeting, we would have to decide which ones deserve support and which ones don't. Now, at first, when it was starting out, merit was primary, but it will not come as a big surprise to you if I say that it didn't take very long before politics began to enter, so that there would be pressure put on the panel to make favorable recommendations on certain proposals—not because of their merit, but for political reasons.

And in particular, one I remember is, we had some proposals to support research on supersonic transports. I was involved in this on another occasion, but this one came up when I was on the panel. And the chairman of the Council of Economic Advisors at the time said, "The president"—and the president at the time was [Richard M.] Nixon—said that, "The president would like you to favorably recommend, or to recommend support for this particular proposal." I could not describe, without using impolite language, what the reaction of the panel was, but it was a very clear case of trying to force politics into it to support a bunch of people out there, I think it was in Washington or someplace, where they had a big airplane industry that was in trouble, and this would just be a way of giving something for the aeronautical engineers to work on, that sort of thing.

And then, in general, what began to happen is: "Well, there are too many proposals in Massachusetts that are being supported. Why don't you have some proposals in Idaho or someplace? You should spread the money around in different states," and that sort of thing. And, of course, I haven't been involved with NSF in more than forty years, so I don't know what's going on now. But the trouble with these government organizations is it's very difficult to keep them on the right track, and to prevent politics from entering and screwing them up. Let's see. I guess I was getting support for this before I was on the panel, definitely before I was on the panel.

04-02:02:17
Burnett: Yeah. The research is, yeah, in 1963—

04-02:02:22
Telser: And I was on the panel, I think, '72 or something.

04-02:02:26
Burnett: In the terms that you describe, that was the justification for the financial support. You explained it as this kind of problem, we need to understand; we need to do research and understand on this problem, discrete and continuous time, the use of differential versus difference equations.

04-02:02:48

Telser: Of course, also, because it involves doing things now that will take effect later, you get into a technical problem that is actually very closely related to a problem that came up earlier during World War II, when you're aiming things to shoot down airplanes, and Norbert Wiener and [Andrey Nikolaevich] Kolmogorov had worked on these problems. It's very similar. You are observing the flight path and then you're trying to figure out, where will the plane be when you open fire? And so you can describe this as a set of equations, and they're Euler equations, and it raises some very interesting questions about factoring and so on. It gets into some pretty deep mathematical waters.

04-02:04:07

Burnett: It's no coincidence, in a sense, MIT's [Massachusetts Institute of Technology] Rad Lab [Radiation Laboratory] was doing work on gun laying; the Statistical Research Group [at Columbia University] was looking at the timing of proximity fuses. It comes directly out of that war research that you get the postwar economic research program writ large, at those institutions, at Chicago, MIT, and RAND, that are doing that work.

04-02:04:31

Telser: Yeah, absolutely. This is literally the problem that a business firm faces. It's a very general problem that they face, and they try to deal with it as best as they can. There's a funny thing about this I should mention. In doing this, the difference equations, if they're involving probabilistic elements, are Markov equations, named after a Russian mathematician, [Andrey Andreyevich] Markov. And I used Markov equations. And there was an article in some Russian—I don't know where it was—that argued that this method, that had been applied for the noble patriotic war and was now to be used in selling cornflakes, seemed obscene. And somehow, Joan Robinson was visiting. She spent three weeks visiting U of C at the time. I think Harry Johnson, who knew her well, brought her over. My relations with Harry were very close. He brought her over, and Joan Robinson took a shine to me because I was attacked by this Russian idiot for "misusing" Markov equations for sordid economic purposes.

04-02:06:21

Burnett: [laughs] Well, it's often said—what is the old textbook Econ [Economics] 101 definition of economics? The application of scarce resources to different ends, or, it's this allocation problem. But, in talking with economists about this postwar period, it makes me think of economics as a science of decision making through time, that these factors mature at different rates, and they're interrelated and they're interdependent. And so yeah, it is an allocation problem, but time is this incredible variable that's so important. Well, perhaps we should pause there and we'll continue next time.

04-02:07:21

Telser: Okay.

Interview 5: July 14, 2017

05-00:00:27

Burnett: This is Paul Burnett interviewing Doctor Lester Telser for the Economist Life Stories Project, and we are here in Hyde Park, Chicago, and it is July 14, 2017, and this is our fifth session. We were talking initially about beginning collaborations with Robert ["Bob" L.] Graves, and you are fond of collaborating with people, it seems, and there is another figure, economist, that you started to work with named B. [Basil] S. Yamey. Can you talk about how you met him?

05-00:01:21

Telser: Well, he was visiting Chicago. I don't remember what his official status was. He wasn't a post-doc. I think he was visiting Chicago, and it was not for the whole year, but maybe a quarter or so. And we had common interests, one of which was in resale price maintenance, but we didn't really talk very much about that common interest. The main common interest that we had was on futures markets, and I believe we did a note together on margins in futures context, and why there are margins.

Basically, the purpose of a margin is to protect the broker from loss if the price is adverse to the customer's account at the margin. And in American futures markets, as distinct from British futures markets, there's a limit price rule, so that if the price change gets up to a certain limit, it can either be an increase or a decrease, but if it gets up to a certain limit, the trading stops. And the purpose of that is to give time for the broker to contact the customer and get more margin. Otherwise, the broker will close out the contract. And closing out the contract means that if the customer is long, the broker sells, and if the customer was short, the broker buys, and whatever loss is incurred is a loss to the customer and not a loss to the broker, so that the size of the margin depends on the limit price rule. The bigger the limit price rule, the larger is the size of the margin in order to protect the broker from loss that might occur in the interim before the margin could be replenished.

So the point is, it explains the margin as a function of what is necessary, especially when there are people who are trading in the market, but who are not actually present on the floor, so that their instructions have to be conveyed to the representative on the floor, which means there's going to be a time lag. And it is because of the time lag that a margin becomes necessary and these rules have been developed. Some people regard the limit price rule as interfering with the freedom of the market. Those are not economists or traders who understand what really goes on in the market. I don't want to make derogatory statements about some of those people, but, if you understand what's going on in the market, it makes perfectly good sense to have margins.

And then, of course, the problem that this raises is one of regulation. So for example, there were people who would say that, in order to inhibit trading in futures markets, because speculators destabilize the economy, the government should set margins. They should set margins high enough to deter speculation in trading, and that this will somehow be better for the economy. But those are people who really don't understand either how markets work, what their functions are, or the particulars of this relation between customers and brokers.

If you had a market in which the only people in the market were the principals—in other words, you were not trading on behalf of anybody else, but only on your own account—then, it really wouldn't be necessary to have either limit price rules or margins. But that is not the case. In order to have trading in a market by merchants who are hedging and whatnot, so that they are not trading and present on a market, but they're doing it, in effect, through employees, other kinds of rules are necessary to make it function reasonably well.

05-00:06:49

Burnett: I imagine that some of the regulatory concern about margin requirements had something to do with the story of the buildup to the Great Depression, that your average Joe on the street was getting into the market and was buying on margin. And so they were borrowing tons of money that they didn't have and could never repay, and then they get wiped out and then there's a cascading effect. And so, was that part of the original rationale, but that's not in operation in this case, where you have brokers and organized market.

05-00:07:26

Telser: I don't think it was in operation in 1929, either. That's sort of a popular account. There are people who write novels and fiction and so on about this sort of thing, and so, they blame the stock market crash in '29 on excessive speculation by people who really didn't know what they were doing. We could go into that, but maybe not now. I have written about this, so has Merton [Howard] Miller and others, and we generally agree that the problems in the stock market had nothing to do with margins.

05-00:08:16

Burnett: And I think you write in another article that it's in the broker's interest; the broker is the one who stands to gain or lose, and so, they have this interest. In absence of a margin requirement, the brokers would impose it to protect themselves. And so, in your view, and in view of others, too, it's an auto-equilibrating process, and it doesn't require this over-the-shoulder regulation.

05-00:08:44

Telser: And as a minor point, on one of the joint articles that I wrote with Harlow [N.] Higinbotham—it turns out that Harlow's father is a broker, or was a broker, and so he gave us data on margins that would not be publicly available, but it was very useful for us to see the relation between the size of the margin and

sort of the volatility in a market. It isn't just the limit price rule that governs the margin. What also is important is how much prices tend to vary within those limits in the course of a trading session. And so it may well be that on days where prices are changing a lot, the brokers will require more margin.

05-00:09:47
Burnett: Well, can you talk more about this collaboration with B. S. Yamey and some of the problems that you encountered, and interesting events that you encountered, along the way?

05-00:09:59
Telser: Well, of course we discussed this stuff about margins I was just talking about, and one of the things he was interested in was to see how trading occurs on the Chicago markets. Now the two biggest futures markets in the world, I believe, are the Chicago Board of Trade, and the [Chicago] Mercantile Exchange, called "the Merc." So, as it happened, we chose a day to visit the Board of Trade that was particularly dramatic. President Kennedy had been assassinated on Friday; I believe it was November 23, 1963. And we, Basil and I, visited the market on the following Monday and we talked to the traders. Now, one of the differences between our visiting and other people is that we were actually on the floor of the exchange; we were not up there in the standard guided tour. We were talking to the traders, and we had already published stuff on futures markets, so we were not considered amateurs. And, it was a very interesting story. I believe that Kennedy was shot about eleven o'clock in the morning.

Now, on the floor of the exchange, there are many tickers, news tickers: [The] Associated Press, Reuters, United Press [International]. All of the major news services have their tickers, and they're going continuously. And in those days, unlike now where you can get this on your computer, there were no computers, so the traders on the floor were in touch with whatever important was going on in the world relevant to futures trading, minute by minute. Oh, and then I should also mention the way trading is organized. On a board of trade, there's a pit, and the contract that you're trading in depends on what stair you're standing on in the pit. So there are the traders in the November contract, the May contract, the July contract, and so on. So when you are not busily buying or selling something, it was common for a trader to go over to a ticker, look at it, and see if anything was happening.

So in this case, a trader goes over to whatever it was, one of those tickers, and he comes back, and he says, "Sell 5,000 contracts," or something, a rather sizeable amount, and everybody turns and looks at him. He sells, and there are buyers, and then another guy goes over to look at the another ticker, or the same one, to see why, all of a sudden, out of the blue, nobody was expecting that there would be any interest in selling, and he comes back and sells and so on. They said, in the space of, at most, a couple of minutes, the price had

dropped to the limit, and then trading stops. And they found out afterwards that the news was that Kennedy had been assassinated in Dallas, Texas.

Then there was, of course, the turmoil. LBJ [Lyndon Baines Johnson] was sworn in as president, so a lot of things were happening Saturday and Sunday. Monday, the market opens, and it's as if nothing has happened. Trading begins, the price is where you would expect it to be under normal conditions, and everything goes on as before. And it was, for Basil and me, a very dramatic, real-world example of how these things work. Now of course, if the government had collapsed or whatever, God knows what would have happened, it would have been different. But it didn't, and things went on as before.

05-00:14:54

Burnett: So what does it mean to you? What's the significance, that you can see the stock price as a real brake on psychological – on the passions of human beings?

05-00:15:11

Telser: Yes. This is an example of one of the main arguments that [Friedrich August] Hayek makes, that when you have economic transactions taking place, the prices reflect the information available. It isn't a public opinion poll. It's not an election. You're not voting or anything like that, but whatever the interests are related to the events, they are reflected in the price. And the market under reasonable conditions, adjusts what you might say, rationally, to the effects of the news that comes in. And this is a dramatic example of the way markets should and did behave according to this theory or model of what is supposed to happen. I would say it was a pretty strong confirmation of standard economic theories about a free market.

05-00:16:37

Burnett: So, you worked with Basil Yamey on this and you published a paper on margins, and you continue to work with Robert Graves.

05-00:16:51

Telser: Yeah. The work—excuse me, go ahead.

05-00:16:54

Burnett: Oh, I was just going to say that we had talked about this last session; we had opened the discussion of Robert Graves. We didn't actually talk about quadratic programming as opposed to linear programming, and that mathematical piece of it is something that would be great to talk about, I think.

05-00:17:20

Telser: A good way to begin is to point out the huge difference between looking at a problem, a static problem in economics, and a dynamic problem in economics. If you look at the problem that Bob and I were working on in static terms as a quadratic programming problem, there's very standard mathematics to

describe the results. You have relations between variables, or if you put it in terms of a firm, let us say the firm is selling a large number of different commodities. And they may be related as substitutes or complements, as we say, but there are relations among these commodities, and they can observe the effect of, let's say, changing the price on one commodity, the effect that it will have on the prices of the other commodities that they're selling.

As a sort of a simple-minded example, suppose that you were selling bacon and eggs, which are often consumed together at breakfast. So if you raised the price of eggs, it might have the effect of reducing the demand for bacon, even though the price of bacon didn't change. So that, if you're in the business of selling group of products where these things are related, you have to somehow take into account how changes in one thing will have an effect on other things. Now under static conditions, it's easier. Under dynamic conditions, it's considerably more difficult, because you not only have to take into account the immediate effects, but you also have to take into account leads and lags, and the equations that are used to study these things, relate the current values, let's say, to the values of these variables in the past. And so, whatever it is that you're doing, you know that, because of whatever you do now, which depends on the past, it will have, therefore, repercussions in the future. But it's generally not true that the relation in the past will be exactly the same as the relation in the future.

Now the term that is used that would represent a complete equivalence between the past and the future is "symmetry." That is, if the numbers that you use to describe the relation, if those numbers are put in the form of a table, then the numbers would be symmetric. For example, if the number in row I, column J is the same as the number in row J, column I, so that there would be perfect symmetry, and it's a lot easier to look at that type of situation. Now the main formal model that was used up to that time is in a book by J. R. [John Richard] Hicks called *Value and Capital*, that was published, I believe, in the 1930s. He's an English economist. Another English economist who worked on this stuff more mathematically: R. G. D. [Roy George Douglas] Allen.

So there were standard textbooks that described this. But, when you're talking about dynamics, this doesn't work anymore; it's much more complicated. And so, the relations, because the present depends on the past, therefore you know the future will depend on what you're doing now. So whatever you do now has to take into account the effect of the experience and the likely consequences in the future, and that leads to more complicated relations than would be true in a static situation. And what I mentioned last time is that this is the sort of problem that arose during World War II, and was especially studied in the United States at MIT [Massachusetts Institute of Technology], actually, by Norbert Wiener, and in Russia by A. N. [Andrey Nikolaevich] Kolmogorov. And they developed relations that would allow observations from radar to be fed into computers or programs that would enable the anti-

aircraft and rockets to aim at the likely point where the enemy aircraft would be, based on where it had been in the previous couple of minutes.

So the problems that arose then would also apply to what the problems that Bob and I were interested in, which was to see what is the best way for a firm to price its products taking these into account. And this leads to mathematical complications that are simply not present in the ordinary case. There's a limit to how far I can go this way, because it leads to sets of what we call difference equations, and it would be too complicated. [laughs] I'm afraid to go into it, but let me just mention that, when we deal with these maximization problems, these are very formidable mathematical problems because first of all, you have to decide whether the objective that you're trying to maximize, whether it has an upper bound. And there's a least upper bound, an upper bound. Can it attain the upper bound? Is it unique? And so forth and so on. So, it doesn't take much before you end up dealing with very difficult, abstract, mathematical problems.

Our first foray into this led to an article that was published in *Econometrica* based on work that we were doing when I was at the Cowles Foundation and Bob was here, and we would correspond back and forth relaying our results and so on. The technical area of mathematics that deals with this sort of thing is called calculus of variations, and the first major results in calculus of variations were by Euler, Leonhard Euler, in, let's see, early eighteenth century. So he was a little bit after [Isaac] Newton, not much, maybe a generation after Isaac Newton. But oh, now we're talking about the really great mathematicians. Euler is one of the great mathematicians. He's sort of among the [Wolfgang Amadeus] Mozart group in mathematics.

05-00:26:49

Burnett: We did talk about this last session, that the mistakes that the economists were making was in using differential equations when the proper tool was difference equations. And I'll let others study those differences, come to their own conclusions about them, but basically, your point in at least one of these articles was that these models will produce different results if you use these different equations, and one is appropriate and the other is not.

05-00:27:24

Telser: The difference equations are appropriate when the events that you're looking at are subject to random variations, which is common both in the original applications in shooting down airplanes, and in our applications, unexpected things happen that affect the supply of inputs and a demand for the products and so forth and so on. So to treat the variables as if they are known for sure and how they will change for sure from now on is a big mistake. In your models, you have to take into account the fact that you're living in a world where unknown shocks or unanticipated shocks are occurring that affect what you want to do. So you have to take that into account. And, difference

equations are much better in doing this than models that assume there's perfect certainty and none of these things are happening.

05-00:28:39
Burnett: That's perhaps a good segue for talking about another happy, fortunate event in your life, in that you take a year at Yale [University] at the Cowles Foundation. And I think, in one of your notes, you mentioned that the NSF [National Science Foundation] support enabled you to do that work, and to take that leave or that sabbatical to do so. So, can you talk about why you went to the Cowles Foundation? Was it to continue this work?

05-00:29:33
Telser: Well, the Cowles Foundation was carrying on the work of the Cowles Commission, so my moving or going to the Cowles Foundation was a natural move given my previous experience at Cowles. So by going to the Cowles Foundation, I would hope to find an environment that is people with the skills and interests that would complement and support what I was interested in. There wasn't any other place that I could think of that would actually have those sorts of resources. So to me, Cowles was kind of a unique place to continue this kind of work. Although, some of the people who had been at Cowles, at the Cowles Commission, did not go to Yale. For example, Jacob Marschak went from Cowles in Chicago to UCLA [University of California, Los Angeles], and Gérard Debreu went from Cowles in Chicago to [University of California at] Berkeley. I'm not sure I can remember—

05-00:30:56
Burnett: Well, [Leonid] Leo Hurwicz went to Minnesota.

05-00:30:57
Telser: Leo Hurwicz, right, thanks. Leo Hurwicz was at Minnesota. Some of the original members of Cowles were in varied, different places. Well, Tjalling Koopmans was at Chicago and at Yale, and actually, aside from Tjalling, I don't think there was anybody else that I can think of offhand that went from Chicago to Yale. Although Jimmie Savage, who was at Chicago — he was not at Cowles but was in a statistics department — he, via Michigan, ended up also at Yale. But in general, there really wasn't direct correspondence in people, but then there were other people at the Cowles Foundation who were interested.

So, for example, my office was right next to [Herbert Eli] Herb Scarf, and so we talked a lot, and [Robert John] Bob Aumann, Robert Aumann, was also visiting at the Cowles Foundation, and he was an expert in game theory. Actually, both Scarf and Aumann were experts in game theory, so I did learn directly from Scarf and Aumann about game theory, things that I hadn't known before, and in particular, the most important topic that I got from what Scarf did is on the theory of the core, but we can come to that later. Scarf wrote a major paper that opened up aspects of game theory that were directly relevant to economics. Just to say a little bit, the typical way of thinking of a

game is, it's a contest between people, whereas, in an economy, that's only partly true. There is this combination of rivalry and competition, and so what is called "the cooperative game" reflects this, and the theory of the core is really the model that more accurately represents what, in my view, is the typical economic situation as distinct from the other view, the rivalry view, non-cooperative games.

Was Martin Shubik at Cowles at the time? I don't think Martin Shubik was at Cowles Foundation at that time.

05-00:34:20
Burnett: But he was important in this elaboration of these—

05-00:34:25
Telser: I think Martin might have been at the RAND Corporation at that time.

05-00:34:30
Burnett: And so, this is a bit of an awakening for you. Did it occur to you all at once, or was it a gradual recognition that these tools could have applications to the kinds of things that you're interested in? Because it sounds analogous to some of the questions and results that you were getting in your research before.

05-00:34:58
Telser: Yes, it was. Well actually, I should have mentioned that the article that Scarf wrote that explained core theory was published, I believe, in 1963 in *Econometrica*. So that was before I went to Cowles, and in fact, it was because of that article, and because Scarf was there, that was one of the major reasons for me to want to go to Cowles and to have direct contact and talk with him and so on. Well, I think it would be relevant to talk about this. After the death of [John] von Neumann, which was 1957, I believe, the leading game theorist in my opinion in the US was Lloyd Shapley, and Lloyd Shapley was at the RAND Corporation, and actually spent most of his career at the RAND Corporation.

During World War II, Lloyd Shapley was—I'm not sure; I think it was called OSS [Office of Strategic Services], but he was actually in the Army. He was stationed in Burma, and he knew Russian. This is going to be important, I think, in a couple of minutes. At that time, Russia was not in a war, but we were sending support to the Chinese over Burma to China. And so, the transports, the airplanes, would be strongly affected by weather conditions in Siberia, but the Russians would not give us the weather information because they're neutral, and their weather information was in code; it was encrypted. Lloyd Shapley broke the code, and it's sort of funny about it, and he got a medal for it, for breaking the code. I think he was actually a non-com [non-commissioned officer]. He wasn't an officer, but he broke the code. He got a medal, and in the description for why he got the medal, "meritorious services" or something like that, and nobody really knew why Shapley got the medal until years later.

The point is, Lloyd knew Russian, and there were people in Russia working on game theory for their own reasons, let's say, Cold War reasons. And there was one person in particular, her first name is Olga, O. N. [Nikolaevna] Bondareva, and what she did was extremely clever, and that is, she was able to formulate the von Neumann model of a cooperative game in the form of a linear programming problem. After you saw what she had done, you would say, "My God, it's obvious," but nobody had actually thought of it before she did it. And what she did was really very clever. So, I found out about Bondareva's work through an article that Lloyd Shapley published. Her article appeared in Russian in a journal called [*Problemy*] *Kybernetiki*. The US did publish or translate the important articles from *Doklady Nauk*, which is the standard Soviet science journal, and *Kybernetiki*. Those were translated in English and then published and made available to US OR [operations research] economists, mathematicians and so on.

So, I don't know when Lloyd actually read about her work. It was fairly early; it was in the early sixties, but then he reported it, and then, Scarf used it in developing core theory. There's one other point that's important here, and that is, Martin Shubik is the one who wrote an article pointing out the direct relevance of game theory to studying general equilibrium in the economy. So he was the first one to relate what [Francis Ysidro] Edgeworth had done in the 1880s to what economists were interested in, in general equilibrium in the 1950s. And then, Scarf used the Shubik results and the Bondareva results to make game theory tractable, useful.

It would not have been possible to have done my work without using linear programming. Without linear programming, you have very difficult mathematical relations, and it's very hard to say anything concrete about it. But, what Scarf did was to give the most powerful theorems, or the most general theorems, to prove the existence of a general equilibrium using core theory. And then there was another more technical paper that Scarf wrote together with Debreu. So there was a joint paper by Scarf and Debreu. The first paper that Scarf wrote was actually in the proceedings of a conference at Princeton [University]; and the published paper was, I think it was in the *International Economic Review*, and that was a joint paper with Debreu. So all of this was going on, and then, Aumann was important for me at Cowles, because he explained to me what [John Forbes] Nash [Jr.] was doing in game theory, and that, however, what Nash did was almost entirely on non-cooperative games, and I can't think of anything that Nash ever did in cooperative games.

05-00:43:05
Burnett: Yeah. I don't know, that might have been his particular orientation to the world.

05-00:43:12
Telser: I think we should leave it at that and not get into psychiatry.

05-00:43:20
Burnett: No, no, of course. So, there's so many questions at this stage, but I'm nervous about making you digress in any direction.

05-00:43:32
Telser: No, I have a tendency to do that. [laughter]

05-00:43:36
Burnett: Because I do want to get the full story of your exposure to this intellectual thread that these figures are putting together, and that you then press into service in your research. And just for the non-expert, as I understand it, some of the efforts at Cowles Commission at Chicago, under Tjalling Koopmans, were a kind of search for general equilibrium, but using game theory, or not?

05-00:44:31
Telser: No, at the Cowles Commission itself, I wouldn't say that there was much direct use of game theory. The only time—and it wasn't really game theory—the only direct use of somewhat related material is one that was called "The Assignment Problem." This was an article that was joint of Koopmans and Martin Beckmann, and the problem is, you have a bunch of jobs and you have a bunch of people, and if person one is assigned to job A, the results are such and such. So you have a table that describes what would happen if you assign people to jobs. Now if you have a big table, for instance, ten jobs, and ten people, and you just do this by trial and error, it's going to take you a long time to figure out what is the best assignment. And, von Neumann—and here he comes into the picture—von Neumann figured out that there was a way of solving this problem by using a market mechanism. And that is actually one of the first direct applications of the theory of the core to solving an economic problem.

The method that von Neumann proposed for solving this problem was then elaborated and used by Koopmans and Beckmann, and this work started at the Cowles Commission. And the lecture that they used for their work, I think that von Neumann actually published it earlier, so it was in the early fifties, 1952 or three. So this very direct example of game theory, from von Neumann through the Cowles Commission to Beckmann and Koopmans, resulted in a paper that I believe was published in *Econometrica* that solved a very important, practical problem in economics, and in fact, it was then used by others. In fact, Shapley and somebody actually wrote a program that medical schools use to assign applicants for residencies to teaching hospitals.

05-00:47:44
Burnett: Matching.

05-00:47:45
Telser: If you finished medical school and you're a resident, and you have a specialty and you apply to different places, there was a program that puts all of this stuff together and figures out the best assignment of the medical school students to teaching hospitals for their residences. And I think it was Shapley

and Alvin [Elliot] Roth, they worked this out, and in fact, Alvin Roth, I believe, got a Nobel [Memorial Prize in Economic Sciences] for this stuff. So there's this direct line over a period of maybe twenty-five or thirty years, from the first solution proposed by von Neumann, through Cowles, Beckman, Koopmans, and then Shapley, and Roth.

05-00:48:53
Burnett: Right. But it's a trickle. The economics profession as a whole is not really awake to game theoretic or cybernetic calculations, at least with respect to the core, but perhaps more generally, in terms of game theory, it's not dominating the profession in the late 1950s, early sixties.

05-00:49:24
Telser: Absolutely. I imagine it's only because it is an important example of what you were asking about, game theory through Cowles and so on. But the more direct effect of this on economics is through general equilibrium, and that is from, Shubik, Bondareva, Shapley, Scarf, Scarf and Debreu. And so then, you have an important example where game theory gives the best results to date of the existence of a general equilibrium in the economy.

05-00:50:32
Burnett: So I wanted to ask about the place of the Arrow–Debreu theorem in all of this.

05-00:50:47
Telser: Well, the Arrow–Debreu theorem, it's sort of interesting. It's a model of an economy with competitive markets, but what's interesting about it is that it uses non-cooperative game theory to do this. And although you might say I'm sort of prejudiced—I'm not sort of, I am prejudiced—in order to talk about this in the context of the economy using non-cooperative games, I think you have to sort of distort a little bit. For example, in order for their stuff to work, you have to prevent people from forming coalitions. That is the way their model works, and in general, in non-cooperative models, you assume that there's a group of people that are in, let us say, coalitions. Usually the way you think of it is, they're individuals acting on their own, and they're not talking with each other or doing anything together. So then the problem is, given this background, can you get a result that would lead to what we consider a general equilibrium?

Whereas in core theory, it's entirely different. In core theory, you are explicitly modeling bargaining. In the Arrow–Debreu model, there's no bargaining. It's kind of a formal mathematical way of looking at it. This is maybe a derogatory way of describing it: it's sort of like curve fitting. You don't know anything about what's going on behind the curves, but you have found a curve that fits the data. So because the curve fits the data, you're happy; you're done. Whereas in core theory, you're not happy that way. You want to know more about the process that leads to the result, the bargaining, the interaction that leads to the result.

05-00:53:21
Burnett: It does seem to speak to your approach that you like to marry the highly formalist, mathematical approach with empirical research and proceed in an iterative fashion, and that is part of core theory, in that it unfolds temporally, right? You start with initial condition, and then you move through time, and it allows dynamic reactions to be represented and to be manifested, and you can then compare that to how this works in the real world.

05-00:54:06
Telser: And what I would say is that the way I'm doing it is really much more in the Chicago tradition. In the Chicago tradition, I would say, yeah, let's say typically or normally, we don't just have formalists. We don't have people that all they do is, they sit there and they write down equations. We like to have people that want their stuff to be relevant to the real world, to actual problems. What leads to the theory is the desire to explain what's happening in the real world, and also from the theory, to understand better what is happening in the real world. I did know Debreu, not very well but reasonably well, because he was at Cowles, and in fact, we used to drive home together. So I have a pretty good idea of Gerard, and I don't think that he has an interest in this sort of stuff. He's primarily a mathematician and a theorist, and with [Kenneth Joseph] Arrow, I wouldn't say it's true to the same degree, but to a large degree.

Neither one of them ever did any kind of empirical work that I am aware of. Their primary interests were theoretical, so that, if you give them a problem, they're going to try to find a formal solution. Arrow and Debreu would not have any interest in discovering that what they are talking about has anything to do with trading on the Chicago Board of Trade. Whereas for me, the models that were used to describe markets, especially, I would say [Eugen] Böhm-Bawerk, the Austrian, and earlier, Carl Menger, those models, in what I have been doing, and others, those models really do directly apply to what you can see taking place on the floor of the Chicago Board of Trade. We want to see an actual, real, working situation where the model is telling you something about what is going on, whereas, I don't think that there's any such interest in anything that Arrow and Debreu have done.

05-00:57:13
Burnett: I think, in the history of economics, there's a big book called *Machine Dreams: Economics Becomes a Cyborg Science*, in which historian Philip Mirowski—

05-00:57:24
Telser: It's a recent book, isn't it?

05-00:57:26
Burnett: Two thousand four.

05-00:57:29
Telser: Yeah, well that'd probably be pretty recent. [laughter]

05-00:57:34

Burnett: So Mirowski looks at how game theory filters into the economics profession, and there are three major sites for its uptake, and he talks about Cowles at Chicago as one of these sites. And in discussing Arrow–Debreu, it's framed as part of this Cold War rationality: these disembodied, atomistic, generic individuals who act according to a certain kind of highly prescribed and constrained rationality, and Mirowski links it to this larger context in which the individual has a kind of moral valence. That is also coupled with this story about the politics of economics at this time, in the Cold War context: the effort to develop a strongly capitalist economics that is in contrast to any kind of socialist framework, which is not to say that core theory [laughs] is socialist, but that it admits of any kind of cooperation or collusion amongst actors, whether they're corporations or what have you, or traders.

So that's the way [Chicago's role in the incursion of cybernetics into the economics profession] has been framed historically, and what you're telling is a very different story, it seems to me, and an important one if we're to be agnostic about what is happening at Chicago. There are many different kinds of Chicago economics, in the business school, in the economics department. So you're drawing a line that actually not only goes back to von Neumann, but stretches even further back. Can you talk about this long history that goes back to Edgeworth? How is that related to the research that you do, or that you were doing at this time in the 1960s?

05-01:00:04

Telser: Well, the interest in what actually takes place in a market, in an attempt to understand what's happening in America—not in terms of drawing a supply and a demand curve, but in terms of explaining how people act and trade in a market—starts, I would say, with the elder Carl Menger. I said Böhm Bawerk, but actually, it was the older Carl Menger. And it's interesting that when he was teaching these courses, his students, the young Austrian men who were his students, I assume, were grabbed by his describing a market for horses, so that there were horse traders, and how does the price of horses get determined. And you may end up from your analysis with a supply curve and a demand curve, but that only comes after you have described what is happening that leads to it.

And in fact, it's illustrated by the model that von Neumann was talking about in the 1920s in Vienna, that buyer has an upper bound on the price he's willing to pay, a seller has a lower bound on the price he's willing to accept, then you put them together and you try to figure out what will happen as a result of trading among these people. And even earlier, in the eighteenth century, the London gold market, which I believe used to meet once in the morning and once in the afternoon, and the traders were the leading banks, and they had orders from merchants and so on—and so they would actually work these things out in their trading in a way that is very closely reflected by

the models that were constructed later by Menger, Edgeworth, and Böhm Bawerk.

Now one of the interesting things that Edgeworth did was to show that, as the number of traders increased in a market, given assumptions about preferences and all that, there would be a tendency for the indeterminacy of the resulting prices to shrink, and then a limit; there would emerge a unique market-clearing price in transactions. So that, very few economists actually read *Mathematical Psychics* [*An Essay on the Application of Mathematics to the Moral Sciences*] of Edgeworth. It's not an easy book. Very few read it, but they knew about it because most textbooks describe the Edgeworth model without going into detail. So it becomes a big victory of game theory when Shubik later shows that this model of Edgeworth can be demonstrated in terms of what we call core theory.

And then, in the work that's done by Scarf and Debreu. It's interesting. Here is Debreu. He's working with Arrow, and he's working with Scarf. And the result of the work with Scarf is a much more general model and more insightful model of what happens in a market, than the work with Arrow, and Scarf is a professional mathematician. He has a PhD [Doctor of Philosophy] in math from Princeton, but I think it's important to note something: He is a Princeton PhD when von Neumann is at Princeton. Whereas, Arrow, I don't think he has a formal PhD, but anyway, his context, it was [Harold] Hotelling that led him to study voting, and it's also interesting, by the way, that it came as a surprise to Arrow to be told that, in around 1790 or the late 1780s, the Marquis de Condorcet had already derived this result, the paradox of voting. The only thing that Arrow said is, it's so obvious, he's surprised that nobody ever had found it before. Well as a matter of fact, somebody had found it before, and by the way, Condorcet was murdered in prison by his one of the henchmen of his rivals, [Maximilien] Robespierre. I guess we should mention that today, it's Bastille Day. [laughter]

So there is this interesting history that, starting in the middle nineteenth century, there was a period of about seventy years, but most economists, my guess is that most economists today, they tend to think of this in very abstract—I mean, core theory, it's very abstract, and unrelated to sort of a practical economics, even though there have been attempts made to make it more accessible. In the *Journal of Economic Perspectives*, for example, there's an article by Scarf, and there's one by me. Now Scarf's article is more technical than my article, but they're both designed to try and bring to a very broad audience of economics the applications of core theory to the standard economic problems.

05-01:07:55

Burnett: Well let's walk through those two things: One, the nature of the core. We can talk about the theory of the core, but I think just to lay it out for the audience, the nature of the core, what its advantages are over other theories, and some

examples. And at the same time, talk about your great publication, *Competition, Collusion and Game Theory*, which comes out in 1972. It's a compendium of the kinds of research that you've been doing, and it's very interesting the way you present the chapters. So, I'm wondering if you could tell us, first, what basically is the core, and then, how you then proceeded to lay this out for the economics profession.

05-01:08:53

Telser: Well, I'm going to start with a sort of a funny little story. I often would walk home either with Milton Friedman or [Theodore William] Ted Schultz. So once, I was walking home with Milton Friedman, and he was interested in game theory, and he asked me to tell him what is the core, and what I did, and I'm embarrassed, but I have to tell you, I said, "Well, the core is a set of undominated imputations," which is the technically correct definition, but unless you know what all of this stuff means, it's useless. The basic idea of the core, if you put it in the context of a market, is that you have traders who are getting together and trying to work out deals among themselves. In fact, I think this is pretty much the way Edgeworth describes it. There are traders getting together trying to work out deals, and they reach a tentative agreement, and they say, "Well, keep me in mind." And then they look around, and they look for other possibilities, and they're always trying to find better terms.

So the buyers are always trying to find lower prices, and the sellers are always trying to find higher prices, and, you describe this not in literary terms, as people roaming around and doing this, but what you do is, you write down a bunch of inequalities, and each inequality represents this little group of people—it's called the coalition—and you say, "They are looking for final terms that will be at least as good as what they could get if they just stuck among themselves and reached a deal." So you look at all of these inequalities as a mathematical problem. And that represents the scurrying around of all of these people looking for better deals. The point is that you are actually representing, in formal, mathematical relations, what is being talked about in this discussion of how a market works. You are converting the literary discussion into a set of mathematical relations, and then, what you're doing is trying to find a solution to these mathematical relations.

And so, for example, Bondareva described an algorithm that would work, and would find a solution. But the big achievement in the first place was to understand that you have, literally, a formal representation of every one of these things. At that point, you can then reduce it to a well-founded mathematical problem, and either it has a solution or it doesn't. If it has a solution, what it means is that there is no result that would give any coalition a better deal than they could get from this particular result. So that, what you are actually demonstrating is, the effect of freedom of contracting, that under certain conditions it will result in the best allocation of the goods among the traders. When it works, it does represent, in a sense, the invisible hand theorem of Adam Smith. You have written down a set of mathematical

relations that represents what economists have written about verbally, and that now, we have the technique and the tools to work it out. And we also have, in fact, the machinery that, if you want to test this stuff empirically by collecting data, real data from the market, you can actually test it.

So we are now at a point that was not feasible, thanks to the computer and all of that, that was not feasible before. I mean, even on some of the work that I am doing using a mathematical program, a tool, Mathematica, I can get the computer to solve problems, enormous problems, so fast it's unbelievable, and that wasn't possible for me sixty or seventy years ago. It wasn't even possible for me ten years ago. What we now have in a modern computer is the equivalent of a super computer of the 1970s, for about a thousand bucks. And for instance—again, a slight digression—I was on the Computer Board for the University of Chicago, and so there would be a long discussion: we want to add memory, a megabyte, it would cost a million dollars. How much does a megabyte cost now?

05-01:15:43
Burnett: Virtually free. [laughs]

05-01:15:44
Telser: Nothing, yeah.

05-01:15:46
Burnett: They're given away as swag at conferences. One of the things that I'm struck by is the echoes I'm hearing when you're describing this, because I hear echoes of the minimax theorem, right? I'm thinking of von Neumann and the story of [Jacob] Marschak in Berlin in 1926, von Neumann circling the table saying, "You can't speak of equations; it's inequalities. The buyer will pay at most this and this seller will accept at least this." So, there's that. There's Pareto optimality, in that people want to improve their position without making anyone else worse off, and so there's elements of other economic theories in here.

And what's striking about the book, *Competition, Collusion and Game Theory*, is that you lay this out in the first couple of chapters, to describe what kind of questions could be answered. When is there a competitive equilibrium? When can you achieve it? Under what conditions can you achieve Pareto optimality? Can you have equilibrium if there are coalitions of traders? And from that, you elaborate on the concept of a balanced game. And now, you described what happens when there is a core. What leads to an empty core, and what is an empty core? Can you talk a little bit about that?

05-01:17:37
Telser: Well, in formal terms, an empty core means that the inequalities do not have a solution formally. So now you can say, "Well, what is it that causes this?" Now there's another way I have been using recently to describe a core. A core is a voluntary consensus, so that, if you cannot get a voluntary consensus, that

means that you have an empty core. So then the question is, what do you do in a market? What is interesting is that, in a market, you do get a voluntary consensus, in the following sense: that there are people who are trading, and there are people who are not trading. The people who are not trading are not trading because they are better off with what they have than with the deal they could get in the group that is trading. And the people who are trading are trading because they are better off with the deal in this group than they would get elsewhere.

So in effect, what happens is that if you don't want to trade, you're out of the trading group. So that in the trading group, you have a voluntary consensus, in a sense, that as a result of the trading, everybody in this group is better off than they would be if they weren't trading. And in the other group, where they're not trading, they are better off by not trading than by trading. So in this sense, when you have a non-empty core, you have a voluntary consensus. So when is it that you don't have a voluntary consensus? Well, it's when it's not possible to find an arrangement that would not be objected to by anybody. A voluntary consensus means, you are accepting this because it's the best you can get, voluntarily. Nobody's forcing you to accept this, but you are accepting it because, taking into account all the possibilities offered by freedom of trading among the participants, this is the best that anybody could get. And, there are circumstances when, for whatever reason, you are not willing to have anybody outside the trading group.

In effect, there is sort of an element of coercion involved when you have an empty core. Or put it this way: if you have an empty core, in order to restore what you might call a non-empty core, it would require an element of coercion to get rid of the people who are preventing the agreement. An empty core means there is no voluntary consensus. So the only way you can get a consensus is by imposing some kinds of restrictions. And so this raises the question, under what conditions would it be in the interest of society to do this? The best way I think I could put it in general terms is this way—that, and mathematical terms: There's a global maximum, and there are local maxima. It may be that, if you allow all kinds of coalitions to form that there would be a local maximum, so that they could not be paid off without screwing up the global maximum, so that there's a conflict between the local optimum and the global optimum that somehow cannot be resolved.

The simplest example that I can think of that explains this, and that I've used, is *The Treasure of the Sierra Madre*. In *The Treasure of Sierra Madre*, the way I put it is that there's a big rock, and under this big rock, there's a very valuable treasure. It takes at least two people to move the rock, but there are three people present, so that they have to form a two-person team. Well if they form a two-person team, which two-person team will they form, which means that somebody is going to be left out. Now, one two-person team can form, and they decide on how to divide the gains from the rock. The simplest way to think about it is, let's say, equal sharing. And then the one who was left out

could say to one of them, "Look, if you join me, I'll give you more than half, and I'll just take two-fifths. So you'll get three-fifths, and I'll get two-fifths." So he says okay. And this one guy, he's going to do better with this other; the other guy who was left out would get nothing. But then the one who was left out comes back and he says, "No, no, no. I'll give you seven-tenths and I'll take three-tenths, and you just kick out this other guy," and so on.

And you see that, when you work through what this is doing, you're saying that we have three inequalities, and each inequality involves two of the participants, but the total that is available for the three of them sets an upper bound on what can be paid. And when you go through this formal mathematics, you will see it is impossible to find three, non-negative numbers that will satisfy these four inequalities, the three involving the pairs, and what we call the feasibility constraint involving all three of them. And [B.] Traven's book is, in effect, a literary description of what happens when you have an empty core. And of course, we know what happens. [laughter] They kill each other and one guy is left. One guy is left.

One solution that I have written about in this kind of a situation: the government intervenes. It says, "Well, moving the rock is very dangerous, and it really requires the attention of an expert. So, any group that wants to move the rock must have a certified professional rock mover, and if you don't have a certified professional rock mover, you will not be allowed to move the rock." So you can work through the model when one of them is a certified professional rock mover, and you will find a solution. And the solution is that the certified professional rock mover ends up with all the gold, and the other two get nothing! Now, that's very bad. The problem is, we don't have enough certified professional rock movers. Let's say we have two. That isn't going to help, because if you have two certified professional rock movers, you will end up with no core, empty core. You can only have one.

This is a very simple version of the kind of a problem that you try to handle with core theory. It's something of a puzzle to me why it isn't taught in principles courses. One reason, I think, is that there are a lot of students who would get upset by this result. I'm not really sure. We're sort of getting involved in psychology here, but if you tell somebody that there's no obvious solution to this problem and they feel somehow that there ought to be. For instance, suppose the government passed a rule and said that if you have three people, they have to share equally; or if you have four people, they have to share equally, et cetera. Well then, in a sense, there won't be any conflict until some of them get together and say, "No, this is a stupid regulation. We'll deregulate, and well, everything is going to work out."

05-01:28:53

Burnett: What you talk about in the first couple of chapters is that core theory can show when increasing returns lead to an empty core, and that, therefore, you would need a natural monopoly—or that can describe the conditions under

which you have a natural monopoly. Is that also what you're describing here, in that—

05-01:29:17

Telser: Well, you see, as a practical example of what you might call a natural monopoly, we can see it, in terms of a computer language. When we started out—there were many operating systems, and different computers and so on, and then it became pretty obvious that you have to have some way of allowing all the computers to communicate cheaply with each other, so that having a common operating system seemed advantageous. And a common operating system, for a long time, was the one that was created by Microsoft, and to this day, Microsoft, I suspect, is the leading operating system.

So here is a situation where, first of all, there's really no variable cost involved. You have a bunch of people. They get together and they make an operating system, and it has a certain number of features. And of course, the more features and the more complicated it is, it may be the more expensive is the operating system. On the other hand, it isn't going to cost more to have two million people using it than to have one million people, or ten million people than one million. So, how do we handle that type of a situation in practice, and in economic theory? The point is, it isn't just an esoteric possibility, and say, "Aha! natural monopoly," and then you can have theological arguments about: Does it exist or doesn't it exist?

It's like, if I may say so—I shouldn't probably—about the Trinity. Should we be Unitarians? Do we accept the Trinity? We already know there were lots of people that were fried because they were Unitarians so, this is sometimes a touchy issue. In the case of operating systems, as far as I know so far, nobody has been executed because they refused to use [Microsoft] Windows. [laughter] But you could resolve an empty core problem if you said, "Either you use Windows or you go to the Gulag." And the point is, now do we want to say this is an externality? Well, the group, presumably, is better off if they have a common operating system. So then the question is, how do you do it, and how do you arrange it? And that means you really have to sit down and think about, how do you actually solve the problem?

We use a common language. You and I are speaking pretty much in a common language, and it took a long time to get to this point. I think I may have mentioned this maybe at dinner or something: Sylvia and I—well, and the kids, actually—we spent about a year in Belgium. And in Belgium, there are the Walloons and the Flems [Flemish]. The Walloons speak French, and the Flems speak Dutch, and in order to deal with this problem, at Jacques [H.] Drèze's institute, the official language is English. It's neither Flemish nor French. Now we were there in '69, '70, and since then—it's called, by the way, the Core, but that's for other reasons; it's got nothing to do with the [theory of the] core. But since then, they've run into a lot of trouble, and that solution has not survived. And here they are. These people have been living in

the same country for thousands of years and they haven't figured out yet how to get along.

05-01:34:20
Burnett: To put it this way, a non-empty core or an empty core situation evolves through bargaining, developing coalitions, and the empty core is the failure of an optimal solution. And so, it's either no trading at all, or no activity at all, or if there's an activity, it must be imposed by some powerful group.

05-01:34:58
Telser: That's a formally accurate description, but I think that what happens in the real world is that, if people are fairly reasonable, they work out arrangements and rules. And in the case of the market, we know that rules have emerged and have worked out so that what we now see in the market is nothing like the way it was six or seven hundred years ago, or in futures markets. I guess the original markets started in Amsterdam around the 1600s, and they worked out rules and so on and that takes a long time. And if you have reasonable people with the right kind of attitude towards each other with the morals and the ethics, the sort of thing that [Frank Hyneman] Knight emphasizes, it isn't just a question of straight economics. It's a question of understanding how human beings have to get along and work with each other in order to achieve certain ends. And when you have an empty core, it means that, in this particular set of situations, there is a problem.

So this problem has to somehow be worked out. And it is hard to anticipate, just sitting in an ivory tower or whatever, what the rules should be, but it certainly is a mistake to say, "No rules." There has to be rules, but the rules have to evolve and develop on the basis of some kind of basic, ethical principles. It's taken a long time. This is what underlies all of this stuff, and I would say that I recognize this now more, better than I did sixty or seventy years ago.

05-01:37:23
Burnett: The importance of culture, the importance of society, that there is economic activity that can be broken down in this atomistic fashion or in terms of coalitions and bargaining, but in the end, that doesn't occur in a vacuum, or in an atomistic model. It occurs in society where there are rules and laws. Is that also what's happening at Chicago in the fifties and sixties, because there's law and economics, and you've published in *The Journal of Law and Economics*, or is that a different thing? Looking at, reexamining—because what I see is, in your work and in others, you're reexamining assumptions behind laws, assumptions behind regulations, and testing them, and examining whether it's true in all cases or in some cases, and trying to get a more nuanced portrait of the spirit of the law behind the letter of the law.

05-01:38:25
Telser: When things were going well, yes, that's what was happening, that we did, with the right people present, with the right attitude and personal

characteristics and so on, these things developed. There were certain people who were amenable to discussion. For example, Ronald [Harry] Coase is somebody who is an easy person to talk to. You don't have problems. There's a Scottish economist [Roger Sandilands] who worked with Lauchlin [Bernard] Currie, and got to know Lauchlin. He's in Edinburgh, and we have corresponded often.

Anyway, Lauchlin Currie started, after he left Harvard [University], he was at the Federal Reserve Bank. And then, he came to the attention of FDR [Franklin Delano Roosevelt], and he became the first economic assistant to the president of the United States. I don't know if they have a formal position like this, but Lauchlin Currie occupied this position. And by the way, FDR was a very charming man, one of the best you can imagine. And he said to Currie, "Well, the main reason I chose you is that, whenever there was a discussion, you never lost your temper." Of course, Currie is also a very smart guy and all that, but I think that was—and whose name I, offhand, can't think of right now. I have my list here but I forgot; I didn't put him on my list. By the way, I believe he wrote a biography of Lauchlin Currie.

There were two people, on the US side, very important in negotiating the IMF [International Monetary Fund]: Harry Dexter White and Lauchlin Currie. Lauchlin Currie, I'm not sure if he'd be considered a Canadian. He's from Nova Scotia, originally. But he moved to the US, and Harry Dexter White was an American. In fact, he was Aaron Director's boss at the Treasury.

05-01:41:37
Burnett: Wasn't Harry Dexter White discovered to have been a Communist?

05-01:41:42
Telser: Both Currie and White were accused by Whittaker Chambers, and Elizabeth [Terrill] Bentley, I believe. They were both accused of being Communists, and Aaron [Director], who knew White very well, said, "The only thing he would say about Whittaker Chambers's testimony is that it is clear that he did know Harry Dexter White, because he said things about White that only somebody who knew him would have said." But whether the rest of it is true, we don't—

05-01:42:30
Burnett: Yeah, and I think Elizabeth Bentley's testimony has been called into question. But I think, in any case, Lauchlin Currie went down to [the Republic of] Colombia, is that right? The country, not the—

05-01:42:39
Telser: I think so, yeah. He left. Yeah, he went to Colombia, and I think he became a rancher.

05-01:42:48
Burnett: And he was actually called in, became an advisor to the government and was given—yeah, he was a lauded contributor to that country. And so that, you're

talking about the importance of civility in scientific work, that it's important to be able to have freedom, I think, a freedom to range over possible options without fear of undue hostility or humiliation.

05-01:43:26
Telser: Yeah, you have to be able to discuss things without anybody blowing their top or losing their temper. It has to be reasonable discussion.

05-01:43:38
Burnett: I don't want you to name any names, but were there folks at Chicago or elsewhere, at Cowles, that you felt did not have the right attitude in collaborating with people, or having conversations with people?

05-01:43:58
Telser: Uh—yes.

05-01:44:00
Burnett: Okay. [laughter] And you can elaborate on that as much as you like or as little as you like.

05-01:44:06
Telser: Well, let me put it to you this way. For some reason that I do not understand, Frank Knight would somehow like to confide in me. He would tell me what he thought about this one or that one, and so on. And I decided, I am never going to repeat this to anybody.

05-01:44:27
Burnett: But there are reasons why you are talking about civility and being able to get along with people while you're discussing difficult economic principles. You've often talked about, in your collaborations with people, that they were good to form coalitions with, let's put it that way, and you had fruitful collaborations and you liked to work that way.

05-01:45:01
Telser: Well, with some people, it was. One person who is exceptionally good at this is Zvi Griliches, and he left Chicago. We were very close friends. He was probably one of my closest friends. And he left Chicago in—I guess it was when we went to Belgium. I think he left in '69, and he went to Harvard. Eventually, he died of pancreatic cancer. Anyway, so Zvi was very good at this, and at working with people, and he went to Harvard, and he was one of the very few people, one of the very few faculty at Harvard, who was accessible to the students. If anybody wanted to talk about an economics problem, Zvi's door was always open and they could go in. And eventually—now it ended fairly soon—this was recognized; he was made a chairman.

And then Houthakker, who was at Harvard, and a good friend of mine for obvious reasons—so Henk [Hendrik Houthakker] told me that the main thing that Zvi did in the three years he was chairman is to get rid of the administrative assistant who was awful, and it took three years to, I don't

know if you want to say, fire her, or to get her out, because of there's all kinds of restrictions you get in with the NLRB [National Labor Relations Board] and God knows what. He said, "Henk was sort of disappointed. He was hoping that he would accomplish more." But Zvi's main job was getting rid of a very troublesome administrative assistant, the one who was sort of running the department most of the time.

And so, that sort of thing really is important, and we have had experience. If you've been on a faculty and you've gone to faculty meetings, you know that there are some people who are reasonable, mostly are, and there may be one or two who are unreasonable. And so there are occasions. I remember once walking out of a meeting together with [David] D. Gale [Johnson], and he said, "Things really went well together, and," he said, "you know why? It's because so and so wasn't there." But I won't tell you who it was. [laughter]

05-01:48:39

Burnett: There's that Pareto saying about twenty percent of the people doing eighty percent of the work, and maybe another percentage causing eighty percent of the trouble, there could be that as well. So, the stereotype about Chicago economics is that there is this fetish of the coldly rational individual acting in his self interest, the classical, neoclassical framing of the individual. You were interested, in your work practice, in collaboration. You seem to seek out people to work with. You like to publish with them and you are intellectually interested in the problem of collusion, collaboration, cooperation, *and* competition. So you have then a kind of ecosystem of human behavior that ranges from just stark competition all the way to collaboration and cooperation, which mirrors research in the life sciences. Instead of seeing nature as red in tooth and claw, people are looking more at how organisms collaborate and cooperate. Is that how you see things?

05-01:50:08

Telser: Yeah. I try to. I fail. I often fail, and it doesn't work out. In some cases, well, I'm disappointed, but this is what I really try to do.

05-01:50:36

Burnett: Part of the process for you is testing things, and so you want to see how much competition, when does competition cease to be productive, and when is collusion more beneficial to the agents involved?

05-01:50:56

Telser: I would rather say "cooperating" [chuckles] rather than colluding.

05-01:51:00

Burnett: Yeah. So, you didn't want to title it *Competition, Cooperation and Game Theory*? [laughs] So, collusion is a formal term that is used that other people would recognize. But if you had your druthers, you would talk about cooperation?

05-01:51:21
Telser: See, collusion is the technical term that is used as the opposite of—there's a problem of the connotation of the term, and I didn't take that into account, I think, enough.

05-01:51:50
Burnett: Yeah. You have to deal with existing frameworks. You have to deal with the language that people use in order to be understood. And I do want to talk more about this book, because there's a lot more in it, and it summarizes and includes lots of research that you were doing in the 1960s and early seventies.

05-01:52:18
Telser: It also has a lot of typos. [laughter]

05-01:52:23
Burnett: Yeah, a lot of books have that, that's for sure. Well, I want to just finish off today talking about that year at Cowles in terms of what it did for you, and you described these encounters and so on. Did you embark on a research project? Did you take an existing research project? How did the work unfold for that year at Cowles?

05-01:52:58
Telser: Well, I took an existing research project, a big project with Bob Graves, and I would say that one of the main things that happened is, in the process of doing it, I had to learn a lot of mathematics. I do not—and this may reflect what we were talking about—I do not read mathematics just out of interest in mathematics. I read mathematics that I think will be useful, and it turned out that there were certain parts of mathematics I did not know, and that I had to learn in order to get useful results on the problem I was working on with Bob. And I think, in his case, too, that there were parts of mathematics that he had to learn, because there were new problems that came up.

And so, one of the main things that I did is, by being there, without having any teaching responsibilities or going to department meetings and all of the rest of that, I did have more free time so I could do all of this stuff, which is, I think, one of the purposes of having time to do some serious research on what I think is an important problem. And that year enabled me to do that, especially because it involved a kind of abstract mathematics that I didn't think would actually be relevant in economics. It was sort of funny, in fact, yesterday, in talking with Lars [Peter Hansen] and Jim [James Heckman], I mentioned that, "Well, you know, there are some people that work in Banach spaces." And so Lars said, "That's bad unless you're doing it." [laughs]

And as a matter of fact, we do spend a chapter in this book in a Banach space, because it was actually relevant to the problems that we were working on. I can give you an example. When you're trying to find out if a function has—in this case, we're talking about cost. So we wanted to find out if the cost function has a minimum. If you've had any math courses, then you can think

of a curve that looks like this, and it has a minimum [*draws a curve in space and points to the bottom*]. It doesn't seem like a difficult problem. But, if it's a problem taking place over an infinite horizon with a lot of things going on, the problem of finding out whether that thing has a minimum is very difficult.

And it turned out that there was a condition that I thought of. In fact, I think I thought of it when we were at a concert. With mathematics, it's not like what people think, that you're sitting down with pencil and paper and scribbling around. That's not the way it works. Most of the time, it's going on in your head. You may not even be aware of what's going on, but something is happening in your head, and all of a sudden, aha! That's the answer. Later, in our book, we called it "strong convexity." And then it turned out, through one of my friends, Arcadius Kahan—who was an immigrant from Russia and on economic history in the economics department—so, we found out that there was a Russian at Moscow State University whose name is Polyak. And it turned out that he had thought of the same condition and had written articles about it published in *Doklady Nauk*, and we had no idea of this before.

So, I asked. I said to Arcadius, "It would be nice to send a copy of our book to him." And Arcadius said, "Well, you have to be careful. What you can do is, send the book to the chairman of the math department and tell him that you would appreciate it if he would bring this book to the attention of Polyak. If you send it directly to Polyak, you would get him into trouble." So that's what we did. We sent it to the chairman.

05-01:59:16
Burnett: Right, right. Speaking of different kinds of cultures in different departments and different countries.

05-01:59:22
Telser: So this was—let's see, I don't know—probably middle seventies or so.

05-01:59:26
Burnett: Well, perhaps we should pause for now, and we'll continue next time.

05-01:59:34
Telser: Good, very good.

Interview 6: October 16, 2017

06-00:00:14

Burnett: This is Paul Burnett interviewing Doctor Lester Telser for the Economist Life Stories Project. It's October 16, 2017, and this is our sixth session, and we're here in Hyde Park, Chicago. The last time we talked, we were starting to talk about your collaboration with Robert Graves, and you mentioned that one of the theories that you had elaborated on was also being worked on by a mathematician in the Soviet Union named Polyak. So, I'm wondering if you could tell us a little bit more about this discovery of strong convexity, and what it is.

06-00:01:07

Telser: Okay. Well, first of all, there's a problem that we dealt with, looks at the best policy a firm could choose thinking over a very long horizon, in fact, infinitely distant in the future. Now we don't actually work with an infinite horizon explicitly, but you have, in effect, an infinite horizon if you're always thinking in terms of, let us say, the next five years. So every year, your plan is looking ahead five years, and what that means in practice is, you don't give a lot of weight to events in a very remote future, but you're always giving weight to things that are likely to happen in the near future. And I use five years because it's sort of a popular notion, since in Russia, they were always talking about five-year plans. It has nothing literally to do with what we were doing, but the idea is that you're essentially always looking ahead and trying to figure out what you're going to do. Now, what this means is that you have to take into account the fact that whatever you're doing now will have repercussions in the future, so that whatever you have done in the past, if you start thinking systematically about the problem, is always taking into account what is going on into the future, and also, it's taking into account the fact that things happen that you weren't anticipating, so that there's a constant interplay between what you have been doing, keeping an eye on what the likely effects are going to be in the future.

Now this raises problems that are not dealt with ordinarily in the mathematical version of the instruments that we were talking about. In mathematical problems, the machinery that is used is called calculus of variations, and it makes certain assumptions that are congenial to the usual applications in mathematics, which at that time, are primarily to astronomy and physics. But in economics and in business, it's very different, so that you can't just blindly take over the techniques that the mathematicians developed for handling for applications in physics and astronomy to economics, because in economics, you are always explicitly trying to think about how what you're doing now is going to have an effect later on. So this means that we have to devise the problem to take this into account.

Here is a very simple and practical way to think about it: suppose you're interested in the demand for automobiles. An automobile lasts for a long time,

depending on how much it's used and how it's used and so on, so that the life of an automobile may run up to as long as let's say ten or twelve years, and then, in addition, the potential customers, many of them, have had their cars over this period of time, so the stock of cars, the age distribution of the cars, et cetera, will affect the current demand. So if you are trying to figure out what the effect of your current policy is going to have on your business, which is the manufacture and sale of new cars, you will be taking into account, among other things, the existing inventory of cars. In addition, what is important is that when people are buying cars, they do not usually pay cash up front for the whole car, but they think about paying off the car over a period of years.

So in all of these ways, the past enters, the prospects enter, business conditions in the future enter, and so on. So we worked with a demand model that takes into account all of these factors, and then, when you write down the mathematical form of the problem, it is the economic version of the type of problem that comes up in physics and calculus of variations, but the structure of the problem is different. And so this required us to develop new tools to handle it, and some of them are, well, I'm not really sure how original. But now there's another analogy that is actually a little bit closer to what we are doing than the one I was describing, physics and chemistry, and this is the analogy that arose in World War II. It was the problem of shooting down airplanes.

Now, an airplane is following a certain course, and it's trying to evade being shot down. On the other hand, the radar system has observed the path of the airplane, so that data is being fed to the antiaircraft guns, and two people, Norbert Wiener and Andrey Kolmogorov in Russia, worked on this problem. What is the optimal form? How should guns be aimed to take into account the actual path of the airplane so that you will maximize the chance of shooting it down? And the mathematical version of that problem is very similar to the mathematical version of the economic problem, which is to choose the sequence of production in sales that will maximize the present value of the profits. This, however, leads to a certain kind of difference equation, that is, an equation that relates or takes into account not only the past, but also the future, and it raises technical problems of how you fix it up so that you can make it manageable. It's called the factoring problem, which doesn't really tell you very much, but basically, it involves separating the factors from the past into the likely effects they will have on the future. So a good part, especially in the year I spent at Cowles at Yale, we worked on this problem, and primarily, by mail. That is, you know—

06-00:10:07
Burnett: With Robert Graves.

06-00:10:08
Telser: This is, of course, before the age of the Internet and all of that. This is 1964 or '65. It's sort of ancient times, as far as the present digital age is concerned.

And it turns out that one of the things that happens is, although we didn't give it that description at the time, this really leads to a mathematical formulation of what came to be known as rational expectations. The factoring problem that arises in maximizing the present value of the profits requires dealing with what we call the necessary conditions for finding the best solution. It does this by factoring the problem into the stuff that depends on the past, and the stuff that's going to depend on the future. And it seems obvious when you talk about it, that since what you're doing now is going to affect the future, obviously, you have to take this into account. But it actually poses very hard mathematical problems, and in fact, I was looking at it recently. When we were working on it, there were certain versions of this problem that had not been solved. Since I haven't been working in this area now, I don't know whether, or to what extent, some of these problems have been solved.

06-00:11:57

Burnett: Well, one of the things that you have pointed out was that there was this, in neoclassical economics, reliance on thermodynamic models, and you caution against adopting them wholesale in their application to economic problems. How common was that view by that time? Was that a Chicago approach? Was that across the profession? Were people saying, "This neo-classical stuff is too simple. We need to get more sophisticated about how we approach this?"

06-00:12:32

Telser: Well, I would say that the main place where the more sophisticated approach was developed was at Chicago. Samuelson, for example, really, although he tried to take dynamics into account, his approach is much closer to the standard mathematical approach, and it was more widespread in economics. Now, the simplest version of it is that you might say that what people do today is a result of habit, or, they will tend to repeat doing what they've always done without taking much into account of the repercussions or looking ahead. And I think that was pretty common, and there were elaborate models, some of them called cobweb models. That is, especially in agriculture, there was a tendency to say, "Well, of course, they're farmers, and farmers aren't very smart, so the farmers think that the price next year is going to be the same as last year, so they'll just go ahead and do the same thing next year that they did last year," and so forth and so on. This is the sort of thing, by the way, that would send Ted [Theodore W.] Schultz up the wall. He really disliked this kind of an attitude.

So what would happen when you follow this approach is you get cobwebs. You get things sort of circling around, and people are making systematic mistakes and are not really smart enough to correct it, whereas in what we were doing, we were trying to take into explicit recognition how it would be sensible to behave in a situation where you know that what happens now is going to have an effect in the future, and you don't simply blindly believe that what happens in the past will just repeat itself. And in fact, Milton Friedman, for example, said that "to believe that tomorrow's price, or tomorrow's

income is going to be the same as yesterday's," is what he called "naïve expectations," which is, of course, obviously, not a complimentary description. So we weren't doing that. We were trying to take the dynamics into account in as sensible way as we could.

06-00:15:54

Burnett: So I do want to come back to strong convexity, but while we're on this subject of the larger Chicago context, you partnered with Robert Graves on this project, and then you applied for NSF money, and you got this. You got two NSFs to do this work, and then, you used that to buy out your year at the Cowles Commission at Yale. And so, can you talk about the types of conversations that you were having at Chicago? Were you getting something out of the Yale experience that you weren't getting at Chicago? How was this research received among your colleagues at Chicago?

06-00:16:43

Telser: Before we get into that, I didn't actually answer you about strong convexity, so let me answer strong convexity and then I'll try and answer your questions. Now, in order to get conditions for a maximum or a minimum of a function, you have to say something about the shape of the function, and it is more complicated in an infinite-dimensional space. The behavior of the function and the shape of the function is much more difficult to handle in this type of situation, in a finite-dimensional space, which we call a Euclidean space. Life is much easier in an infinite-dimensional space because you're dealing with sequences over a long horizon. You have to worry about the rate at which you are converging, and how the shape is changing. And we were actually stuck at this point, and there was a way of dealing with it, and that had to do with putting a condition on the rate at which the function changes with the passage of time, and this led to strong convexity. And as it turns out, I found, or we found out later, that there was a Russian who had been working on this problem independently that we didn't know about—his name is Polyak—at Moscow State University, and in fact, we didn't find out about his work until after our book was published. And I don't remember if I mentioned this to you, but it's sort of a little—

06-00:19:04

Burnett: Yes, you did.

06-00:19:05

Telser: —a little aside. One of my colleagues at Chicago, Arcadius Kahan was, well, actually from Lithuania, but it was in fact part of the Soviet Union. So what we wanted to do was to recognize the work of Polyak, although we couldn't do it since the book had already been published. I don't think we referred to it in our book, so we wanted to send a copy of the book to him, and Arcadius said, "No, if he gets a book from people at Chicago, you'll get him in trouble. What you have to do is send a copy of the book to the chairman of the math department and say, 'We would greatly appreciate if you would bring to the attention of Blabbity Blabbity,'" and so that's what we did. And the

interesting thing is, we never heard from Polyak, never got a response, so I have no idea whether he actually, in fact, got the book. He was still there when we sent it to him. That was just the end. We made the attempt, but there was no response. It turns out that this concept does have important consequences in pure mathematics, which was of no interest to either Bob or me.

06-00:20:35
Burnett: I won't go into it, because it would be so over my head, but this is Banach space, is this right?

06-00:20:46
Telser: Yeah.

06-00:20:47
Burnett: So, is one of the reasons why this area was not so explored by economists because it requires a great deal of computing power?

06-00:21:03
Telser: I wouldn't say it was computing. To this day, although there are more economists who are equipped to read and handle this stuff than there were back then—most economists, for example, most of the people in the economics department right now, most of them I don't think would be able to read and understand what we were doing. Now, you were asking what I got at Cowles. Well, there were visitors at Cowles who did know about this stuff, Herb Scarf is one of them, and he had a degree in mathematics, I think it was a PhD from Princeton. Bob Aumann was also visiting, and although my conversations with Aumann were mostly on game theory, not on this, I did discuss the dynamic problems with Herb. And so at that time, there were more people at Cowles who were interested in this stuff than there were at Chicago.

Now when the Cowles Commission was at Chicago, there were people there, Leo Hurwicz, notably, and others, Gérard Debreu and others, who are expert in these sorts of topics, Martin Beckman, so you could talk with them about it. But that was not true at Chicago after Cowles left. They left in June of '54, so that there was a gap at Chicago, and I think the only ones who were actually working on this stuff were Bob and me, and I'm not really sure. Now Milton was interested in computers, but I do not recall that he ever showed any particular interest in this stuff, even though since he was working on permanent income and so on, it really did have direct relevance to the work he was doing, but I don't remember him ever asking me or talking with me about it. In fact, as an aside, I actually gave a lecture on this stuff at Columbia in, I think it was in 1965. In other words, the year I was visiting at Cowles, I gave a talk in New York at Columbia, and Milton and George Stigler were in the audience, and they heard me, and after the talk, I had dinner. I think that Milton was visiting Columbia at the time, so I think I had dinner with Milton and George, and they didn't say anything to me about this stuff. So I don't

think he was interested. The interest was mostly from people who were not at Chicago.

06-00:25:03
Burnett: Right, and so you found other networks, other connections at that time. This is a period in which you are still at the Chicago School of Business, and you also, do you spend a full year at RAND? Is that right?

06-00:25:35
Telser: Well, no, I was a consultant at RAND, so I didn't spend long periods of time. I would spend maybe two weeks or so, so these were very short periods. I think I probably started in maybe '62 or three, and some of the people, for example, I met at RAND, there wasn't really any interest in the research that I was doing at Chicago. They were more interested in my trying to help them on some of the problems that they had. They were not so much interested in learning about what I was doing elsewhere. And the basic approach at RAND is to try and inject economic rationality into military decisions, so that you would think in terms of cost and benefit. Now here is an example: At RAND, I wasn't involved in any classified work, although you did have to have a top-secret clearance just to get into the place, unless you were accompanied continuously [laughter], including going to the john. If you didn't have a clearance, you were never left alone for a second. So security was very tight.

But one of the problems that I was involved in is the—and by the way, RAND is for the Air Force [founded by Douglas Aircraft in 1948 to support research for the US Army Air Force], and they were in the process of getting new transport planes, and I think it was the C-5A, so it was a very big airplane, and it could carry a heavy load. What they were concerned with is, how should they arrange the terms of the contract? Should they have a fixed-price contract? In other words, you say, "Well, this is the stuff that we want to have in the plane," and then you get bids from companies, and you give the contract to the lowest bidder, or, the alternative is that "we're really not sure of what we want in the plane, and we may want to add things or subtract things as we go along." So we can't have a fixed-price contract, but if we don't have a fixed-price contract, then how are we supposed to handle this sort of thing? That is, if you want to add a feature to the airplane and it's going to cost something, how does it fit in with what you're already doing? Now these are very complicated problems, and as I recall, what essentially ended up, after thinking about it, is they decided fixed-price contracts wouldn't work. And then, you get what are called cost overruns, grist for the newspapers. These guys, they're interested in selling the paper. They're not really interested in having a sympathetic ear to the problems that you are realistically facing.

So this is the sort of area that I got involved in at RAND. And then there's also a very funny thing about these high-security places: every office has a safe, so whenever you take out a classified document, you have to put it in your safe and you have to keep track of it, and lock it up. So the effect of this

is that people are reluctant to take out classified stuff, and if you want to read something, you go to the library, and you read it in the library, and then you give it back to the librarian and it goes into the safe. Maybe that's really what they wanted in the first place. There wouldn't be things floating around in the building, even though everybody had to account for it and they had a safe.

06-00:31:02
Burnett: So, do you think it discouraged research on the things that were probably the most important? [laughs]

06-00:31:10
Telser: It could be. I don't know.

06-00:31:11
Burnett: What was the work culture like there, as far as you knew, because you didn't spend a lot of time there, but compared to say, an economics department, or, is that quasi-academic? Is it a complete meeting between a military-type organization and a para-academic organization? How would you describe it?

06-00:31:34
Telser: It seems to me that at lunch and stuff like that, we really didn't talk about what we were doing. You're really sort of inhibited from talking about your work.

06-00:31:53
Burnett: Right, and there's a tremendous cost to that.

06-00:31:56
Telser: Yeah, right.

06-00:31:59
Burnett: But it was an interesting experience for you.

06-00:32:01
Telser: Oh yeah.

06-00:32:02
Burnett: When we were talking earlier about the research on how you modeled mathematically the influence of the past on decision-making about the future, and for a lot of your research, you like to ground or root the sophisticated mathematical modeling in observations about how people actually behave, so, did you do research, or were you talking to people who did research about executive decision-making, or the decision-making of firms?

06-00:32:46
Telser: Well, to some extent. Now first of all, one of the interesting things about my experience at the business school is that they made a very conscious effort so that the economists would be in contact with the real world, would talk to executives and would know what's going on in the real world, and I would say that we did learn more about the real world than is the case now with

many economics departments. That is, although I think it may be changing at Chicago, but there was kind of a split. And I forgot to mention this, that the reason I got involved in the first place on this topic with Bob Graves, is because I was interested in what we call intangible capital, so that you are building up a stock of knowledge that will be useful, but, it is not carried on the books of the company. So that when people are trying to judge how successful or unsuccessful the company is, they would sometimes be surprised, because using the conventional measures of assets, it would either show up as a profit or as a loss, which wasn't accurately reflecting what was going on at the company.

So one of the main purposes of the research that Bob and I were doing was to see how you would actually measure the intangible capital by using these models that take into account how things are changing over time, and this became very important. For instance, I remember once, I spent a day—I don't remember the name of the company, but they made candy, sort of Life Saver-type candy. I was talking with them about how the advertising outlays they make would affect future sales and how it actually acts as an asset, so that there were two kinds of expenditures that were typically ignored: promotional outlays, advertising sales and so on, and research and development outlays. It may seem surprising that these were ignored in the conventional measures of assets, but they were, and I met with executives who were interested in this sort of thing, and this isn't really consulting. I remember once, I spent an afternoon at Standard [Oil] of Indiana talking about that, and Bob Graves too, by the way, because of his interest would spend a fair amount of time with big oil companies because they were using linear programming to figure out the best techniques to use to refine crude oil into the products: gasoline, kerosene, diesel fuel, et cetera. So we were exposed. As another example, and this is also on advertising, economists had a tendency to think that advertising is some kind of a blemish on the economy. It's bad, and—

06-00:37:23
Burnett: It's unproductive, in some ways, right?

06-00:37:25
Telser: Yeah, and this is sort of the, let's say the Galbraith school, but actually, there was a Chicago economist very hostile to advertising: Henry Simons. He's one of the saints of the Chicago school, but what he wrote about advertising is simply awful.

06-00:37:47
Burnett: In the *Positive Program for Laissez Faire*, is that right, in the thirties?

06-00:37:49
Telser: Yeah, I think, and in the "good economy," there wouldn't be any advertising. And I remember once, I had a visit by three lawyers from the *Chicago Tribune*, and the problem was that the *Tribune*, like many newspapers, charged lower advertising rates for local advertising than for national

advertising, and this ran counter to the sort of what you might call prevailing view. Who were the national advertisers? These behemoths, General Motors, Ford, Chrysler, evil, big corporations, and yet, despite their monopoly power, they actually paid higher rates for advertising in the newspapers than the local firms. So how could this be? And one of my students, Jim Ferguson, wrote a dissertation about this, and pointed out that the local firms have weekend sales, and so they actually sell more papers on Wednesday, Thursday, and Friday than they sell in the first part of the week. Anyway, these three lawyers came to see me, and they said they were asking the people at the *Trib* why are there these differences in the rates, and nobody could tell them, and one of them said, "Well, why don't you go and talk to Telser?" something like that. So I explained to them that local advertising increases the circulation of the paper, and therefore, as economists would put it, it has a negative marginal cost. A negative marginal cost means it brings money in, which is a sort of a peculiar way to describe it. But anyway, that's what actually goes on, and this was astonishing, because it is, not only lawyers are taught certain things about business in law schools based on, let us say, whatever their ideology is, but it's not necessarily—exhibit any genuine understanding of what really happens in the business world.

There are many examples in my own work of contacts with [industry]. We met once, or we had a lunch once, with executives at Sears Roebuck, and one of them said to us, he said, "You know," he said, "I bet you never thought about the fact that the Sears catalog is smaller than a Montgomery Ward catalog." And we said, "No, we hadn't thought about it," and he said, "I'll tell you why. Which one do you put on top? The smaller one." So it's that sort of thing. The idea that is coming across here is that there's a lot of clever thinking that's going on, and you don't just look at this stuff and say, "Well, it's obviously wrong," and this just shows—and I'm going to make a nasty crack—this just shows the need for behavioral economics. There isn't any real need for behavioral economics except among economists who don't know anything about how the real world works.

06-00:42:05

Burnett: This is something that George Shultz talked about with respect to the Graduate School of Business, that there was an insistence on not only getting out there into the real world, but also interdisciplinarity, so that mathematically oriented economists would be talking to sociologists, and they brought psychologists on staff in the fifties, and that's part of the Allen Wallis restructuring, that he wanted to have a truly multidisciplinary faculty that would have tendrils out to the wider world, but also to the economics department, so that there would be these connections.

In my work on the history of Chicago economics and agriculture, I'm interested in the relationship between knowledge and place. Does the fact that the Chicago Business School and the economics department, does the fact that they are in Chicago make a difference, that there's something about Chicago

itself as the center of certain kinds of commerce, giant corporations, and the history of the university as a private university, that is not necessarily beholden to the interests of the state, by contrast with Ted Schultz's scrape-ups at Iowa State University? Is there something about Chicago itself, and could you talk about that a little bit?

06-00:43:41

Telser: Yeah, I think that's a very interesting thought, and as you were speaking, I was thinking, well why is it? My impression is that, for example, this was not true at Harvard. Now of course, Boston is not a major center of anything, but then, what about at Columbia and New York and NYU, what's going on there? Why didn't it happen? Then we can sort of speculate about it. It may be because Columbia was older, and when it started, I think it was originally called King's College, so when it started in, I don't know, eighteenth century or something, it wasn't similar to Chicago. When the U of C started, Chicago was already a major industrial city. There were the stockyards, International Harvester. International Harvester was a very big business in Chicago, and meatpacking and all of that was also a very big business. This was the leading center for agricultural markets—wheat, corn, soybeans, et cetera—so maybe that had some kind of an effect. I shouldn't even say maybe; it seems that there was this kind of an effect of the environment, and an attitude at Chicago that you don't find elsewhere. Now one of the major founders of the U of C was John D. Rockefeller. And the interesting question is, why at Chicago? There wasn't anything special, as far as I know, linking Rockefeller to Chicago. But he was, I think, the starting point.

But then there's another factor at Chicago that I think is important, it's a little different, and that is, there was always the emphasis on a graduate school and research, and this was very unusual. The only other university in the US with this type of an attitude, as far as I know, was Johns Hopkins. And the early faculty, the first faculty at Chicago, many of them, had been trained in Germany, and so they were familiar with the German model. Now of course, they were brought to Chicago when it was founded, so somebody must've been thinking about this. I don't really know who it was, but somebody must've been thinking about this, that the United States needed a research university, and the U of C was it.

06-00:47:22

Burnett: And Johns Hopkins as well was absolutely modeled after the German university, and that was it, and then those models then creep into other universities and become important there as well.

06-00:47:32

Telser: In the case of Hopkins, you really can't explain it in terms of Baltimore, no. There's something else going on. Now, for instance, at Hopkins, very early, they hired one of the leading mathematicians in the world, [James Joseph] Sylvester, who was originally a barrister in London and then got into mathematics and did very important work in algebra, but why he was hired

there. But then at Chicago, there were very important mathematicians hired right at the outset, and important scientists, and there were major economists hired at Chicago right at the start.

06-00:48:29

Burnett: Well, it seems to me that Chicago is an absolutely important, crucial economic hub, a center not only for manufacturing, but also for distribution, if you look at all the railroads that come out of there, right? So maybe that's the Rockefeller connection, oil, railroads, etc. But, we know that the economics department was training economists with the meatpackers in the twenties, short courses. So there's that organic connection to the economy, in a similar way that Iowa State College was getting mathematicians to work on the sophisticated problems of calculating the agricultural economy, right? So you can see that there's a relationship between the place and the kind of knowledge that's being produced there. We can keep that in mind as we're talking about how the Graduate School of Business and the Chicago economics department evolve.

So you're doing this work, and you move from the Graduate School of Business to the economics department, and we joked about this in an earlier session, about, I asked you why, and you quoted Al Harberger— [laughs] So, can you talk about that transition, and what it was like when you arrived in the economics department, how it was different, some of the people that you knew already, and some of the people that you began new relationships with when you moved there?

06-00:50:27

Telser: Well the, one of my closest friends was Zvi Griliches, and his dissertation was on the origins of hybrid corn, and I was also very close to Al Harberger, and of course, they were both in the economics department. I was in the Money Workshop of Milton Friedman, starting I guess almost when the Money Workshop began, and the work on my dissertation on futures trading, although it's certainly closely related to agriculture, it also has monetary implications. That is, the way I think of the futures market is, it really creates a kind of a special monetary framework for handling a large range of agricultural products. There was Yehuda Grunfeld—was in the economics department, and had done a dissertation on corporate finance, so we were close—Dick Muth, who was working on housing. In the faculty, I would say that the relations were really closer with former graduate students, fellow graduate students, and with some faculty with whom I had close relations.

I did not attend the Agricultural Economics Workshop regularly. The one new workshop that I did attend regularly, which started in the economics department, was joint with the Law School, was the one that Stigler started called the Industrial Organization Workshop. Now George [Stigler] and Aaron Director started that workshop, and in fact, I believe that the first paper at that workshop might've been on the work I was doing on resale price

maintenance, so that could not have been in the fall of ’58, but it could’ve been in the academic year of ’59 or so. I’m pretty sure it was the first session of the Industrial Organization Workshop. And then there were other people like Reuben Kessel, who was interested in monetary economics and industrial organization, so I was pretty close to Reuben Kessel. He died at a very early age, prematurely, and it was a blow. He wasn’t much over forty, if forty, when he died.

Anyway, so there was Reuben Kessel and others, John McGee, who was in the Industrial Organization Workshop, and in fact, it was McGee who introduced me to Director. And so I had interest with people. Oh, and then Hans Thiel was—in fact, Merton Miller and I were primarily responsible for bringing Thiel to Chicago.

06-00:55:25
Burnett: And George Stigler comes to Chicago in ’58, I think?

06-00:55:31
Telser: Yes. The same—

06-00:55:35
Burnett: Same year as you, but you’re in the Graduate School of Business and he’s got a joint appointment between them. How many people had joint appointments, between economics and Chicago, economics and GSB?

06-00:55:48
Telser: I think the only ones I can think of offhand are Stigler and Thiel.

06-00:55:59
Burnett: And you were participating in the workshops pretty much as soon as you landed. As soon as you came back to the University of Chicago, you were attending those workshops, as were other people from the Graduate School of Business. It attracted people from GSB, obviously the economics department, and also the Law School, that early on?

06-00:56:21
Telser: There were some from the Law School. Now, of course, the workshop system started at Chicago when I was a graduate student in the fifties, early fifties, and that was actually one of the major attractions to coming back to Chicago, this active workshop system. Harvard did not have workshops; Berkeley did not have workshops, although I very seriously considered the offer from Berkeley. So Chicago was really unique in this regard, that they had this atmosphere of ongoing research involving faculty and students. At Harvard, for example, if you were a student, it was almost impossible to ever see a faculty member in his office. It was almost unheard of, whereas it was pretty common at Chicago. And as a matter of fact, I remember there was somebody who said something about, one of the big advantages of Chicago was this openness. This was a guy, he was on the faculty at Chicago, and I don't remember, I think he had come from someplace in the east. Anyway, so there

was a certain atmosphere of joint work at Chicago that you really didn't find anyplace else, as far as I knew.

06-00:58:08
Burnett: It's a flat kind of social structure, in which the graduate students are seen as near peers, right, and that you would work together on projects, and it was an opportunity to explore and reexamine common assumptions?

06-00:58:34
Telser: Absolutely.

06-00:58:35
Burnett: Did some of the work in the workshops from the late fifties on, did that inspire you to some degree, in addition, of course, to the work that you encounter with Robert Graves, and the people at Cowles at Yale? There's a paper, "Why Should Manufacturers Want Fair Trade?" in 1960, and I think Aaron Director—did he inspire you to work on that problem? How did that come about?

06-00:59:09
Telser: Well, Aaron had—I'm not really sure about the funding, but he—that here was a new area of research, and that is, to try and give an economic analysis of antitrust, that is, in these cases, you would get very clear evidence of certain business practices, and these business practices seemed to violate standard economic theory. And so, whoever it was that was sponsoring or behind this—I'm not sure about it—made a connection with Aaron that, what he would do is get people to study this question, and see if you could figure out why the business firms were doing it, and some of this work became very famous. John McGee's, in particular, was on predatory pricing by the Standard Oil Company. And the reason it was a problem is, if Standard Oil wanted to acquire a company, why didn't they just buy the company? If you just think about the simple arithmetic, if Standard Oil, let us say, has more than 50 percent of the market, and they start cutting the price, then they're going to lose more than the guy they're trying to drive out of the market.

So Aaron would say, "Well that sort of thing doesn't make sense. Is it really happening?" So what McGee did is he looked at the record to see, is this really a fact, or is this sort of part of the fiction in US history by Ida Tarbell, and there was, in the 1890s and so on, the muckraking. So maybe this is all made-up stories that really didn't happen. In fact, I did a little bit of work myself on the price of refined oil, and there was a guy at Harvard who claimed there was predatory pricing, but the point is that the Standard Oil Company and John D. Rockefeller was one of the first big corporations to hire research chemists to figure out how to refine oil at a lower cost. So the reason their prices were lower is because they could refine oil at a lower cost.

Then there was something else that Standard Oil was doing. They said that they forced the railroads to give them rebates on oil shipments if the railroads

carried oil produced by Rockefeller competitors. Well, this ignores the fact that what Rockefeller did—and this, of course, is before the days of oil pipelines—he said, "If I give you a long-term contract that, every couple of days, a line of tank cars will show up at the oilfield, we're going to fill it up, and it goes on, and so you're going to get a steady stream of business from us, I don't want you to go around picking up a couple of cars from these other guys who are literally freeloaders. I'm not going to subsidize them. If you do it, you're going to have to pay the cost of it, not me."

Well, these are the sort of practical things that would not be thought of by people who have certain prejudices. What you really have to do, or you have to try and do, is to look at these problems, and try to figure out what's going on here. You don't approach it with some kind of—

06-01:04:07
Burnett: Preconceived—

06-01:04:07
Telser: —conceptions about the evil businessman, or, they're either evil, or they're stupid, or both.

06-01:04:16
Burnett: So at Chicago, there's a pattern of reexamination of the moral valence of interpretations. I think we spoke of this in a previous session, with respect to speculators. There are ideas about speculators, that they are profiting at the expense, in an uneconomic way, and they're taking something and exploiting. When you get rid of those kind of moral aspects and you analyze what they're doing, you can come up with other reasons. And so there was a drive for that, I understand, at Chicago, to look at monopoly. I don't want to put words in your mouth, but I imagine this had an impact on you when you're thinking about competition, when you're thinking about concentration ratios and such. So this starts to influence you to some degree, I imagine.

06-01:05:25
Telser: Well, see, the problem, the standard argument in economic theory is that you assume a monopoly. You don't ask yourself, how is it possible for this guy to become a monopolist? Is a monopoly some kind of an exogenous event that we take as a given, or is it an endogenous fact that we try to explain the structure of the economy in terms that would make sense, in terms of net gain? That is, instead of thinking of it only in terms of the sellers, it's better to think of this as the result of the actions of both sellers and buyers. Now, and then there's, of course, the problem that, if you have a few sellers and many buyers, then the sellers are powerful and the buyers are weak, and the sellers can therefore exploit the buyers. But then there's another force that is at work, and that force that's at work is competition among the sellers. So, is it the case that there's always an incentive, a powerful incentive for the sellers to get together and collude in order to screw the buyers? And there are a lot of antitrust cases that come up like this.

Now I was involved in one, and that was a case that was brought by the Federal Trade Commission against the manufacturers of breakfast cereal, Kellogg's, General Mills which made Wheaties, "the breakfast of champions," and I think there were some others, Cheerios. And I remember, Ezra Solomon was one of the expert witnesses for one of the cereal companies, and I don't remember who asked the question, whether it was the defense counsel or the government, why this company wasn't a defendant in the case, and he said, and Ezra said, "Well, it's because they weren't as successful as the ones who were." [laughs] So, but I was an expert witness for Kellogg. And then we got into the question. One of the major issues that came up was, first, were the firms colluding on price? And of course, in that particular case, it's very difficult, because Kellogg's Corn Flakes was a brand, and their competitors tried to have rivals that would capture the consumer but without success. So the argument was, they were colluding on price, and the competitors could've been more successful if they lowered the price and took business away from Kellogg's.

So then, in the course of the case, we ended up in an argument about a settled antitrust case against the cigarette companies. That was against the American Tobacco Company, I believe it was in 1911. During the Teddy Roosevelt era, there were a lot of antitrust cases. And in that situation, there was an issue of collusion in two ways: first, collusion in buying tobacco from the farmers, so if one cigarette firm went into a local market buying tobacco, the other guys would stay out. So there was a problem of what we would call oligopsony, not oligopoly, in the purchase of tobacco. Then there was also the question of the lack of price competition in the prices of cigarettes, and what happened in the 1930s is, ten-cent brands appeared on the scene. The regular price of a pack of cigarettes back then was twenty-five cents, so when the ten-cent brands appeared, they did very well. And the response of the major cigarette firms was some decrease in their price, but not a very large decrease in the price, and eventually, the ten-cent brands disappeared from the scene. So the question was, why did this happen? What did the cigarette companies do, if anything, that resulted in the demise of the ten-cent brands? Why didn't, before this, for example, when there were major brands, Lucky Strike, Chesterfields, Camels, et cetera, why didn't somebody cut the price?

Now I have to say that in that case, when you looked at some of the evidence, for example, telegrams, I remember there was one telegram between A&P, Atlantic and Pacific, and a cigarette company about price cutting. There really was evidence of collusion. So what happened in the course of the cereal case is that when they started to try to compare the situation in Corn Flakes with the situation in cigarettes, the analogy broke down, because you could see that there were things that were going on in cigarettes that were not going on in Corn Flakes. For example, one of the main things that was happening in Corn Flakes was that they would give coupons, so you would get lower prices if you made repeat purchases. That never happened in the case of cigarettes. So then the question is, why did people do this? It turned out that there was

evidence that the people most likely to use the coupons were the heavier buyers of Corn Flakes. God knows why they would be; maybe they had big families. Who knows? Anyway, the heavier buyers would use the coupons, and the ones who didn't buy a lot of Corn Flakes wouldn't. So the case begins to unravel when you start looking at the actual situation and the facts.

06-01:14:34
Burnett: That's a real pattern in your research. When you look at a shibboleth in economics, you dive into the data, and you find that it's a lot more complicated than a simple rule would dictate.

06-01:14:49
Telser: Yeah.

06-01:14:50
Burnett: So just to clarify here, you were summoned or subpoenaed to be part of a court case because of your expertise.

06-01:15:03
Telser: Yeah.

06-01:15:03
Burnett: Okay. And, roughly when was this?

06-01:15:05
Telser: Pardon?

06-01:15:06
Burnett: When were you doing the Kellogg case?

06-01:15:12
Telser: I think it was in the early seventies, '72, '73.

06-01:15:24
Burnett: But all this to say that, at the beginning of the sixties, there is this interest, and so, this is, the paper that you write, "Why Should Manufacturers Want Fair Trade?" is for the *Journal of Law and Economics*, and that's the third volume, so that journal begins in 1958. So this is, I understand, a kind of a Stigler project, and he has the Walgreen Fund as part of his purview, I think, or his responsibility, so I think there's something about that.

06-01:16:01
Telser: Well, this may have had an effect, but my impression is, Aaron [Director] started his project before [George] Stigler came to the university, and he did have funds from a foundation but I don't really know which one.

06-01:16:28
Burnett: It's from a furniture manufacturer— [the Volker Fund]

06-01:16:35

Telser: Now but see, what intrigued Aaron is the question: here is a product. In this case, the actual case involves light bulbs produced by Westinghouse and General Electric. So, they sell the light bulbs to wholesalers, and the wholesalers sell the light bulbs to the retailers. But resale price maintenance means, a floor on the retail price. Now, it doesn't affect the wholesale price, so that from the point of view of GE and Westinghouse, it would seem that they would prefer that the retailers gave away the light bulbs free, but GE and Westinghouse isn't giving away their light bulbs free to the wholesalers, so what would maximize their sales would be the lowest possible price at the retail levels. So why would they want to set a minimum price on the retail level? See, this is a puzzle. This is sort of the typical Aaron puzzle. It looks like the company that is in the dock is doing something that doesn't really make any sense, but why are they doing it? They're not fools. And so my job was to read—oh, and then there was somebody else who had been assigned this problem, and I think he came up with all kinds of complicated price discrimination, and it didn't make any sense.

So Aaron gave me briefs and records of the case, and this was actually when I first got back to Chicago in '58, and he said, "Well, read his stuff, and see if you can figure out, why were they doing this?" And I should mention something. By the way, these are all minor details. As you know, there are nine justices on the Supreme Court. And when a case is argued at the Supreme Court, the lawyers have to present a brief, which is like a book. So each justice has the book, the briefs, and what they usually do is, they deliver them to a law school. Usually, it's the law school from which they had graduated or they were on the faculty or whatever. So there was a justice on the court from the U of C, so the Law School got his briefs and records, so I got the whole thing about the light bulb case from the briefs and records. And I remember sitting, we were in an apartment on Blackstone, and I remember sitting there, and I would read these briefs, and I would dictate. It's the only time I ever used a dictating machine, but you have this voluminous material; you're not going to sit there and write. So I was dictating my notes as I went along, reading the case, and then I would meet periodically with Aaron and we would talk about it.

And this went on, and although I think I must've spent about, well, among other things, I think I spent a couple of years on the case. And the other thing that we had to do, since it was published in a law journal, I had to cite cases, but of course, neither Aaron nor I had any interest in citing cases, so a law school student was hired to go there, find all the cases, give us the citations, and that goes into the article. And I still have a bunch of index cards upstairs with all these citations. [laughter]

06-01:21:26
Telser: And there's one other thing I should mention. I had a mathematical model of this stuff, and Aaron said, "No, no model. If you put the model in, nobody's going to read it." So that was taken out. No model.

06-01:21:43
Burnett: So Aaron was shrewd in that sense about audience; he was thinking about who he wanted to reach.

06-01:21:47
Telser: You want people to read this. Oh, and then yeah, there's another thought's come to me. In that particular issue, I guess it was the 1960 issue, it has Ronald Coase's famous article and mine. Now his is more cited than mine, but mine is, I don't know, it has a couple thousand citations, I think. And Ronald and I were on very good terms, and we have a common complaint, and the complaint was, nobody actually reads his article or my article, or at least if they do read it, they only read the first part, and they never read the last part. Now in Ronald's article, he talks about the effect of transaction costs, so people criticize him, because they say, "Well, he thinks that if there weren't any transaction costs, everything would be great."

That's not the point of the article. [laughs] The point of the article is to study the effects of transaction costs. He obviously knows there are transaction costs, and in my case, I talk about what we call the special service argument. You know, one retailer is explaining to this boob customer how you screw in a light bulb, and "No," I said, "that's got nothing to do with it." By the 1920s, everybody knew about light bulbs. There was no special service; the real explanation of the case had to do with a cartel agreement between GE and Westinghouse. So they only read the first part of the article, and the special service argument, and they never read the last part of the article, which actually explains why GE and Westinghouse were doing it.

06-01:23:49
Burnett: So, that's a way to perhaps segue into, when you moved to the economics department at the University of Chicago, you start a graduate seminar called Theories of Competition, so I'm wondering if you could talk about that with respect to the types of projects that you were exploring with your students, maybe a little bit about the students themselves during those five years. I say five years because you say later that its five years of teaching this graduate course that helped you to formulate the 1972 book on game theory, competition, and collusion. So, if you could talk a little bit about that, and perhaps about the relationship between teaching and research.

06-01:24:55
Telser: Well, let's see. I think I mentioned one project, which was Jim Ferguson on the difference between the local and the national advertising rate, so that was a dissertation. Then there was another one by Kristian Palda. He was a Czech student, and he did a direct study to estimate the advertising capital, I believe

it was for beer, [Lydia Pinkham's vegetable compound] and his dissertation won a Ford Foundation prize. The Ford Foundation would give prizes for the best PhD dissertations in economics, and there were several from Chicago, and I think I was involved in all of them. Then David Meiselman — although, I don't remember if he was actually in the workshop or not — who wrote a dissertation on the term structure of interest rates, and there, the problem was, the standard argument was, that the reason that short-term interest rates are lower than long-term interest rates is because a short-term interest rate isn't as risky as a long-term interest rate.

Well, that's absurd, because the people who make long-term commitments, like insurance companies, so that, in other words, they are making a long-term commitment to pay insurance to people twenty or thirty years into the future, they want to hedge their income. So how do they hedge their income? Well, they have to hedge their income by issuing long-term bonds. So a long-term bond doesn't raise their risk, it lowers their risk. It's part of their hedging strategy. Anyway, so that's another example, and it was J. R. Hicks, I think, who was a big proponent of this: that the reason for the difference in the interest rates is short rates are less risky than long-term rates. I don't think that J. R. Hicks—he's a good example of an economist whose knowledge of the real world is as close to epsilon or zero as you [laughs] can get.

I'm trying to remember some of the others. Now, Nat Wilcox did a dissertation testing some of the propositions of Tversky, and he did experiments with subjects on how they behaved with respect to risky prospects. And one of the things that would come up is that, if the probabilities were either close to one or close to zero, there would be a tendency for the subjects to round to one or to zero, and Tversky made a big deal out of this, and the problem is that usually, when you're doing this stuff, the amount of money that's involved is really trivial. And so you're not going to, unless you're a lightning calculator, you're not going to sit there and start worrying about nickels and dimes. And this is similar to an issue that I worked on much later in "The Ultimatum Game." We can talk about this later, but the behavior is understandable when you take into account the effects of the actual financial incentives, that you will behave differently if you're talking about splitting ten bucks than if you're a baseball player and the amount of money that you're talking about is $500 thousand. That has a very different effect on your decision than two people talking about splitting ten dollars between them.

So anyway, so there was Palda, and then there was another dissertation on interest rates I don't really remember. There was a dissertation by Mary Sullivan on brand loyalty.

06-01:30:45

Burnett: So, to be clear, the Theories of Competition, it was a graduate seminar, or it was a workshop like the Money Workshop? How was it set up?

06-01:30:54

Telser: Well, it was really like the Money Workshop. The people who were in it were working on dissertations. There were some people in the audience, but almost everybody was working on a dissertation, and Mary Sullivan's dissertation was successful, and actually, she was hired as an assistant professor at the business school.

06-01:31:21

Burnett: So, in your monograph, *Competition, Collusion and Game Theory*, you talk about the Theories of Competition Workshop as being a kind of incubator for some of these ideas. And one of the things that's striking about it is this examination of these kind of classical notions of let's say, Cournot, for example, and going back into the history of economic thought, is that a kind of an influence of other teachers that you've had? Is that part of the process of teaching that inspires research, going back to the drawing board and saying, "Now why do people have this assumption?" That reexamination process, can you talk a little bit about that?

06-01:32:23

Telser: Well, it really goes back to the work on monopolistic competition. [Edward Hastings] Chamberlin, for example, has a model in which firms can affect prices, the amounts. If they want to sell more, they lower the price. And so, in the standard economic model, it is assumed if there's competition, that the prices are given and cannot be affected by the seller, so all the seller has to decide is how much to sell. The decision about the prices is not his to make. The price is determined in the market. Well, but if you take Chamberlin seriously, one of the things that you're led to do is to study the effect that sellers actually can have on prices, and that they do exert on prices. So for example, this comes up in studying the effect of competition of the ten-cent brands and the standard brands, and what I did is, I tried to estimate price elasticities for branded goods using data from consumer panels, so that we could see the effect on the consumers of price changes.

Now the panel data was collected by a firm called Market Research Corporation of America, MRCA, and one of the founders of the company was Oskar Morgenstern, who was also interested in this theoretically, but of course, he was interested in it as a way of making money. So I used the panel data to estimate the demand for branded goods, and I think it was frozen orange-juice concentrate, margarine, coffee, and some others. And the model that I used did assume that there was a certain amount of lag, which is, by the way, related to the theoretical research I was doing, that the basic model was a Markov model, that is the probability that you buy the product today, depends on the probability that you bought it yesterday. And if there are a bunch of brands in the industry, what you have is transition probabilities between pairs of brands. So I was interested in estimating aggregates of these transition probabilities as well as the effect of your previous market share on the current market share, as well as the effect of the price, your price, relative to the prices of competitors.

So I got estimates of market-share price elasticities, and at that time, I don't think anybody had ever tried to do it before. So this was pretty novel stuff, to see how much price competition there really is in a market for these highly advertised brands. And one of the results, I remember, it's kind of interesting, that if a brand had a price special, so they lowered the price—and sometimes, or often, the way they would do it is by sending out coupons, price cuts, to the customers. And one of the things that I found was that it took longer for these price cuts to effect an increase in the sales, and presumably, the purpose was to build up some kind of brand loyalty, to give customers an incentive to try the product at these lower prices and still continue to buy the product because they liked it, even when after the price had fallen to the previous level. Well, it was asymmetric. The shape up was slower than the decline coming down, which was surprising. And of course then, the other thing that you get out of this is the difference between the short-run price elasticity and the long-run price elasticity. The long-run price elasticity, well, it depends on the importance of the lagged sales.

06-01:38:48

Burnett: It's enormously complicated, the kinds of topics that you are working on with the graduate students that worked on this, and the theories of competition in your own research, and not to make things even more complicated, there's a striking chapter in *Competition, Collusion and Game Theory*, and that's chapter eight on "Returns to Manufacturing," and it is all about the complexity of the data and the complexity of interpreting the data. I'm wondering if you could talk a little bit about this. This is about assumptions that people make about the relationship between the corporate concentration ratio, that classic four-firm concentration ratio, and rates of return, and this assumption, how can that persist without some kind of monopoly power or manipulation or protection from market competition? And it's extraordinary what you do in this chapter. Can you talk a little bit about the wrestling with the complexity of the data?

06-01:40:12

Telser: Well, the first problem is related to what came up earlier, and that is, what is it? You see, one way, the conventional way to look at this is that the higher the concentration ratio, the less competitive the industry. So the concentration ratio is supposed to be a measure of how competitive the industry is, and if it's very high, then there isn't that much competition in the industry, so the standard argument would be that the profit margin should be higher. But then the problem that this raises is about cause and effect. It may be that the reason that the concentration ratio is high is because the leading firms are able to produce products that are more appealing, or cheaper or better, than their competitors. And one of the subtleties that comes up here has to do with the variability in the quality of the product.

One of the things that I found from a study—I think it was done at the University of Florida—is that they compared measures of the quality of

branded goods—maybe it was orange juice or something—with the off-label brands. And one of the things that they found was that the quality varied more for the off-label brands than it does for the standard brands. And then, and this also goes back to what you think about consumers, that even if you know that this product is cheaper than that product, so why should I buy it? Well, I don't buy it because it's advertised, but I may buy it because I can count on it; it's always going to be okay. Whereas, for the off brand, sometimes it's not going to be okay and I'm going to get stuck.

And an interesting manifestation of this, I think, is that what kinds of products tend to be branded versus the kind of products that tend not to be branded? And I think you will find that fresh fruits and vegetables tend not to be branded, and I think one of the reasons for it is that it's very difficult for the wholesaler, or the producer, whoever is selling this stuff, to ensure a standard of quality for these goods; whereas, for stuff that you buy in cans or boxes or whatever, that where it's easier, cheaper to control the quality, are more likely to be branded. And so that there's a problem of cause and effect. But one of the things I also found: I think one of the products I may have been looking at might've been instant mashed potatoes, and it was a new product, and the leading brand was very successful, and the market share was initially very high, and then it declined, and it declined, not because that the buyers of instant mashed potatoes switched to a competitor, but because there was entry, that the pioneering firm opened a market that was entered by other firms who hadn't thought of doing it.

So all of this enters when you start looking at aggregate data that relates the concentration ratio to the rates of return, and one of the problems that you run into here is that the concentration ratio depends on how the Census of Manufactures classifies the product, and decides what category to put it in and so on. So then you get into the mode of classification: two-digit industries, three-digit industries, four-digit industries, five-digit industries. As the numbers go up, the classification gets finer and finer. And I know, I remember one of the problems that I ran into was that semiconductors and vacuum tubes, that the semiconductors were relatively new products, and the Census Bureau, they changed their mind about what category to put it in, and then they had to invent new categories to accommodate the new products. So then you get into trouble. If you're using the standard data and you don't really know what's going on there, you can get results that are very misleading, because of the way the products are categorized. And I also found there was a nonlinear relation. The relation between market share, or concentration ratio, and the profit rate was much stronger in the higher concentration ratios than in the categories with lower concentration ratios.

06-01:47:35

Burnett: One of the really interesting parts of that chapter is that you start to talk about and uncover how capital is counted, and there's uncounted capital among the firms in the concentration ratio, such that the uncounted capital overstates the

returns to capital, basically. And it's not just one thing. You start to unpack this layer by layer, and it does kind of undermine your trust in a simple formula, say, this concentration ratio is related to this kind of return to capital. And you do the same thing for human capital, and so that you're looking at the 1963 Census of Manufactures and the Bureau of Labor Statistics, statistics on labor and job turnover, and you use that as an index for understanding the investment of firms in their labor force, and that that's part of the story as well, and counting that is so difficult.

06-01:48:59

Telser: There's firm-specific human capital, and when there's a new hire, the new employee, depending on the job, of course, is learning about the business, and so the marginal product to the company is lower than it would be later when the employee has been more fully trained, so that there's a pattern of the wage rate for a given employee that depends on the length of employment with the company. And then, one of the things that I point out is, and one of the ways I measure it is, with the turnover data, quit rates and layoff rates, to distinguish between quit rates and layoff rates. In other words, when the employee leaves the company, is it because the employee was fired by the company, or is it because the employee quit the company? And so, these things are correlated with the concentration ratio. It is also correlated with how successful the company is, so that a more successful company, let's say that has a higher share of the market, may also have a lower turnover rate for its employees than a less successful company.

06-01:50:56

Burnett: So you were talking about the investment in human capital that these large firms undertake, and how that's related to the concentration ratio and the returns to capital.

06-01:51:10

Telser: What I was going to say is that there's a difference in gender, that is, a turnover rate tends to be higher for women in certain industries than it is for men. And in particular, I found, in the semiconductor industries, where some of the work, it's not as demanding, it's more repetitive, so that they tended to hire less skilled people who did not spend as much time with the company, higher turnover rate, and so on. So some of this stuff enters into the rate of return. And this is something that the companies themselves are very much interested in.

By the way, this reminds me of a very interesting dissertation by someone who was in my workshop—I just happened to think of it—Mason Rosenthal, who did a dissertation on the location of gasoline stations, and this was sponsored, in fact, by Standard of Indiana. And what he wanted to do was to figure out which stations are more successful. What are the factors that determine how successful a particular gasoline station is? And what was particularly attractive to me about Mason, as I recall, he did not have very good grades, but he was a very smart guy. I talked to him, and I didn't really

care about his grades, and I think when we got to know each other better—I think he was probably bored in some of these classes. But his dissertation was very clever, and he got very good results, and Standard of Indiana learned from it and they were very happy to get it, and it said something about what it is that affects the success of a gasoline station. It was very interesting.

06-01:54:03
Burnett: And I think that there is a sense of the influence of working with these students, and some of those students helped you do some of the research and some of the analysis. I think Hodson Thornber helped you with the returns. Was it the returns to manufacturing or the stuff on orange juice? I'm not sure.

06-01:54:25
Telser: I think it was manufacturing industries.

06-01:54:27
Burnett: Well, next time we'll talk about the core theory part of this. This volume is part of the Aldine Treatises, which is, well, it's a series in new economics, and between University of Chicago and London School of Economics, and Harry Johnson was the editor. So, can you talk about how the book project came together? Were you invited to participate in this series? How did that work?

06-01:55:07
Telser: Harry Johnson and I were very close. As a matter of fact, when he came to Chicago, which I think it was maybe 1959 or so, very early, the first sort of dinner he had on arrival, Harry and Liz, was at our house. And what I remember was that Harry then left to go to a Political Economy Club meeting—this is all sort of by the way—and at the Political Economy Club meeting, he delivered a very critical lecture on Milton Friedman, which the students there were very surprised to hear, and then he came back to our house. Anyway, Harry and I were very close, and often with Liz, and he was a tremendously energetic guy, tremendous. It was incredible. As an editor of the *JPE*, he would leave the office and be carrying ten, twelve, fourteen manuscripts. He would read them all and the next day, decisions made on all of them. He was just a phenomenon, a big guy, and he was interested in my work, by the way. So he was he was the main person responsible for me doing this book with Aldine.

06-01:57:07
Burnett: And so he invited you to participate in this, and then this became the project. Well, why don't we leave until next time to talk about game theory in this book, and develop that theme a bit further as we get into the 1970s.

06-01:57:23
Telser: I just have to say one other thing about Harry— that when we would go to London, I would call him. Apparently, it's not an uncommon name, so there was somebody else in London named Harry Johnson. So we'd say, "I'm looking for Harry," and he must've gotten this call, I don't know, twenty,

twenty-five times. He was very nice about it. “Oh,” he said, “you must mean so and so.” So [laughter] he put up with this.

06-01:57:56

Burnett: He’s probably got a list of Harry Johnsons beside his telephone. Well, we’ll pick that up next time.

06-01:58:02

Telser: Okay.

Interview 7: October 17, 2017

07-00:00:18
Burnett: This is Paul Burnett interviewing Lester Telser for the Economist Life Stories Project, and we're here in Hyde Park, Chicago, and it is October 17, 2017, and we were beginning to talk about, *Competition, Collusion and Game Theory*, but you were talking about the students that you had at that time, and you wanted to enter a correction.

07-00:00:55
Telser: Yes. The correction is that Kris Palda's dissertation was on the advertising outlays of Lydia Pinkham, and the data was the result of a lawsuit between two groups of shareholders, one claiming that too much was being spent on the advertising, and the other denying it. And so, as a result of the lawsuit, we got a huge amount of very valuable data that Kris could use in studying the tangible capital built up as a result of the advertising outlays. That is, he could treat the advertising outlays as creating a source of capital that did not appear normally on the balance sheet of a firm.

07-00:01:56
Burnett: So this is an enormous resource for economists that doesn't always emerge. It's a bit like the fossil record. Sometimes you get lucky, and there's a court case that contains a wealth of economic information. And so someone lets you know that this stuff is available, and you hear about it, and you can go through it, but it's not always visible to the profession, is it?

07-00:02:21
Telser: No.

07-00:02:23
Burnett: So last time, we were beginning to talk about Cournot, and the Cournot model for competition, and I remember you talking about Aaron Director and his puzzles. And so there's a puzzle about competition and the classical model of competition, so I'm wondering if you could lay that out for us as we talk about how this book came together.

07-00:03:00
Telser: Well, competition was not a rigorous subject in economics until perhaps Cournot studied it and published a book in 1838, giving a mathematical model of competition between firms, in his case, starting with two firms in Vichy, France, who were competing and selling Vichy water. And so, he introduced demand equations and cost equations, and assumed that each would choose the output that would maximize profits, while recognizing that the output, the total output, would be affected by the quantity that the two of them were selling. And he showed, among other things, that as a result, because they are allowing for the effect of their sales on the market price, they would make more than a competitive return. The more interesting question he developed was, how would this change as the number of sellers increased? So he's really one of the first to try and give an explicit mathematical model of the

relationship between the number of sellers and the price relative to the cost of the product.

So that really started this whole area, and I think it was largely ignored. Not many economists really knew about it, and many economists tended to be hostile to the use of mathematics. And I think that the first one after Cournot to study it, in a much more sophisticated way, was Edgeworth, F. Y. Edgeworth, and he published his book in 1881. And in fact, I would say Edgeworth's model, which discusses exchange in a market and shows how, as the number of traders increases, what we call the contract curve—that is the locus of possible equilibria—shrinks down to a point, and finally, when you have an infinite number of traders in a market, it shrinks to a single point that is the competitive equilibrium. And what is remarkable about Edgeworth's model is that he did not assume a demand curve and cost curves or anything like that. He simply assumed that the traders derive a utility from a bundle of goods.

So he really introduced what we call indifference curves, that is, what collections of commodities a trader would regard as yielding the same amount of utility, and it's a very sophisticated approach. I would say that after Edgeworth, many economists refer to it, but very few actually read it. Even though the mathematics wasn't that complicated, the logic, and working it through, is not easy. It's very difficult to work it through. Edgeworth is, himself, a most remarkable person, and I believe he was also the founder of the *Economic Journal*, which was the official publication of the Royal Economic Society. He's also a very important contributor to statistics, especially mathematical theory of statistics, so he is one of the great economists.

07-00:07:56

Burnett: And so, that gives us kind of the sort of classical and neoclassical efforts to render a more sophisticated portrait of competition. Now, in your exploration of this, were you basically teaching history of economics in your seminars, and that it came to you that way? How did this come to you in your work practice?

07-00:08:24

Telser: Well, actually, I would not, in my classes. I very rarely would talk about what predecessors had done, so we really did not go into it in classes. I did write about it. I would refer to it in publications, but I had a very different approach in teaching. In fact, one of my favorite, you could call it a ploy or gimmick, is I would refer to movies, and I would use certain movies to illustrate some complicated mathematical propositions that were certainly not obvious if you read the formal article. I don't know if we want to talk about it now, but there were several, I had several favorite examples.

07-00:09:33
Burnett: Well, I think you embed those examples in your published work, and you did talk about *The Treasure of the Sierra Madre* before, and we can bring it in as needed, and *Prizzi's Honor* comes up later, in some later publications, so we'll cover those as we come to them.

07-00:09:50
Telser: And the Zero Mostel constraint. [laughter]

07-00:09:55
Burnett: So that's a good pedagogical tool to communicate these ideas to your students.

07-00:10:05
Telser: When I first started to teach, I remember I was teaching a course in econometrics, and I think that might have been in 1961or so. Actually it was a fairly technical course in the Economics Department. It was not listed in the business school, but it was an Economics Department course, and I made the mistake of doing it in a very abstract framework. I used Hilbert spaces, and the enrollment in the course dropped from about forty to three, and the three students who ended up in the course turned out to be very outstanding. One was Bob Lucas, G. S. Maddala, and Neil Wallace. Of course, and Lucas got a Nobel; Maddala, unfortunately, died at a fairly early age, but he was a very, very smart guy, and was also my research assistant; and Neil did a lot of work on the term structure of interest rates, and he did his dissertation with Harry Johnson and me. So those three turned out very well. I won't mention some of the people who dropped out, but there was considerable attrition, and I learned from that: don't do it again. [laughter]

07-00:11:53
Burnett: Well, this is very sophisticated mathematics, and you need to be able to reach people. And so, coming back to these definitions or mathematical elaborations of a competitive environment, so with Cournot and Edgeworth, there is an updating with game-theoretic conceptions of competition. Can you talk about how we move from a basic Cournot model, which is relatively static, to a Cournot-Nash model, and what the differences are, briefly, and why that's important?

07-00:12:34
Telser: Well, one of the big problems with the Cournot model is that it depends on what firms think the other guy is doing. So it introduces expectations as part of the strategy of the firm. Now, what Cournot himself assumed is that each firm acts on the basis of what the firm did in a previous time period. So that's a very naïve kind of expectation, and then, when you try to treat it formally as difference equations, it turns out that you may not get a stable equilibrium. When you're talking about difference equations and things that are happening over time, it raises the question of convergence. Will the thing settle down to something, or will it just blow up?

Well it turns out that, when you start looking into expectations, some expectations blow up, and some settle down, but in any case, treating expectations in this sort of a mechanical way, it seems kind of arbitrary. It's not a natural way to study the problem, and it's always open to the objection, "Well, why are you assuming that they behave in this way? Why aren't you assuming they do something else?" And it's very hard to defend, so then what happens on some of these expectations models is that you get convergence, and in others, you don't, so presumably, we should prefer the models that give convergence. But then there's a problem: there may not be only one kind of model that gives convergence, and it still seems kind of arbitrary.

07-00:14:51

Burnett: Right, what's the causation that results in convergence?

07-00:14:55

Telser: So, that was a very difficult problem. Now, then there's a more fundamental problem with the Cournot model, and this is relevant not only to what I was doing, but also to what Ronald Coase was doing, and that is, even if it does converge and you do get an equilibrium—and this actually applies also in monopoly—money is left on the table in the sense that it's not an efficient equilibrium. It would be possible, by changing certain things, to make some people better off without making anybody else worse off, and so this became a formal part of economic theory introduced by an Italian mathematician, Vilfredo Pareto. Now the fact that he was also a sympathizer of the Fascist Party in Italy somewhat colors the attitude that some people have to him. But the fact is, he did introduce an important concept into economic theory apart from his views on politics. And so a Pareto optimum means, it's a situation where the only way somebody could be made better off, is if somebody else is made worse off. However, if there's a situation in which it is possible for somebody to be made better off without anybody else being made worse off, then why don't they do it?

And so, when you look at a Cournot equilibrium, because it's not efficient, because it leaves money on the table, it raises a question. And this is really the basic point, I would say, that Coase tries to make, and very early when I was just learning about core theory, this issue comes up, because when you define a core, you assume that the players are at a Pareto optimum. But you don't say anything about how they get there; you say, "Well, of course they'll be at a Pareto optimum, because otherwise, they would be leaving money on the table." Now surely you're not going to argue that people don't make mistakes, and sometimes, for whatever reason, money will be left on the table. For instance, it depends on whether I will be worse off in this situation, and you will be better off, and you might not worry about the fact that I will be worse off, as long as you're better off, so that we don't adopt kind of an altruistic view of what happens in the equilibrium.

A better way to think about it is, which is essentially the way Adam Smith thought about it, that it isn't that we consciously think about the public interest; the question is whether what we are doing in the course of our ordinary behavior leads to an efficient outcome. So it raises the very serious question of, what kinds of things will lead to a good outcome, and what kinds of things will not lead to a good outcome? And when you talk about a Cournot equilibrium as something that does not lead to a good outcome, how do you defend it? Why does it happen, or what's going on here?

07-00:19:05

Burnett: Right. And there's also something that is introduced in the Cournot-Nash equilibrium where there is adaptive expectations, and that's different too. Can you talk about how that differs from the old model, or is that a bit too mathematically obscure?

07-00:19:28

Telser: Well, adaptive expectations is one of the ways to respond to the criticism of the Cournot assumption. The Cournot assumption says, "I'll assume that what these guys did yesterday, they're going to do today. I know goddamn well they're not going to do the same thing today that they did yesterday, but I'm going to assume that." So then it raises the question, instead of just looking at what they did yesterday, maybe what we should do is look at sort of an average of the things that they did in the past, not just what they did yesterday, but some kind of a weighted moving average of the things that they did in the past, and maybe that would give us a better prediction of what they're going to do tomorrow than just looking at today. The basic idea of it seems reasonable. It's saying that it's usually better to have a bigger sample than just a sample of one. And so there are very fancy ways of choosing the weights for the past observation, in the hope that it will make better forecasts.

Now, for example, and some of this became a very serious part of economics—Friedman's permanent income model, for example, is a case where he assumes that the consumers' consumption does not depend on current income, but depends on permanent income, and the permanent income is some kind of a weighted moving average of the consumers' past income. Now the trouble with that kind of a model is, what weights should you use? See, again, it raises the question, well, maybe there would be a better way, a better average to use than just a simple average, and this gets into statistics, where it does become a serious problem in sampling what in statistics they call these efficient estimators. So an efficient estimator not only takes into account the difference between the actual observation and the prediction, but it tries to find a prediction that will minimize the error in a certain sense. And the way it does it is, it calculates the standard deviation of the error. That is, it calculates the square of the difference.

The reason it uses the square is because you want to take into account overestimates and underestimates, and if you just took the sum of the errors,

you would get the difference between the plus estimates and the minus estimates. But you don't want to do that; you want to take into account all the errors, ignoring the sum, so the way you do it is, you square it, take the sum of the squares, and then you take the square root. So you calculate the square, and then to get rid of the distorting effect of doing the squares, you then take the square root of the sum. It may sound funny when I put it in this blunt way—

07-00:23:43
Burnett: No, that's actually really clear.

07-00:23:44
Telser: —but that's really what's going on. And the first person, in fact, who proposed this in his work in astronomy was Gauss. He is the one who invented what we call efficient estimators when they were making observations of asteroids and planets and stuff, and there would always be an error. You never got it exactly right, so you'd try to figure out where is the planet, by figuring out how to correct for these errors. Now, there's random errors and systematic errors. The systematic error is called the bias, and a random error is what you try to cover by the formula that you use to weight your observations.

07-00:24:37
Burnett: So you're applying sophisticated tools to develop more robust estimates for the body of work that you're establishing. Let's pull back from that, and I think, in this period, with your generation, you're dealing with this question of markets with these disequilibria, right? You have price variations when, according to classical and neoclassical theory, there should be some equilibrium price, right, in a single market, so you're finding there are submarkets. There are price differences for the same product, in the same area. Why is that? And different intellectuals and scholars, even at Chicago, developed different ideas about how to treat this.

07-00:25:32
Telser: Yeah, well, in fact, the one who studied it very systematically was Stigler, because in his search theory, the argument that he used, which is a very reasonable argument, is there are a lot of prices out there, and it's sort of like trying to estimate the average weight of adult males. So there's a lot of adult males out there, and their weights differ. So how do you figure out their average weight? Well, you take a sample and you take an average. But in the case of prices, it's a little bit more complicated, because the prices change. With adult males, in most cases, their weights will be pretty constant over reasonable periods of time, but in the case of markets, that isn't going to be true. Prices may change; some will go up, some will go down and whatever. So the problem is, how does that affect what happens in a market?

So what George said was that it's as if the buyers are taking random samples of prices, and they look at the price—although I'm making it a little bit more

sophisticated, in fact, than he did—but they sort of take a price one at a time, and they look at it. And then they have in their head the notion that, how much will it cost for me to draw a price? There's a sampling cost. And so I look at the price I got, and I have in mind the sampling cost, and I think that, well, if I take another price, will it be lower enough, relative to the one I have in hand, to justify the cost of search? So that what happens in his model is that there is some kind of an acceptance level, and if you draw a price that's in the acceptance region, you stop search, and you take it. And if it's not in the acceptance region, then you keep on going, and what really governs this is how much variability is there in the prices in the population and how much does it cost to sample?

When you pursue this, it runs into an obstacle, and one of our students, Allen Jung, did a dissertation on this for automobile prices. Now Allen's main hobby, I would say, or interest was, going around to auto dealers and pretending that he wants to buy a car, and trying to get the best price from them, and you'd have to see him. He was sort of a formidable-looking character. He didn't look like Joe Louis or anything, but he was a tough guy, so when he would go into the auto dealership and start bargaining with the salesman, it was not a one-sided operation. The salesman had met his match. And so he collected data from a lot of dealers, and the problem that he discovered was, you would think that if this stuff is going on, that it would be in the interest of the dealers to know what the other guys are doing, and so if they think that their prices are out of line and they're losing sales, that they would lower their prices in order to pick up the sales that they were losing.

And what he found was that there were persistent differences among the prices for the same model, and it's very hard to understand. See, if they were colluding, you would say, "Well, sure, they're all going to sell it at the same price." But that isn't what was happening. Also, I should add that in the car market, there is the complication of the trade-in, so getting prices which not only takes into account the retail price, but the trade-in allowance, and the other stuff that's going on, is more complicated. But the bottom line was that there were persistent price differences among the dealers, and that really wasn't consistent with Stigler's search model. So it was a puzzle. How do you explain this? This is the sort of puzzle that Aaron would like, you see something that seems to be contradicting standard economic theory.

Now I can tell you what would seem to apply here, and that is, a dealership, it's not just that you have identical dealers selling an identical product. That's not what's really going on in the real market. The dealers differ, and some of the dealers are fancy and offer more services, et cetera, and others are more bare-bones dealers. And if you start looking into this, you will find that what explains the systematic differences in the prices of the cars among the dealers has to do with the nature of the services that different dealers are providing—and other factors, but that was the main factor.

07-00:32:07
Burnett: So, there's an economics of information that emerges to explain some of this price variation. Could you talk about the application or the development of core theory? Is that a similar kind of response with a different logic, or a different explanation? You incorporate economics of information into your work, right?

07-00:32:29
Telser: Yes. One of the things that happens is that, if you write down a model based on the constraints that core theory suggests, what happens is, each bargain between a buyer and a seller creates an inequality, which determines the minimal terms they would be willing to accept. And then in core theory, and also in Edgeworth's theory, these are considered to be tentative contracts. In core theory, they're called coalitions. The reason for calling them: it's an unfortunate term, but it goes back to the notion that it originates in the theory of games where there's hostility and coalitions and all of that stuff. But basically, what a coalition represents is a tentative agreement between a group of traders. And so they leave, and they say, "I'll get back to you. I'm going to look around, and I'll get back to you."

And so in core theory, what you do is, you represent all of this stuff with a bunch of inequalities, and then, what you say is, "Is there some solution of this set of inequalities such that nobody could get a better deal?" And it may seem surprising, if you don't know anything about linear inequalities, that this would ever happen, but in fact, it does happen. It turns out that what, under certain fairly general conditions, there will be a solution for the whole set of inequalities, that will have the property that the final deals will be accepted by a subgroup of traders, and there will be other traders who don't accept the deal because they figure they're better off standing pat than going into this submarket. So that, what you're really talking about is the machinery of a competitive market, when you're using core theory.

07-00:35:30
Burnett: And by inequalities, you mean the minimum you're willing to sell for, or the maximum you're willing to pay.

07-00:35:35
Telser: Yes, if you're a buyer, it's the maximum you're willing to pay, and if you're a seller, it's the minimum you're willing to accept. Now if you have a coalition that has both buyers and sellers, then what the model assumes is that you calculate the net gain for the coalition, so whatever terms they're going to get in some other coalition has to be at least as good as what they could do off by themselves. And the terminology that is used in the formal theory is that the points in the core are undominated. Well, to say "they're undominated" is the technical language that the mathematicians use, and what it means is that these are terms that nobody would be capable of objecting to it by getting better terms. That is, the buyer would like to get a lower price, the seller would like to get a higher price, but they can't do it. A buyer can't get a lower

price unless he can find a seller. A seller can't get a higher price unless he can find a buyer. So that when you get into this kind of a situation where you have an undominated solution, you have what we call a nonempty core. That's the best you can do.

And there were some people that, for whatever reasons, they somehow couldn't understand this. I think they thought that was attacking economics or something when I described it in these terms, but that isn't true. What I'm really talking about is the way a market presumably works, and if you actually go down to the Board of Trade, you can see before your eyes, this is really going on.

07-00:37:50

Burnett: Well, let's try to explore that. What economic theory or principles, or ways of seeing—let's put it that way—does the idea of the core challenge? What's threatening about it?

07-00:38:13

Telser: Well, first of all, to use the term "coalition" bothers people, and I think that the language that Edgeworth used is more palatable. I think he called it "recontracting," and then for historical reasons, because of the way game theory originated, and actually because Morgenstern is an Austrian, von Neumann is a mathematician, and the Austrian economists really didn't know English economics very well. So I don't think that Morgenstern knew very much about England, that's my—

07-00:39:15

Burnett: And about Edgeworth, right.

07-00:39:16

Telser: —opinion, and so he tended to use language, and of course, the other thing I think that's important to keep in mind, and it may sound silly, but they probably spoke to each other in German. I can't imagine that von Neumann and Morgenstern, in working together on their book and all that, would use English. I'm reasonably sure that they would use German, and they would use the terminology that the German and Austrian economists would use, and nobody has actually explored this. It's not the sort of thing I'm terribly interested in, but I think it would be very interesting to see what the language they probably used was, and how it would relate to what they published. Now, the book is published in English. They did do it in English, but I'm pretty sure that when they were working it out between them, they probably talked to each other in German. And I don't know what the terminology was in German that would correspond, so they did not use "recontracting," and I'd be willing to bet you at least one Reichsmark that Morgenstern would not have used the term "recontracting" or any of that.

07-00:40:50
Burnett: Well, I can think of another; something you bring up in *Competition, Collusion and Game Theory* is group rationality, and so, given that the game theory was about individual—so, rational expectations theory, that's individual decision making, right? Group rationality – and we talked about this in an earlier session – but let's just explore it in a bit more detail. Does this somehow threaten the kind of individual basis for decision making in a market according to theories by Friedman, Stigler, and others?

07-00:41:36
Telser: Yes, very much so. Very much so, because the really hard problem, the classical example of this, aside from the movie and the book, *The Treasure of the Sierra Madre*, is the legislature. In a legislature, a law is proposed, and in Congress, most of these laws will pass if it gets a majority in both Houses. And party discipline is not as strong, I think, in the US as it is in the House of Commons, but the British system is of course very similar to the American system, but party discipline in the House of Commons is much stronger. So if you're a Tory and you voted with the Labour Party, I think the Tories would probably kick you out. I'm not sure about it, but that's my impression. Whereas, in the US, it is understood that you have other loyalties; your loyalties are to your constituents as well as to the party. We won't go into what's going on now, however. But then if you do the formal theory, it says that, if you can put together a majority, let's say, fifty-one out of a hundred, then the thing will pass. But in order to do that, the guys, the majority group, presumably offers certain incentives to the members of the group in order to accept this legislation.

Well, then you begin to get into the bargaining process that game theory starts talking about. So there might be some guy who's in the minority who will say, "Look, I'm willing to join you, because I'm not going to get anything in the minority party. I'm willing to join you on very good terms if you give me some kind of a deal." So what they'll do is, they'll kick out somebody and replace them with a cheaper vote. And when you work this out, in the case of majority rule, it turns out that it breaks down; you get an empty core. It is not possible to have a stable coalition made up of a majority of the members. So I wrote an article on "Voting and Paying for Public Goods," and I said there's a very critical factor that's involved here, and that factor has to do with whether the majority can shunt the cost of the legislation onto the minority. And if they can, then that tends to destabilize the legislature, because then, there is an incentive to make the majority as large as possible.

But then there could be a different rule, and the other rule makes it into what I call a semi-private good, that the majority has to bear the whole cost of what they do. So, if the majority has to bear the whole cost of what they do, and they can't shunt it onto the minority, that gives them an incentive to get as many people into the majority as possible. And so in fact, one of the predictions of the theory of the core, to get a nonempty core, is that they won't

just use majority rule, but what you will see is very large majorities. And how do they get very large majorities? They get it by logrolling. See, how do you get guys from the minority to join you, so that you can reduce the cost to yourself? Well, you do it by promising them stuff, and if you actually look at some of this legislation, you see things that surprised the naïve, but not what you might call the more realistic students of the problem, actually Al Harberger is a very good example of what I'm talking about. He is very much aware of what goes on here. You will see that there were congressmen in Manhattan that were voting in favor of price supports for corn in Iowa or someplace.

07-00:47:19
Burnett: Absolutely.

07-00:47:20
Telser: And why are they doing that? Because they're working out a deal. "Okay, we're going to vote for your corn in Iowa." And the guys in Iowa will say, "Fine, and we're going to vote for food stamps in Manhattan," or wherever, and they work out these deals. And then, what is interesting about it is, if you actually look at the numbers on these votes, and I had students who did that, what you will see is that legislation usually passes in the US Congress with huge majorities, until the last eight years, when we had amateurs running the government. But before that, when we had professional politicians who knew which end was up, we didn't run into these problems. I mean, LBJ [Lyndon Baines Johnson] did not get the health bill by telling the Republicans to go shove it.

07-00:48:21
Burnett: Yeah. No, that's very true. There's an interesting connection, too, with Ted Schultz, that he was involved in developing new rationales for agricultural policy that could then be sold to other constituencies not involved in agriculture. Not that he was a huge supporter of agricultural subsidies, but he developed a different rationale that wasn't based on giving a farmer a price that they quote/quote "deserved." Anyway, the question I wanted to ask you about that is, does that compare with say, Arrow's impossibility theorem?

07-00:48:58
Telser: That's a very good question. Here I'm going to delve into history. The thing that really astonishes me: Now I know Arrow's supervisor on his dissertation was Harold Hotelling. He did his dissertation I believe, or most of it, at Columbia, and Harold Hotelling, who is a very eminent statistician, was the supervisor. What I really don't understand is how they could be so ignorant of the very extensive writing on this stuff that took place at the time of the French Revolution, and in particular, the Marquis de Condorcet. I think I mentioned in something that he was really the first person, I think, to apply mathematics to the social sciences, and he studied majority voting and all of that in the process, when they were trying to develop the French Republic. And he was a great admirer of the American Constitution, which had just been

finished in 1787. Condorcet, a lot of his writing is in the 1780s. He knew Benjamin Franklin, who was in France, earlier, trying to get help. Well, the French were willing to help the colonies, not because they were in favor of the republic, but because they were against the British Empire, but never mind; we won't look into motives here.

Anyway, so Condorcet was aware that when you have voting in a legislature, you get funny results that violate what mathematicians call transitivity. *A* beats *b*, *b* beats *c*, but *a* does not beat *c*; *c* beats *a*. So there was a tendency to say, "Well, this just shows that people aren't rational." Now, there were psychologists, not to mention historians, who were presumably students of the French Revolution—oh, and by the way, Condorcet and Robespierre, I guess you would say they're enemies. I think Robespierre was more of an enemy to Condorcet than the other way around, and Condorcet was found dead in his prison cell at the *Invalides*, so, we think that he was probably murdered by henchmen of Robespierre. And the French, they are sort of embarrassed by this. I don't think they put Condorcet into, I forget what it's called. The Heroes or something, the Academy, I don't remember. Anyway, they didn't—

07-00:52:52
Burnett: The Legion d'Honneur?

07-00:52:53
Telser: —actually put Condorcet in there until fairly recently.[10] Nobody knows where his body was or anything like that, but I think there's a statue among the great French thinkers. Anyway, so apparently they [Hotelling and Arrow] didn't know about Condorcet's work, and I got intrigued because it didn't seem to me that this result of Arrow, although I was very much taken with it when I first read about it, didn't seem to me that obvious.

Now it's true [transitivity] if we're talking about what kind of ice cream you like. If I like chocolate more than vanilla, and I like vanilla more than strawberry, then I'll like chocolate more than strawberry. So maybe it's okay if we're talking about ice cream flavors, but if you're a baseball fan, you know goddamn well this doesn't work, that the Yankees beat the Red Sox, the Red Sox beat the White Sox, and it's not unknown, believe it or not, for the White Sox to beat the Yankees, so that's a violation of transitivity. And this is pretty well known to sports fans, and as a matter of fact, psychologists who were studying people, preferences and how they make up their mind—there was an article in the *Proceedings of the National Academy*—I think it was in 1938—it refers to a violation of transitivity in experiments. That happens to be one that I know about, but it was pretty well known by psychologists that if you're talking about comparisons between things, it really is like a contest, a sporting

[10] Cordorcet was interred symbolically in the Pantheon by French President Francois Mitterand, December 12, 1989.

contest, like a baseball game, and it's not obvious that rationality applies and always leads to transitivity. It does not.

And I have to say, in going back to another topic we discussed a little bit, and that's von Neumann, in the appendix to the *Theory of Games*, because I had occasion to read it very recently, and he also I think is on shaky ground. In mathematics, when you violate transitivity, it's called acyclicity—you know, "not cycles"—and that bothered him. But I can't take that terribly seriously, although I have to admit, it's in the second edition that he discusses it, but in the second edition, in 1947, I think he probably knew that he was dying, and I don't take seriously everything that he was doing between '47—I think he died in '52 or so.

07-00:56:14
Burnett: I think maybe a bit later; is it '55? [John von Neumann died in 1957, but was diagnosed with terminal cancer in 1955]

07-00:56:18
Telser: —a little bit later. But it doesn't bother me. It not only doesn't bother me, I think it's very important, acyclicity.

07-00:56:33
Burnett: But, I think your point is that this got taken up by the profession, that part of it, [in the appendix to the second edition of *The Theory of Games and Economic Behavior*] and this kind of insistence on an individual basis for rational expectations—

07-00:56:45
Telser: Exactly, without—yes.

07-00:56:48
Burnett: —can have a limiting effect on the imagination, right? Let's say that.

07-00:56:53
Telser: Yeah, in fact, there's another point that is related to this. I think it's related to a question that you raised in the last memo you sent me, and that is the differences between what I'm doing and neoclassical economics. This is something called revealed preference, which was invented by [Paul A.] Samuelson, and actually made him very famous. And what Samuelson said is that, "if you bought *a* and you could have bought *b*, that reveals that you prefer *a* to *b*." And then, if you carry that through a sequence, it leads you to making transitive choices. Now, in terms of the theory, it leads to predictions from the standard theory that are obviously wrong.

Here is a practical implication of revealed preference. It has to do with what economists assume is a symmetrical effect of the change in price on different commodities. So for example, take the demand for tomatoes. How does the demand for tomatoes respond to a change in the price of lettuce? So, according to the Samuelsonian theory, it would say, "A change in the price of

tomatoes with respect to a change in the price of lettuce is the same as a change in the quantity of lettuce with respect to a change in the price of tomatoes," which is certainly not obvious. It's not only not obvious, it's probably not true. And the reason you get into this result is when you start translating this stuff about revealed preference into utility functions, and you impose certain mathematical conditions on the utility functions, you get a kind of a symmetry that cannot really be justified empirically. But transitivity has been pretty much regarded by many economists, unless they've really thought about it, as the hallmark of rationality. Anybody whose preferences are not transitive must have something wrong with them.

07-01:00:04

Burnett: And so, in core theory then, the impression that I get, and I think you wrote something in *Competition, Collusion and Game Theory*, you incorporate the fact of costly information, so economics of information is part of your research, but there's something about the core, or coalitions, that permits people to make decisions without necessarily having information, or necessarily the right information with which to make these perfectly rational decisions. There's an iterative approach to coalition building. That's my limited interpretation of what you were talking about.

07-01:01:01

Telser: Now here, we get into something about the theory of the core that's related to the more recent work that I'm doing. In the classical core theory, where you talk about coalitions and a sort of static, you don't really take time into account. You're not worried about the sequence of events. You act as if all of these things can happen simultaneously, and this is also true in Edgeworth's recontracting model. It's as if these guys get together, they work out a tentative agreement then they split up and they wander off and they make up another one, and all of this happens sort of instantaneously and timelessly. And that sort of is carried over into core theory, but once you start taking time into account and the fact that there's a very definite relation between things that happen now, and what happens later—there are sort of obvious examples of this. If you're growing wheat, for example, you plant the wheat and the wheat grows, and then you harvest it. You don't get the wheat instantaneously after you plant it.

So, when you start doing this sort of thing, the kind of theories that I've been working on most recently involve sequences of relations among the members, and then, it raises the question of, are the relations that took place in one coalition, does it get carried over into the next coalition? In other words, it tries in a way to take time and growth and so on explicitly into account, and that seems to be, in my thinking, it's still in a relatively early stage, but it's a little bit more complicated. Now, there's one place in game theory where this does become very explicit, and that's in the case of the prisoner's dilemma, where it changes very drastically if you go from the static prisoner's dilemma into the dynamic.

Now, in the prisoner's dilemma, the way it starts out, two guys have been arrested, and the police don't have evidence on them. So, they are being questioned separately, and they are each told that "if you rat on the other guy, you'll get a very light sentence." On the other hand, if neither one of them confesses, since there's no evidence, they'll go free. But, if they both confess, then there is evidence against them, and they're much worse off than they would be if the two of them had cooperated and kept silent. So that is the classical example of the prisoner's dilemma, and when Nash originally proposed his version of a non-cooperative game, [Albert] Tucker immediately proposed this prisoner's dilemma, and asked Nash, "Well how do you handle this?" And I don't think that Nash had an answer.

But then, one of the ways of dealing with it is to say, "Well, suppose that this is going to happen repeatedly, that these guys are professional criminals, and whatever they're doing, the cops get them, and every couple of weeks or so, they're in the cell and the same thing is happening." So then what you have to say: "Suppose that I confess," and here, there is a little problem. If each guy says, "Suppose I confess," then he has to say, "Well what's going to happen to me as a result if I confess?" You see, there is a sort of a dilemma here, because if I confess and the other guy sticks to the agreement, then the only one who could punish me is the cop, so this story doesn't really apply directly to the prisoner's dilemma, but there is another version of it where it does apply, and that is, suppose these two—we're not talking about two prisoners or anything, but we're talking about two firms, and they are colluding, and if they collude, then they each get a monopoly profit, and if they compete, they get the competitive return. And if one of them cheats on the collusion agreement but the other one sticks to it, then he will gain much more than he would get if he were to stick to the monopoly agreement.

So if they're going to play this thing repeatedly, the only punishment that would occur to this guy cannot occur, because it's the last time they're going to play the game, and so, the other guy isn't going to be able to do anything. What'll happen is, they'll simply revert back to the competitive equilibrium. So you know that on the very last play, what's going to happen is that if I cheat, it will be to my benefit, and then, the thing is going to be over, so it doesn't matter. I will not suffer any penalty as a result of cheating. Well, but if that's going to happen on the last play, so I know I'm going to cheat on the last play, why don't I cheat on the next-to-the-last play? So then through a process of what we call backward induction, you end up that, if I know for sure that we're only going to do this ten times, and so it's to my advantage to cheat the tenth time, as a matter of fact, it will be to my advantage to cheat right away, as soon as we reach the agreement on the first one. So that this is one way of, let us say, of getting around the Tucker's objection.

So how do we handle this? Well, one way to handle is to say, "It is not certain that the game will continue. We'll toss a coin, and if it comes up heads, we'll play one more time, and if it comes up tails, we'll stop." But that means that I

have to calculate the present value of the monopoly return that I will be giving up if I cheat, and I don't know when it would pay to cheat. It may be that the game will stop as a result of chance. So if it's very likely that the game will continue—in other words, if the probability of continuing is high enough—then, it's very likely that there will be collusion, and it won't break down. So this is a case where the dynamics of the game enters, time really does enter, and it gives a very different result than you would get in a static model.

07-01:10:56

Burnett: So in *Competition, Collusion and Game Theory* in 1972, you're thinking about trying to calculate or game when does it pay to collude rather than compete, so you're looking at the cost of collusion. And it's based on observation, so there's empirical research in here. You're talking about the observation that firms tend to cluster around a particular size of industry, or size of firm, and so I think that there's a really great effort to try to determine under what conditions firms will collude rather than compete. And there's different kinds of competition. There's competition from product variety, right? There's other things that you're exploring, and in certain exercises, you hold certain things constant, so static demand, for example. But in the end, I think, you conclude that there's no really good theory for determining the monopoly return for colluding firms, at least in this tentative exploration. But you're on a path now, and this is something you devote your career to, more or less, is exploring this set of themes using these sets of techniques—or using different techniques to explore these themes, let's put it that way.

07-01:12:41

Telser: Well, there's a very obvious case where this kind of issue comes up, and that is, when we used to have streetcars—so there were streetcar tracks in the main cities, and usually, there would only be one pair of tracks, so that meant that as far as the tracks are concerned, you could say they're sort of a monopoly, and if you wanted to have competition, nobody in his right mind would say, "Well, what we should do is have ten pairs of tracks and different companies running the streetcars up and down," and so that you can just take the cheapest one. The point here is that competition is not an end in itself, what we're presumably trying to do, or what the policy should be trying to do, is to figure out the best way to satisfy the demands, or the welfare, of the public, and that's pretty difficult. And you can't just say, "Well, we'll have antitrust laws and we'll have competition, and if anybody violates them, we'll haul them into court, and if it turns out they did, we're going to throw them into the slammer." It's more complicated than that.

07-01:14:34

Burnett: So this book, *Competition, Collusion and Game Theory*, is published in 1972, and you write in the introduction that a number of Chicago folks read it and gave comments. Can you talk about the reception of the book, both in the preparation of the book, by your Chicago colleagues, and afterwards, when it was published?

07-01:15:07

Telser: Well, I don't recall that among the senior faculty, that there was any particular interest. My impression is this: that they thought that here is a growing area of economics, and you should have it represented in an economics department that claims to be well rounded and on the edge of progress and so on. I didn't have the feeling that this was a part of the actual interest of people in the department. There were some who had some interest in it. Now, for example, to put it into definite context: Zvi Griliches, who was one of my closest friends, left Chicago in 1969 and went to Harvard. [Hiro] Uzawa was a mathematical economist who had come to Chicago from Stanford, but his interests were really not along these lines. I would say he was more interested in growth theory than he was in this sort of thing. The Industrial Organization group, which included, say, Peltzman—well, I think, did read my stuff, but he never actually talked about it with me.

Now, there were certain examples, there were dissertations, in which people used this. For example, one scholar used core theory in his study of shipping conferences, who was at the University of Illinois and presented a paper on shipping conferences to the Industrial Organization Workshop that Stigler attended.[11] And I should mention that a shipping conference was an arrangement of a group of shipping firms to provide regular services to certain ports in Southeast Asia, and they started in the latter part of the nineteenth century, and it went on for years, and it was commonly regarded as some kind of a collusive arrangement. Whereas, according to core theory, it created a good that was very useful for the planters who were growing tea and shipping it to England, and it was very important for them to have regular service so that the ships would come when the cargos were ready at regular times, and go out through the Suez Canal and bring the stuff to England.

And now, what would happen is that a tramp could show up, and if the tramp knew that a cargo was waiting, he could offer a very good deal to the shipper, who would be able to get it to England sooner at a lower cost. But, if that were allowed to continue, you would no longer get regular service, so that, although it would seem that this is some kind of a violation of competition, in fact, what it was doing is providing a service that was beneficial both to the tea growers as well as the tea consumers, even though to the naïve person, it might seem as a departure from competition. And there is a hidden claim that under competition, it would actually be cheaper. So the point is that here we had tools from economics that could study this stuff, and come up with sensible conclusions and test it empirically, so that the ones who really benefited—although Stigler did publish the article based on this research in the *Journal of Political Economy*, it's hard for me to say that he actually had any genuine sympathy for this kind of work.

---

[11] William Sjostrom, "Collusion in Ocean Shipping: A Test of Monopoly and Empty Core Models," *JPE* 97:5 (October 1989): 1160-79.

There's another example of this in another case, and that was on the producers of pipe, special: the Addyston Pipe case. And there was another student here, George Bittlingmayer, who did his dissertation on this using core theory, and that was one of the early antitrust cases. And in that situation, the customers were cities soliciting sealed bids from the producers of this special kind of pipe that they would use in the water system of the cities, in carrying wastewater, fresh water, et cetera, so they wanted to have a sealed-bid auction, to discover who would be the lowest-cost producer. Now the problem in producing the pipe is that there's a very substantial fixed cost, whereas the cost of the pipe itself, the variable cost, is not very high. So if there's some guy who is in trouble and has lost some business, he might have an incentive to put in a very low bid and get the business, but if this continued, what would happen is, you would destroy the industry. You would no longer get a group of firms willing to continue in the business of supplying water pipes to mostly municipalities on reasonable terms. So what the firms did is, they had a private auction before the public auction, and they figured out who would be the low-cost producer in getting the business. But, the other guys in the group would offer higher bids so the one they had picked would get the business, and what he would do is share some of the revenue to cover the fixed cost of the other firms in the business.

So this is an ostensible violation of the model of a competitive industry in the Addyston Pipe case, and the case actually went to the Supreme Court. I think that the Court of Appeals ruled that it violated the antitrust laws, whereas the Supreme Court reversed and they were more reasonable, and their decision made a lot more sense, and there are a number of cases like this, where what was happening in the real world would be challenged by the Justice Department using the antitrust laws. And by the way, in the Addyston Pipe case, I think the result was that the firms merged. So once they merged, and then the problem of some guy coming in and offering a low bid that would harm the others, that would not arise, so that there was a way by combining that would enable them to provide the services and cover both the fixed cost and the variable cost. So they merged.

And then, one of the effects of that, by the way, was the Clayton Antitrust Act, which put obstacles in the way of mergers. And Theodore Roosevelt, by the way, was quite incensed by the decision that the Supreme Court made in the Addyston Pipe case, and I think he made a very nasty remark about one of the justices or something, that "he had as much backbone as a chocolate éclair," so that we have had presidents who made impulsive and regrettable remarks that they shouldn't have made before the present one. Anyway, so obstacles were put in the way of mergers, and it became harder for firms to justify mergers on the basis of efficiency. There was a tendency for the law to assume that the main purpose of a merger is to get a monopoly return.

07-01:26:12
Burnett: So this dovetails with some of the research in the history profession that was coming out in the 1970s, for example, Alfred Chandler's *Visible Hand*, which is about the rise of the vertically integrated enterprise. And the basic take-home of that is that it's not naked profit that motivates these kinds of transformations, or the desire for naked monopoly profit; it's a desire to smooth the functions of the operation and to make things more predictable.

07-01:26:45
Telser: And to understand the technology that's really involved. I'm very happy that you mentioned Alfred Chandler. He was one of my fans and I had forgotten to mention him before, but he did understand this stuff. Chandler was at the Harvard Business School. And as a matter of fact, they even hired one of my students. Let's see. There are two Kevin Murphys. One Kevin Murphy is here; the other Kevin Murphy, who was my student, was then hired by the Harvard Business School.

07-01:27:28
Burnett: This is just to say that this is in the air, that people are reexamining some of our popular and legally based assumptions about the profit-seeking motivations of corporations, for example, and you continue that work into the 1970s. So I want to pivot a little bit to talk about this work with William Best, John Egan, and Harlow Higinbotham.

07-01:28:05
Burnett: So, this is some consulting work that was done in the 1970s for the A.T. Kearney consulting firm. Can you talk a little bit about that work, and how that led then to some research with Harlow Higinbotham and others about the theory of supply for the ethical pharmaceutical industry, and then later, with Higinbotham alone, "Organized Futures Markets: Costs and Benefits?"

07-01:28:33
Telser: Well first of all, the consulting firm that was working for the Pharmaceutical Manufacturers Association was A.T. Kearney, and Higinbotham and John Egan were former students of mine. [William] Best and [John W.] Egan were consultants at Kearney. And the problem was to try and understand how competition works in the pharmaceutical industry. So what we were looking at were therapeutic categories, and a therapeutic category describes a set of pharmaceuticals designed to treat certain problems. So there're drugs for high blood pressure, and this and that. But one of the most important aspects of this is the source of data. We got very good data, because the PMA [Pharmaceutical Manufacturers Association] collected data from the customers of these pharmaceutical companies, so that we knew what they bought, and how much they paid, and we could relate this to sort of the conventional measures of competition. And we also had this divided into two categories: the drugs that were bought by hospitals, and the same drugs that were bought by physicians in private practice.

And one of the interesting results, which really, at least to me, wasn't anticipated, is that the physicians in private practice were more innovative and willing to try new drugs than the boards that were typically in charge at hospitals. That is, the way this thing works is that there's a list of drugs in a category that is supposed to be used by the hospital, so if you're a pharmaceutical manufacturer and your drug is not on that list, it will not be used by that hospital, whereas it might be used by a physician in private practice. And the model that we used was rather different. Instead of doing it and sort of writing the model in the standard terms of monopolistic competition—price quantity, cost, and so on—the basic model said, "What would be the output and prices that would maximize the net benefit?"

So, in other words, it is based on a hypothesis you might call it a Coasean hypothesis: that the industry responds in such a way as to provide the drugs at the least total cost. It is not a situation where the normal supply-and-demand process applies. It does to some extent apply, but it's very difficult, because the basic demand for the product is not coming from somebody who is buying this because they like chocolate ice cream or anything like that. These things are prescribed by physicians either in a hospital, or in a private practice, for patients who need it. So that, if there's anything going on here that is relevant for welfare, it is to meet this criterion that you satisfy the needs at the least total cost. And we are not disputing the needs; we are not trying to tell the doctors, "No, you shouldn't use this; you should use that." So, it was sort of an unusual application of economics to an area where you really had to use different criteria than the standard ones.

07-01:34:10
Burnett: "It was an essay in positive economics," is what you wrote in the introduction, that there's no sort of prescription involved, no pun intended, but no advice to be given to the industry as a consequence. You were studying this problem as a problem.

07-01:34:25
Telser: That's right.

07-01:34:26
Burnett: And you apply multiple regressions to the data, and what you find is that, if you hold the number of firms constant, there's more entry into markets where there are larger firms; holding market size constant, there's more entry in markets with a small number of firms initially; and that entry increases with advertising. So these are some of the conclusions that you come to. Are there consequences to that, or it's an exercise that you—I think the pharmaceutical industry has been the subject, and is today the subject, a lot of criticism about what is perceived to be a kind of, certainly, an oligopolistic position in society.

07-01:35:25

Telser: Well, see, one of the underlying factors here that we did not go into is, there's a sort of a symbiotic relation between the firms, the pharmaceutical firms and the sources of innovation and research. University research, it's very complicated because universities do, some of them, do research on pharmaceuticals, and I think there's some research that is done in some government agencies. But one of the main things where it's very expensive is that the drugs have to be tested, and the testing of the drugs is usually almost always done by the pharmaceutical firms, and you really can't separate the innovation and the changes in the drugs and what they're doing with the results that they obtain from these tests. Tests are very expensive.

And so then you get involved in questions of, what should the prices be taking into account, the costs of these tests, also reckoning on the fact that a lot of these drugs were not the result of innovations in the drug companies themselves, but were the result of research, let's say, in a university that might have been financed by the drug company. And then you get very complicated relations. You really can't look at the pharmaceutical industry, let us say, the same way that you can look at the auto industry. And one of the things that I ended up later in doing was, here I was, in this particular study, where I was using data supplied by the PMA, and the cost of the study was done by Kearney. But then, there was another study—I don't think it was actually published—I'm pretty sure it was not published, and this was very, very touchy—but also in a pharmaceutical industry, and the sponsor of that study was Merck.

Now, the pharmaceutical companies were being attacked on the grounds that their profits were too high. That's an old story. So what Merck wanted me to do was to show that they're very risky, and because they're very risky, the higher profit is a return for the higher risk that they incur. So the way I did it is, I compared the pharmaceutical industry with the transistor industry, which was in a relatively early stage at that point. Now there are two important factors here that I haven't mentioned so far. One is that entry into the pharmaceutical industry is controlled, because nobody can buy or use your drug unless it is permitted by the FDA. So entry is controlled, and that is a much more effective control over entry than just patents. In the transistor industry, although you can get patents, that is not an important factor at all. And the turnover in the transistor industry is very high, and in fact, I had a student who studied this, that if you just take, let us say, a thousand transistor companies that started at some particular point in time, in ten years, 995 of them would be bankrupt, and there might be five of them, or so, that would be very successful—Intel, and maybe a couple of others—and they make a huge amount of money.

Well, so what I concluded in my study was, that actually, the transistor industry is much riskier than the pharmaceutical industry, and in general, not as profitable as the pharmaceutical industry. And why is the pharmaceutical

industry more profitable? Because they can control the entry of new drugs. They can have a direct control of the extent of competition in the industry. Now, you don't have to get involved in let's say, whether you like or you dislike, it's good or bad—it is a fact—but there was an interesting effect of this, and that is that Merck got sore, and they refused to support my research. And this became a big problem for the [University of Chicago] medical school, because Merck was the major supplier of funds to the medical school. And so, it upset some of the administration at the university, and it had certain consequences.

Now one of the consequences was that, when you do these sorts of things in a fourth quarter, you would get two ninths of your salary. So, Merck, that was going to give me two ninths of my salary, wouldn't. And then, I have to say, George Stigler came to the rescue, and from the Walgreen Foundation, I was rescued. And I don't think, I have to say, I don't think that the administration, the upper level of the administration of the University of Chicago did not come off very well in this situation.

07-01:42:53
Burnett: Did they put pressure on you, when you came to the conclusions of your research, or was it too late by then?

07-01:42:59
Telser: No, they did not actually try to affect the research, but what they did try to do is to cut off the funding, which is, of course, a potent form of pressure.

07-01:43:18
Burnett: So, the way it would work is that a sponsoring research organization would sponsor your salary, so they would pay your salary. The university would therefore not have to pay that portion of your salary for that quarter. But if they withdraw the funding, the university—

07-01:43:37
Telser: Because they didn't like what I was doing.

07-01:43:39
Burnett: Right, the university would then have to come in and top up your salary, because they're obligated to, are they not? Or would they just say, "It's too late. We don't have any budget for this. You're just going to have to go without your salary for that time"?

07-01:43:53
Telser: Well, actually, they didn't give any excuse. They just said, "Well, if they're not supporting you, that's too bad." Stigler did this on his own initiative. It had nothing to do with the university administration.

07-01:44:08
Burnett: Wow. That's very interesting. Absolutely fascinating.

07-01:44:14
Telser: One of the results, I guess I could mention, one of the results was that the provost of this university who was on my side left, and he became the president of Stanford, Gerhard Casper. And the general counsel met with me. It was a sort of a messy thing, and I don't think that they came off too well. I shouldn't say, "I don't think." They did not come off too well.

07-01:44:59
Burnett: Now, is that unique in your time at Chicago? Because I think Stigler was an early proponent of a model for the Economics Department to be entrepreneurial, to go out and get funds to support research. Ted Schultz had done it in the era of NSF funding, and he would bring in foundation funding, and Stigler sought funds from corporate sources, from foundations, wherever you can find it. So perhaps he felt some kind of responsibility in the sense that he had encouraged people to go out, and if something goes south, he was there to sort of catch people if there was a problem, that he felt there was undue pressure.

07-01:45:42
Telser: Yeah, that's possible. I don't think we actually explicitly discussed the issues.

07-01:45:52
Burnett: There was this spirit, I think, from Schultz on down, this idea of independence, that it really mattered. He had that ethos of, "you cannot put pressure on scientists to change the results of their research." That is really forbidden in his mind, and he was willing to resign from Iowa State on the basis of that, and that brought D. Gale Johnson here, and it brought Ted Schultz there, and a number of other scholars, through the University of Chicago.

07-01:46:27
Telser: But of course, at the time of this, neither one of them was active. In fact, I think Ted, I'm not sure if he—

07-01:46:41
Burnett: He had retired.

07-01:46:41
Telser: They were not involved in this in any way. The only one of the old timers who was involved was George.

07-01:46:49
Burnett: Yeah. Perhaps he retained some of that ethos.

07-01:46:53
Telser: Yeah. Of course, he had been at Iowa State, way back, I think. I'm pretty sure he was, yeah.

07-01:47:03
Burnett: And so, to your knowledge, there was never any other kind of friction with sponsors of research, with other scholars? Not with you, but with other scholars who were there, was there any talk around the water cooler?

07-01:47:19
Telser: Well, I can't think of anything. I really don't know. I don't know of any comparable incidences, but that doesn't mean there weren't any. It's just that I don't know of any. I only know about this because I was directly involved, and my guess is that probably, I doubt if there is anybody now in the Economics Department that knew anything about this.

07-01:47:51
Burnett: Was there any fallout for you professionally, in terms of getting funding? I guess you didn't do much more research on pharmaceutical companies after that.

07-01:48:05
Telser: Yeah, well, that was the main—[laughter]—that was the main effect, that I decided, yeah, well, it certainly left me with a rather hostile attitude towards the pharmaceutical industry, and in some meetings, general meetings with other colleagues that I attended where there were people from other universities who were friendlier to the pharmaceutical industry, I would say I challenged them, without going into any details. I would say, in this regard, it is my impression that there were people who did research on the pharmaceutical industries that gave results that I think the industry liked. Let's put it that way.

07-01:49:12
Burnett: At Chicago?

07-01:49:13
Telser: No, not at Chicago, no, elsewhere.

07-01:49:16
Burnett: Elsewhere, yeah.

07-01:49:18
Telser: Definitely not here.

07-01:49:19
Burnett: Right. Well, let's pause for now and we'll continue next time, because there's a lot more to talk about.

07-01:49:27
Telser: Yeah.

Interview 8: October 18, 2017

08-00:00:15

Burnett: This is Paul Burnett interviewing Doctor Lester Telser for the Economist Life Stories Project, and it is October 18, 2017, and this is our eighth session! And we're here in Hyde Park, Chicago, and we were last situated in the 1970s, and we were talking about a number of collaborations with former students of yours, and we had talked about the project on the pharmaceutical industry with A.T. Kearney consulting firm. But there was another project that resulted in a paper that came out in 1977, "Organized Futures Markets: Costs and Benefits." Now this returns you to an area of expertise that you've had long experience with, and it's also a collaboration with Harlow Higinbotham. Can you talk a little bit about the genesis for that project, and why you wanted to return to that area of research?

08-00:01:43

Telser: Well, first of all, in my PhD orals, Ted Schultz asked me a question, and that is, "Why are some commodities traded on an organized futures market, and what seem to be similar commodities not traded?" And I did not have an answer, and I guess this was in 1956, so, I had been thinking about it for a very long time. But one of the things that happened starting in the late sixties and seventies, a huge increase in the number of organized—well, not so much in the number of organized markets, but in the number of commodities traded on these markets, commodities that had never been traded on these markets before. And so this is what led us to try and see if we could figure out, how can we explain which commodities would be traded, and which would not be traded? And this led us into a study of the costs and benefits of having commodities traded on an organized exchange. And so that's really how it started then, from a question that I was asked, I guess something like twenty years earlier.

08-00:03:27

Burnett: And I think we talked about, in an earlier session, we talked about why T. W. Schultz was interested in that, and he and D. Gale were both interested in what they called forward prices, and the prospect of designing markets around that, and nothing much came of that, and I always wondered why that research was dropped. Was it because their focus on agricultural subsidies disappeared, or—well, not disappeared, but evolved into something else?

08-00:04:04

Telser: Well, now, I think that it was Gale Johnson's dissertation that resulted in a book, *Forward Prices in Agriculture*, and their primary interest was to try and stabilize the agriculture sector. There was a belief, especially in the 1930s, that agriculture was a less stable component of the whole economy, and so they were trying to figure out why is that, and what changes in policy could help stabilize agriculture. Now one of the reasons given was that the supply of agricultural products is very inelastic, so changes in demand would cause very big changes in prices. And so they thought about what could be done to

stabilize prices, well, and of course, one form of it is associated with the price support program, and that is that the government would step in and buy commodities when their prices are low, and sell them when prices are high, so that the price support program was supposed to operated intermittently as a stabilizer of agricultural prices. And in fact, one of the first articles that I wrote—I think it was in around 1956 or seven—was based on a criticism of a study made by somebody else, who claimed that the price support program did not, in fact, stabilize prices, and my criticism was that he didn't measure it correctly, and the reason was that all he did was to look at prices. He didn't take into account the relation between the government holdings of the commodities, and the effect on prices. And in fact, if you did the study correctly, which I think is what I did, you found that the program did stabilize prices.

In fact, earlier, when Herbert Hoover was president between 1929 and 1933, in his administration, they started a program to stabilize prices by having the government buy and sell in the futures market, so that the idea really started around 1930, and it continued. And I think probably, the main reason that some interest was lost in that particular aspect—well, there's several reasons, but one of the reasons was that in the 1950s and afterwards, the production of agricultural farming seemed to be more prosperous, and there didn't seem to be the same need to stabilize things when, overall, things were looking pretty good. At least that's my impression.

08-00:08:06

Burnett: I think that's right, and I think that the other thing is that there started to be perverse accumulations in the stocks held by the government, and it was having all kinds of global effects, and D. Gale Johnson then wrote about that in 1973. He wrote *World Agriculture in Disarray*, which is about how especially developed countries had these elaborate subsidies programs, and that they created all kinds of distortions. So they went from thinking about how to plan the agricultural economy to figure out how they could have less intervention in it, and that was a historical process that they observed, I think, over time.

08-00:08:50

Telser: One of the problems here is that when you start having the government involved in buying and selling and sort of stabilizing the price, they may stabilize the price, but the question is, at what level should they try and aim for? And if the price level that you aim for is either too high or too low, then there's a distortion, and a loss of welfare, you could say, in the economy as a whole. So I think that, when you're talking about stability, you're sort of talking about the second-order effects. But when you're talking about the level, it's the first-order effects, and they may be very serious. And I think especially in some of these foreign countries where they, well, for whatever reason—I don't want to get into—

08-00:09:51
Burnett: Yeah, that's a whole other twenty hours of conversation.

08-00:09:54
Telser: Yeah. [laughter]

08-00:09:57
Burnett: But, to bring it back to this paper, you write about other conventional economic shibboleths about futures markets and what they do and what they're for, and you write about a risk model for futures, for organized markets, and so you wanted to adjust that a little bit. Can you talk about what that was, where it came from, and what you were trying to do with your research?

08-00:10:31
Telser: Well, in the standard—I think I discussed this to some extent before—in the standard model, which was attributed to Hicks and to Keynes, it was argued that the hedgers are people who are in the industry, and they want to try and reduce the risk of holding inventories, and the speculators buy futures contracts from these legitimate traders, so that this is a form of insurance. Now, it assumes, for example, that the risk is involved in holding stocks, and it ignores the problem, on the other side of the market, that there may be a risk to those who are using the stocks, and have to satisfy future demands in their products, for example, somebody who is producing flour, or a baker, or people like that. They are actually making more or less long-term commitments to their customers, so that, if there's an unexpected rise in the price of their raw material, they incur a loss. The point is that there is risk on both sides of the market, and there's no reason to believe that one kind of risk is more important than another kind of risk. And in the case of many of these agricultural commodities, however, because the production is over a natural period—you plant the stuff, it grows, you harvest it, and then you don't start planting again until almost a year later, and in between the harvest and the new crop, there's a pretty long interval when people actually are holding inventories, so that there is a demand for hedging.

Then, another factor enters into this. If you think of speculators as selling insurance to the hedgers, then that's good. But once you start having a futures market with speculators, and people start to think about speculators are bad, they will start introducing variations in the prices of these commodities that destabilize prices because of whatever it is that they're doing that is uninformed. So one of the issues that came up, that, when you're studying futures markets is whether it is in fact true that it is the hedgers who lose on the futures market, and the speculators who profit on the futures market, or whether something else is going on that is distorting this relation, because you have sort of amateur speculators. Now when you look at the actual numbers, it seems that the ones who make money are the hedgers, not the speculators. And there were some government studies of this that I looked at, and actually Houthakker also looked at it, that would show this, and Houthakker's

argument was, "Well, the reason that the speculators lost money is because we are getting data from brokerage firms that failed, and so they didn't know what they were doing." That was a position that I could not take seriously, or I did not take seriously, and this actually led to some considerable disagreement between Houthakker and me.

Now there are other people who are interested in futures trading, notably, Holbrook Working, who was at Stanford, and what he did, and some of my work is related to this, is that what you really should look at is what is called a spread, the difference between the spot price and the futures price. And it is true that the spread is much less variable, because the two prices are correlated, and the difference tends to be stable, so that instead of thinking about this as hedgers versus speculators, you should think about this as people who are trading on where they think the spread is going, up or down. Now in the standard futures argument, it is thought that you have what is called normal backwardation. Normal backwardation means that the spot price is above the futures price, and a contango, the other term that's used, means the futures price is above the spot price. Now, if an inventory is being held so that a cost of storage is being incurred, you would think that what should prevail in the market is a contango, where the futures price is above the spot price by the marginal cost of storage. And that, in agricultural commodities, is not what we see. We see normal backwardation, and this creates some kind of a problem for the people who propose this stuff, because then, what they have to say is, "Well, there must be a trend in the spread." And as soon as you start talking about trends in these prices, it raises a problem, because if there are trends, then you don't have to be very smart to take advantage of it.

So one of the things that I did is, I started to investigate to see whether, in fact, there are trends, and it turns out that, no, there aren't trends, really. Now whatever you see where prices tend to move together is usually a result of common forces in the economy, so that the economy is either going up, or going down. So Harlow and I, Harlow Higinbotham and I, focused on a lot of these factors that affect the spread and the function of markets. Now one of the main things that happens in a market is that you have one place where the prices become visible, so people, to the extent that prices convey information, an organized market serves a very important purpose. It has nothing to do with hedging or any of that. The purpose it serves is it conveys price information, so that some of the factors that enter into the benefit of futures trading are related to the benefits of having, or lowering the cost of finding prices, which is sort of related to the search costs that were prominent in Stigler's work on search.

08-00:19:07

Burnett: So that's what I took from the paper, and that there is, there is a cost of recordkeeping, so you're mentioning these things—the elaboration of rules to prevent cheating, there's expertise that's involved—and all of this has a cost. So one of the things that I found interesting, coming out of the work of

Working and Houthakker, is that you're emphasizing purchasing power, right? You're saying that it's not about hedging risks or speculating; it's that these futures operate as a kind of money. Can you talk about how that works, or how that operates?

08-00:20:02

Telser: Well, when you have an organized market and a clearinghouse, a trade occurs between two traders, but it is recorded in the clearinghouse, and in fact, it is the clearinghouse that guarantees the integrity of the trade, in other words, that the terms will be carried out. Well, in that case, it is equivalent to having a money supply that is guaranteed by the government, or by a central bank, so that the traders don't have to worry about whether the person that they're trading with is going to carry out the terms of the contract or not. The clearinghouse is going to do it, and the clearinghouse carries, on its books, assets and liabilities. The assets are the sales commitments. It's sort of what the sellers have deposited, in effect, with the buyer, and the liabilities are what the buyers have acquired as a liability from the clearinghouse, and these things must be consummated by the clearinghouse.

And then the other point that's important is that the only ones who trade in a market are members of the organized exchange, so that they are not strangers. The trades are guaranteed by the clearinghouse like a bank, and all they have to worry about is the prices at which they transact, and agree on, and, the quantity that they're trading. So that a futures contract that is created is very similar to money, and there's a very close analogy between money in the economy and futures contracts in these sorts of situations.

08-00:22:38

Burnett: Right, it facilitates trade, and therefore, it expands trade. You're increasing the number of participants, and you're also increasing the turnover of participants, so there is this kind of economic churn that adds value to the market, that's as a result of its being organized.

08-00:23:01

Telser: Right. The cost of trading with an organized market, and the integrity of the trading is, the integrity is increased and the cost is decreased, and the market is very liquid, and if you tell the broker to carry out a transaction at the market, without even knowing exactly what the price is, you can be pretty confident of what price the transaction occurred at.

08-00:23:47

Burnett: Did this body of work, that really begins your career, did it give you an appreciation for the importance of rules and structuring of markets?

08-00:24:00

Telser: Oh, absolutely. I learned all kinds of—well, for instance—the general judges, and the legal authorities, would have a fairly jaundiced view of futures trading. So, the major organized futures market started in New York, I think

around the 1840s or so, and cases were brought to ban futures trading unless there was a provision for delivery. So that was inserted into the futures contract. Now there's one other thing that the courts ruled, and that is that there could not be options. So you could have futures contracts, but options were illegal. So what actually happened in the futures markets is, there was options trading, but it didn't take place on the floor of the exchange. The options traders used to trade in the halls, and make these informal contracts. They made contracts in options with each other in the halls, but these were not recorded in the official books of the clearinghouse, so that even though it was not legal to trade options, they were in fact traded. So that's an example that you can have rules, but if it is really in the interest of the traders to do something that the rules prohibit, they'll figure out ways of doing it.

So in that case, it's okay, but then there were other rules. One of the rules which is still in existence is that when you call and put in an order to your broker, he is obligated to execute the order before his own. Otherwise, what could happen is, since he is in closer touch with the market than you are, he might trade with you and know that he can then trade in a market and make a profit so that you will end up, if you're a buyer, paying a higher price than you otherwise would, or if you're sure getting a lower price—selling at a lower price than you could otherwise have done. So there are all kinds of rules that they had that they developed because the traders would figure out ways of doing things.

08-00:26:50
Burnett: A form of insider trading, I suppose.

08-00:26:52
Telser: Sort of, yeah. It is a form of insider trading that was prohibited.

08-00:26:59
Burnett: And there's a professionalization of the traders, too, such that they, like the American Medical Association, maybe the analogy can be stretched too far, but that they are setting professional norms, and punishing people for transgressing those norms.

08-00:27:17
Telser: Yeah, the big punishment for a trader on the organized market is that he gets thrown off the exchange, which means that he can't stay in the business of being a trader. Now there's another factor that's important here, and that is, how do people who are not members of the exchange—in other words, how do outsiders trade on the exchange? They do it using the brokers, so there are relations, so the exchange has a price limit rule, which means that if the price goes either too high or too low relative to the open, trading stops. And there were people—in fact, somebody was brought to me because they wanted me to explain, why do you have this limit-price rule? It seems to interfere with the working of a free market. Well, the purpose of it is to give the broker time to contact a trader to change his position. And if you did not have outside

traders, you wouldn't need this rule; but if you do have outside traders, then those who are trading via a broker outside the market need to have some guarantee that they can react to unforeseen changes in the price. So that having this rule makes it cheaper for an outsider, or even possible for an outsider, to trade in the market. So there are lots of rules like this that were developed in the course of time.

08-00:29:20
Burnett: Someone reading this article in 1977 might've had in mind the Great Grain Robbery in the early seventies. So the Soviet Union purchased, on spot markets, they purchased over the course of six months an enormous amount of grain. The Great Grain Robbery, it's called. Others call it the Russian Wheat Deal—

08-00:29:41
Telser: I hadn't heard of that.

08-00:29:43
Burnett: The Russians cornered the market, basically, because they were short millions of tons of grain, and they purchased on the spot markets and drove up the price so rapidly that Congress passed a law saying that the Congress needs to be informed if grain purchases reach a certain threshold of— [laughs]

08-00:30:09
Telser: Now, wait a minute. This is in reality?

08-00:30:12
Burnett: Yeah! This is in the—

08-00:30:13
Telser: Oh, I hadn't heard about this.

08-00:30:14
Burnett: —in the early seventies or so, and so there were all kinds of recriminations about the USDA's ability to look at world grain markets and what was going on. People were blindsided by this, and so, it's another example of the elaboration of rules to manage highly volatile—and as you said, that the price inelasticity of demand for agricultural products is tremendously high and volatile, so that's in the mix. And so, the need to structure markets, of certain kinds, under certain conditions, was made apparent to you very early in your career, and it makes me think about your careful thought about the theory of the core, and how markets differ, and how organizations work out their own rules, even tacit rules, for cooperation and competition.

08-00:31:18
Telser: Well now, I also wrote an article about corners in markets. A corner is a very dramatic event, and so, and the people who write about it—I think there's a famous American novelist who wrote; I don't remember his name offhand. Anyway, but if you look at the economics of it, it is not as easy to carry out a corner as many people seem to think, because if you are acquiring a large

stock secretly in order to drive up the price, you then face the problem of getting rid of it later, and it is not clear that cornering commodities ends up as a profitable activity, or even that corners, in fact, occur. Now when a government does this sort of thing, they're not motivated by a profit motive the way a private firm would be, so they might be doing things, for whatever reason, that would upset the working of a normal market.

08-00:32:56

Burnett: Well, the Soviet Union needed every bushel in that case, to eat, [laughs] so they didn't need to sell it on later. But I wanted to ask you, in this period too—we've talked a little bit about this, but the importance of the consulting work, and what you learned from it. There are a couple of other projects, and I'm not sure of the chronology. There was some benefit-cost analysis for the Environmental Protection Agency?

08-00:33:30

Telser: Yeah, that was for Kearney. That was a consulting by A.T. Kearney. The particular project was about noise pollution. There were several. One was about noise pollution, and the EPA. Another project was the bankruptcy of the Penn Central, which was at that time, I think the largest private bankruptcy that occurred. Now when there's a bankruptcy, what happens is that the court appoints an official to carry out whatever is involved to straighten things up. But in this case, it was so big that the referee was an agent of the federal government, called USRA, United States Railroad Administration, and so Kearney was brought in as a consultant to figure out what the government should be doing in this kind of a situation. And for me, it was very interesting, because I learned a lot about the details of running a railroad, and in general, that's not something that you know about.

A very minor point I think I mentioned several times in talks, et cetera, is that there are only single railroad tracks between cities, so that, in order for the railroad system to operate without head-on collisions taking place all the time, you have sidings and an elaborate communication system, so that certain trains will go by and other trains will go off on the siding and then come back on the mainline. So there are all kinds of things, and then there were sorting yards and so on. Some of this stuff was also a topic—but USRA was separate—but when I was at the Cowles Commission, there was a major project sponsored by, I can't remember which defense agency, to figure out an efficient way to design the railroad system during times of war, and that was a project that was directed by Koopmans, Bart McGuire, Chris Winston, and Martin Beckmann. So the problems of the railroad industry came to me in a somewhat related form much later in this Kearney project.

08-00:36:41

Burnett: These are elaborate mathematical problems, are they not—

08-00:36:43

Telser: Yes.

08-00:36:44
Burnett: —this question of coordination with these constraint problems, right? And it's a combinatorial problem.

08-00:36:54
Telser: And firms with sorting trains, a train usually has a lot of freight cars going to different places, and it's not obvious how you're supposed to make up the train so it will deliver this stuff efficiently without running back and forth and making useless side trips. And this is the sort of thing, I have to say, that some economists who shall remain nameless tend to ignore and decide, well, that's not really economics. But it actually does have a very important effect on the efficiency of the economic system.

08-00:37:40
Burnett: Why would they say that it's not economics?

08-00:37:43
Telser: Ah, [laughter] I don't know.

08-00:37:45
Burnett: What's their definition of economics that would exclude that?

08-00:37:48
Telser: It's what they think is economics. [laughs]

08-00:37:51
Burnett: Okay, you're going to identify someone if you describe it any further, okay. [laughs]

08-00:37:56
Telser: Maybe. Yeah, I'll see.

08-00:37:58
Burnett: So, somewhere in here you found enough time to write another book. In 1978, you publish *Economic Theory and the Core*, with University of Chicago Press. And I guess one question is, because often, there's empirical work in the books that you publish, and I always want to think about the impact of the consulting work that you did, and the time spent with different kinds of business organizations that influenced this research—we have already discussed, with respect to this book, the story of the panel data from the MRCA, and the research on gas stations. So, one of the things that's interesting is you do address the economics of information, and there are a couple of places where I think it's a very direct commentary. So, early in the book you say, "Even if information is costless, different prices in different submarkets are compatible with allocations in the core." So, is this something that had been evolving since the '72 book, in the wake of conversations about the core? How did this evolve in the interim with respect to other important theories of economics?

08-00:39:47
Telser: Well, once you start setting up the inequalities that describe the core—and by the way, there's one point I should mention. I don't think I mentioned it before. There was a very important article written by a Russian, Olga Bondareva. [cf. Session Five for a discussion of her work] Without her work, I don't think it would've been possible to do almost a great deal of this stuff that I did later. But once you set up these inequalities, you then begin to play around as rigorously as you can with the effect of numbers of traders on the outcome. And so, in this particular case, if you get away from the notion that a trader, each guy trades only one thing, and you have more than one unit of the goods being traded, and so things have to kind of fit together—you have discrete bundles of commodities and so on—then, there is the technical question of, under what conditions will all of this stuff match up so you can get a uniform price? And the answer is not obvious, that once you begin to take into account indivisibilities, it has really nothing to do with search costs; it has to do primarily with what you might call the details of how a market functions. And so, it is no longer true that you can say, "Aha, you'll always get a uniform price in a market," and so on. If you take the mathematical model seriously, that, it does not follow.

08-00:42:10
Burnett: And if you take the empirical data seriously, because—

08-00:42:12
Telser: Yeah, right, [laughs] and of course, when you start looking at what you get in the real world, you will see a difference between the empirical observations and what some of these models say. It seems to me it is desirable to reduce the discrepancy between what the model says and what you observe. And so this was one of the ways that I tried to do it.

08-00:42:47
Burnett: And so there are efforts, well, basic price theory is that there is a price at which the market will clear, right? And I think you stated forcefully at one point, in an '87 book, that that isn't always true.

08-00:43:05
Telser: That's right. [laughter] Yeah.

08-00:43:08
Burnett: And so, it also makes me think about the reception of your ideas within the Chicago Economics Department, and if price theory is the gold standard, I don't know how that—I didn't take those courses—

08-00:43:23
Telser: There's the as-if thing, yeah. Well there's a proposition in logic: if the moon is made of green cheese, then the speed of light is 300,000 kilometers a second. Well that is actually a true proposition. If you ever had a course in symbolic logic, you will recognize it as a true proposition. Now, it is falsified—well, never mind. [laughter]

08-00:44:03
Burnett: [laughs] You know, it's almost—I hope this isn't too inaccurate—it's almost a mathematically sophisticated return to institutional economics.

08-00:44:31
Telser: Yes, I think that's correct. Yeah, you're looking at what underlies all of these phenomena. You're not looking at it as just a bunch of numbers; you're looking at what happens in order for things to take place. What's the background?

08-00:44:56
Burnett: Right. And so, in describing this, you're trying to find which coalitions can result in an empty core. The empty core is something you don't want, because the regular market transactions are not going to result in trade, or in optimal trade. And so you write, and this makes me think of law and economics, you write, "If these constraints are made illegal, the core constraints that remain can be satisfied." And so you can engineer a market.

08-00:45:45
Telser: And there are different ways of doing it. What this is talking about is, what rules, and how do you change the rules to get desirable results? Now for example, I did talk about an empty core in a legislature in voting, and I talked about logrolling. Now another example of an empty core is what I call the Treasure of the Sierra Madre, where it takes two people to dig up the treasure, but there's three of them, so then you get into a problem because they have to try and work out ways to deal with it.

Formally, if you look at the inequalities, they do not have a solution. An empty core really means that the inequalities that you use to describe what goes on in a market are not solvable. Well, one of the ways that you can solve the problem of the Treasure of the Sierra Madre is that the government passes a law and says that only a licensed person is allowed to be in charge of a gold mining operation, so that these three who are trying to get the gold out, can't do it unless one of them is one of those licensed by the government. And in that case, when you work out the core constraints, it turns out that this licensed individual would get all the profit, and the rest of them would just get zero, essentially, their opportunity cost. However, if you had two licensed rock movers instead of one, then you're back to the empty core.

So there are ways of dealing with the problem that work, and ways that don't work, and what this is saying is that in particular applications, you have to think very hard on how to make up the rules. You can't just wave your hands and say, "Markets will always work." That isn't true. Every market has a set of rules, and it may be or may not be conducive to the market working. It's not a question of telling these people how they should trade or anything, or what the price should be; it is a question of figuring out what the structure of the market should be so you will get the correct results. So for example, in the futures markets, one of the things that they did is, they said that you have to be

a member of an exchange. You have to have a property right in the exchange, or it's not going to work. By the way, if we can leap ahead, one of the big problems with derivative trading is, you don't have that. Most derivatives are not traded on an organized exchange, and the opportunities for skullduggery and crookedness are magnified as a result. And it would be very dangerous for a novice, or even people who think they're experts and really aren't, it would be very dangerous to get involved in one of these things.

08-00:49:45

Burnett: It seems like we have two poles: On one side you have folks saying, "Regulation is bad. Let the market forces be unleashed," and then, other groups who have seemed to have less of a say now, but certainly historically, who have suggested that there are pernicious effects of the unregulated market, and that we have identified the bad thing, like speculation, and so we'll eliminate speculation by making it illegal. And what you're suggesting is sophisticated research into how markets actually function, and then finding a theory that fits or matches with the observed behavior. Does it ever work the other way where you work from first principles, or you work from a logical, "let this be this," and then see if it fits? Or are you always working from existing market problems?

08-00:51:01

Telser: Well, that's a very good question. In some of the more recent work that I've done, it is an attempt to set up some formal axioms, and deduce the results, and then see how it might explain and teach us about actual situations. In a lot of the economics, the institutional details are ignored. So once you start in the framework of game theory, you try to take these into account in a formal way. And, in the *Theory of Games*, the way von Neumann and Morgenstern describe it, there's what they call the extensive form of the game, which is their way of describing a situation that really applies in a particular application, and then, to deduce the consequences of that in a formal way.

Well first of all, that's very hard to do, and it's my impression that that has not been done enough, that there is, for instance, in game theory, there are people who have tried to figure out, given the rules of poker, what would be the best way to play a poker game. Well, in order to do that, you really have to know what the rules say. Well, in an economic situation, you would have to do something similar if you are serious in trying to explain what's going on. Now, the difference, I think, is that the economy is usually much more complicated than anything like a poker game. So far, it's too much to hope that you can set up a formal model that'll always work. You hope that the model will pick up enough of the relevant stuff, so it'll give you useful results.

08-00:53:40

Burnett: In this book, *Economic Theory and the Core*, from 1978, I think you introduce some terminology, and I'm wondering if you could talk a little bit about it. The kind-characteristic functions, these are the functions which give

nonempty cores, and Viner Industries. Can you talk a little bit about these and why they're important to your research?

08-00:54:14

Telser: Well, Jacob Viner wrote a very important article about the equilibrium in a competitive industry. And, in the standard model, you have identical firms with U-shaped average cost curves, and Viner recognized, without really having the formal tools to do it, that this leads to trouble, because depending on the state of demand, there will be some firms that will have an incentive to cut prices to a very low level. And that will upset the equilibrium, so that what will happen in those industries is, you'll have firms that lose money and they go out of the industry, then the industry revives and it does better, and then because it's more profitable, other firms, new firms, will come in, and so you'll get into one of these cycles. And when you do the formal analysis using core theory, you confirm Viner's intuition.

So the reason I use that example is not necessarily because I could give you real industries where this happened, although I'm pretty sure there are, but it was to give an example that would be familiar to someone who had gone through, let us say, a standard set of courses as an economics major, and they could see, with relatively simple geometric diagrams, why this is happening. You could draw the curves and all of that, and you could really see what's going on. What you're doing in this case, in formal mathematical terms, is, you're solving a linear programming problem where you have indivisibilities. It's a sort of a combinatorial problem, and that's a hard problem.

One of the things that I do in this book that I hadn't done before is, instead of having discrete firms and so on, I used the ploy of making the individuals infinitesimally small. So these are what we call nonatomic games. Nonatomic means that you don't have sort of the clumps, that everything is continuous, and it's supposed to make things a little easier, because then the structure of the functions and so on satisfy certain standard mathematical conditions. So that simplifies things, and a kind-characteristic function, in terms of the cost curves and demand curves and so on, has a nonempty core. And an unkind characteristic function does not, and the space that you're looking at, where you're investigating the properties of the core, becomes a subset of a Euclidean space—it's sort of like, looking at a problem in geometry where you're drawing pictures of curves and triangles and so on, and the background that you're looking at, the space that you're looking at, is described by coordinates and by points in the space, but a point is infinitesimally small. It does not have what you might call a tangible existence. So this simplifies the problem, but it also introduces factors that are not present in reality. So it's a way of using a certain kind of mathematics that deal with these problems.

08-00:59:21

Burnett: Well, something that also is tackled in this book is, you're looking at a kind of theory of storage, and so supply and storage become important when you're

talking about the core. And you follow up with this in another book as well in the eighties. So, why is supply a problem with respect to the core? Maybe that's a way of tackling it.

08-01:00:01

Telser: Well, one way to think about this is that when you have a supply of something, it's already there, and the customers are going to come and buy pieces of it. And so you have incurred a cost to create the inventory, but the cost of selling off pieces of it may be considerably less than the cost of creating the stock. Well, in the most extreme form, you can think of it this way: You've created the airplane and it's got empty seats. So the airplane itself is like the stock of the commodity, and the empty seats are the pieces of the inventory that you're selling. Well the cost of selling one seat, if you've got empty seats, and the plane is going to take off, it's zero, so you run into the problem of, what will ensure the existence of these inventories, if the marginal cost of dealing with the demand that materializes is very low? You have a dichotomy between the problem of covering the fixed cost, and whether the revenue that you generate from the variable cost will be enough to cover the fixed cost. So you get into what we call freeloader problems.

A simple example of this: You go to a barber shop. The barber doesn't have a customer, so how much would it cost him to give you a haircut, since he isn't doing anything? And the only time you would have to actually pay a price would be, let's say, two people come to the barber, and there's an auction for the services of the barber. I mean, that would be a very cumbersome way to deal with the problem. Now there are lots of examples of this: a subscription versus buying a newspaper on the stand whenever you wanted. There're all kinds of situations where this comes up, and these are all situations that are sort of classical cases where you can get into empty core problems. In the early example that I gave in the Addyston Pipe case, you had that type of situation. Here are these firms, they've got the capacity to make the pipes, and somehow, there has to be some way of handling intermittent demands. So the facility is idle, but you cannot produce on demand to meet the needs of the customer. It's much more expensive to vary the inputs to meet the demand of the customer than to have it available ready to be met when the customer arrives, but then this creates the problem of, how do you generate enough revenue and so on to cover it?

08-01:04:10

Burnett: It's dealing with these lags, right? It's not that the demand sends a signal that then is responded to by the supplying company. It's more sophisticated than that and more complicated.

08-01:04:26

Telser: The supplier somehow has to anticipate the demand and stand ready, and from the point of view of the economy as a whole, it's cheaper to have the stuff available than it is to try and meet it on demand. And by the way, there is common law to deal with this problem. It's a very old problem. It's raining, so

more people show up at the inn. So what do they do at the inn? Are they allowed to raise the price of a room? Well, there's common law cases probably going back to the twelfth century or something that deals with this sort of thing.

08-01:05:13
Burnett: Price gouging, effectively.

08-01:05:15
Telser: Yeah, right.

08-01:05:17
Burnett: When we were talking earlier about ex-ante equilibrium, is that this challenge that you're speaking about?

08-01:05:28
Telser: Yeah, thinking ahead.

08-01:05:30
Burnett: Now, I don't know if you remember this, but in the examples where you're talking about how to formulate uncertainty with respect to a theory of storage, you look at sixteen different commodities, and they're the commodities that I think you'd find on organized futures markets: foods, metals, petroleum products, rubber, and I'm thinking of the seasonal variation. And you have sixteen pages of these correlograms, these waves of plot points over time, and they show—I'm not quite sure what they show, but they're effectively these plot points that form waves. Well, the midpoint of this correlogram is the zero point of correlation, and then there's one at the top and minus one at the bottom. And so some of the waves are more tightly clustered around the one, and others are bigger waves that go towards one or minus one. So I was fascinated by this, and so, that kind of representation, what kind of illustration was that, and how does that play into this problem with the theory of storage?

08-01:06:55
Telser: This is to illustrate the differences in the responses of suppliers to these variations. Some of the seasonal variation comes on the demand side. Some of the seasonal variation comes on the supply side, as is true for agricultural commodities. But then, aside from agriculture, it's usually cheaper to run production continuously if you can, then there's sort of a balance, a tradeoff between the rate of output and the inventories that you need in order to handle departures of the demand from what you expected. And there are various techniques that have been used: forward contracts, so that there are some people who make commitments to buy certain minimum quantities in advance; and then there are spot contracts and forward contracts. And sometimes, you will see that there does seem to appear to be what is called price gouging, but then there are other times when you have made a forward contract, the spot price is high, and the supplier is still selling you this stuff at a lower price, so that they have to sort of work this out, taking into account what is going to happen in the future, the near future, the distant future, and so

on. I don't remember the details, and you're quite right [laughs] to say I don't, but this tries to illustrate the diversity in how complicated things are, and that you shouldn't try to explain this stuff with just simpleminded, naïve theories.

08-01:09:14

Burnett: In the introduction, you mention that this book addresses more recent work—that's work that happened after your book, *Competition, Collusion and Game Theory*, work by Herbert Scarf in 1973, and Hildenbrand in 1974—and you said that this work—

08-01:09:40

Telser: Wait, who was the first one?

08-01:09:41

Burnett: Oh, Scarf, Herb Scarf, and Hildenbrand, and you said that although this work was important, it was too mathematical, and it had not yet been explained for economists. So this book addresses some of that mathematics, and puts it into a way that could be understood by economists. That was the goal.

08-01:10:10

Telser: I hope so. Now, the first economist that dealt with these problems is David Ricardo, and that was around I think, I don't know, 1805 or something. And what David Ricardo, when he wrote about production, he was usually thinking of agricultural production, and he introduced the notion of the extensive margin and the intensive margin. And the extensive margin means that if you're, let's say, growing wheat, it's not that you're going to be growing more wheat on your land, that isn't the way you respond. The way you respond is, you use more acres to grow wheat, so that's the extensive margin. The intensive margin is a situation in which you can cultivate intensively the given input. Now when you have firms operating and the supply response comes by changing the extensive margin, it's the type of situation that the mathematics of Hildenbrand and Aumann and Scarf are dealing with, measure theory. Measure theory is not intuitive, at least, until you've been exposed to it, and it stems from work that was done in mathematics at the end of the nineteenth century, by especially by Cantor.

Well, here's a simple example that you may be familiar with. The integers are the natural numbers: one, two, three, four, five, six. And then, rational numbers are numbers where the numerator is an integer and the denominator is an integer, so you get the ratio. So there's lots of rational numbers, and so what Cantor said is, "The rational numbers are countable." That is, there are as many rational numbers as there are natural numbers, which, when you think about it, isn't so surprising. Actually, Galileo was aware of this already. But then, there are other kinds of numbers that are what we call real numbers, and an example of a real number, which is not a natural or rational number, is the number π, for the circumference of a circle. And there are other numbers like this: e, the base of natural logarithms. Well, although, there are as many rational numbers as there are integers—that is, even though in any interval

from zero to one there's a hell of a lot of rational numbers—if you look at an infinite number of integers, you can set up a one-to-one correspondence. But with the transcendental numbers, it is not possible to set up a one-to-one correspondence between the integers and the other numbers. In other words, there are more transcendental numbers than there are rational numbers, or algebraic numbers.

So there are different orders of magnitude. So then when you start with measure theory, it turns out, if you're trying to figure out the area of a curve, and on one axis you have the numbers and then there's a *y* axis and an *x* axis, and there's a curve that's going around—I guess you can see, I'm trying to show you the curve.

08-01:15:19
Burnett: It's in the shot. [laughs]

08-01:15:20
Telser: Well, it turns out that if you try to measure the area under the curve, you'll get the right answer, even if you threw out all the rational numbers, which means, throwing out a countable infinity of the numbers would not affect the number that you would get that corresponds to the area under the curve. So that when you start recognizing this stuff with measure theory, you can throw out all the rational numbers. Well, in fact, in economics, or in business transactions, we're always using only rational numbers. The only time I can imagine you might be using more than rational numbers would be if you're getting a continuous flow of electricity or something, so that you have a very refined meter that can calculate exactly how much current you got, let's say, to a million places or something. So then, you probably would be using transcendental numbers, but ordinarily, that doesn't happen. So with measure theory, what you end up doing is throwing out all the numbers that are actually relevant in economics. I mean, you can do that, but I think it could be misleading.

08-01:17:04
Burnett: Right. And so, speaking of information, I think some of your analysis here in the last couple of chapters, and in chapters seven and eight, you are talking about what the members of coalitions know, and what they need to know, in order to make the decisions which are consequential for their remaining in a coalition. And so, you write that "members of coalitions do not need to know each other's functions to enforce core constraints."

08-01:17:47
Telser: It's a little more abstract there, than my current position. I think I've gotten, let us say, wiser than I was back then. I was more accepting of the standard let's-sweep-it-under-the-rug approach than I am now. It is, you could say, in principle, they have to know something about each other, but it doesn't really say what it is that they actually have to know. It depends on the application

that you have in mind. And so I would say I'm not too happy with that statement, let me put it that way.

08-01:18:52
Burnett: Well, you do say that "the core requires knowledge of which offers will be rejected not which ones will be accepted."

08-01:19:00
Telser: That's right.

08-01:19:01
Burnett: So there's a difference there that is significant.

08-01:19:07
Telser: Well, let me put it to you this way. You could think of it this way, operationally: Here's all the people in the coalition, and they get an offer, and then they have a vote, and it is in the nature of the core that if somebody objects, the offer is discarded. The only time the offer is accepted is if nobody objects. What the formal theory really says is that you have to have unanimous consent. And so, you know what you don't want, but that does not necessarily mean that you sit there and you try to figure out from everybody what they would like to have, because the attitude that you take is that, we only have to worry about something when an offer comes in that would have an effect on us, and if it doesn't, if whatever doesn't affect us, we don't have to worry about. So you have to be willing to say, "There are some things that don't affect us, so we don't worry about them."

And this is also one of the problems in preference theory. In some of the theories, they say you have to be able to have a preference between any two objects, and there are some criticisms of this, and this is in the spirit of the same thing. We don't have to worry about all the things that might happen; we only have to worry about the things that do happen, or the current alternative. That's one way of thinking about it.

08-01:21:07
Burnett: Right, what's consequential. So, you do acknowledge that the cost of information can lead to price differences, but you write that "it's foolish to think that all of the observed price differential is due to the ignorance resulting from the cost of information." So you want to get a broader portrait of decision making—so, let me ask you this then. Were people fetishizing the cost of information? Had it become too excessive at that point? It's an important theory, and at this point, was it suffering from its own success, in other words?

08-01:22:01
Telser: Yes. There's another example of that: cost of information and transaction costs. So whenever somebody would point out a discrepancy or whatever, then a very common kind of response is, "Well, it's because of transaction costs or information costs." So it can be used as a copout.

08-01:22:30
Burnett: And so core theory is addressing some of this, perhaps an over-reliance on—so, are some Chicago theories, that were really important to the reputation of Chicago—and the reputation of Chicago was about addressing shibboleths, was about addressing the conventional wisdom. Was there a danger at certain points that Chicago tenets were becoming their own shibboleths, that people were becoming too—I guess you already answered that question. [laughs]

08-01:23:06
Telser: Yeah, I think the answer is yes, and we can see some of that now. For example, a careful reading of Adam Smith by a serious member of the Tea Party, I think would be a revelation to them.

08-01:23:25
Burnett: Well, we should keep that for our last session, for talking about all of these changes. And so we are getting into the 1980s, and you write a number of articles, some of which were published and some were not. I do want to ask you about this conference at the Hoover Institution in 1985, out of which comes this paper for the *Journal of Law and Economics*, "Cooperation, Competition, and Efficiency." Do you remember that conference? What was going on there? What was the purpose of it?

08-01:24:11
Telser: This is the one that was at Hoover, I think?

08-01:24:16
Burnett: It was at Hoover in 1985, and yeah, and you write this.

08-01:24:25
Telser: Well, let's see. I think that was a sort of a broader audience. It wasn't just economists; there were some lawyers present. So it's more of an expository thing to try and explain this to a broader audience, and I think, as I recall, it was sort of a summary, wasn't it, of—

08-01:24:53
Burnett: Yeah, and you put in clear language, I think, what had been addressed in much more sophisticated and elaborate terms in your earlier work. You wrote about the Sherman Antitrust Act, and what you see as an unprecedented move by the United States to preserve competition by making certain kinds of cooperation illegal, and that's obviously the antitrust act, but also the Federal Trade Commission and so on. And yet, the United States promotes many uneconomic policies, for example, with terrorists and so on. And so what you found is that often, a lot of this has to do with naked economic self-interest that is expressed through the actions of Congress. And you make this claim that "competition may require some cooperation in order to obtain efficiency."

08-01:25:58
Telser: Yes, well, this is the lesson of some of the studies that were made of shipping conferences, for example, where, if you want to get regular service—and it's related to this point about the short-run return and the long-term benefit. If

you want to maintain the service, it may be necessary to prevent people from coming in and screwing it up.

08-01:26:34
Burnett: And, you don't have to talk about this conference in particular, but when you would make statements like—and this is just really striking, "It's not true that there's always a price which clears the market," when you'd make these statements, how was that received by people? Did you get pushback on this, or—

08-01:27:02
Telser: Well, I guess, in some cases, disbelief. What has happened, I think, is that economists, in the course of time, are more sophisticated and knowledgeable about mathematics than they used to be. I'm not sure about this, but I think this is true. So that, there are statements that may, to some people—I don't really know what their reaction is, but let me give you a simple example. Suppose we take the sequence one, one half, one third, one fourth, one fifth, and so on, and we sum. And the numbers are all getting smaller and smaller, and if you ask an economist, "What will be the sum?" it depends. Well, they say, "Well, I don't know. It'll be some kind of a number. You're adding less and less as you go along, and eventually—what you're adding is really very small—you'll get some kind of a sum." So you say, "No, that's not true. As a matter of fact, it's infinite. This series does not have a finite sum." I can name two economists who were very prominent who didn't know this. So this is the sort of thing. I mean, there are certain basic mathematical facts that, if you want to be an economist, you really should know them, because when you look at a series like this, you sort of think of a present value of an income stream or something, and you just don't believe it.

Sort of a more or less practical example: The price-earnings ratio of Amazon stock is over 200. That means the price of the stock is more than 200 times the earnings per share. And on the other hand, the price earnings ratio for Alphabet stock is about twenty, so the earnings ratio for Amazon is about ten times more for Amazon than it is for Alphabet, and it's very hard to understand that, even if you look at the growth or whatever way you do it. And right now, the prices of the two stocks, they're always close together. I've been watching it. There's something funny going on, the way—

08-01:30:28
Burnett: Between which two stocks? Between Amazon and—

08-01:30:33
Telser: Amazon and Google, I should say.

08-01:30:36
Burnett: Oh, Google, okay.

08-01:30:37
Telser: Yeah, Amazon and Google. And it's ten to one, Amazon versus Google, maybe a little more than ten to one, and that's hard to understand. Now, I just don't see how—there's certain things that you would have to know, so that, if you tell them, "Well there's some equations, don't have a solution, some systems," they don't really understand what that means. And I don't really know what you do about it, but if you're involved in economic policy, and you listen to some of these schemes, which do involve consistency of relations, and if you're really ignorant of this stuff, you can make some pretty bad mistakes.

08-01:31:40
Burnett: So is it that your average economist is kind of working with something like solid-state physics, and what you're saying is that mathematics is a language that is not perfect when it comes to describing—it does well enough for describing kind of the everyday, but pushed to its logical extension, it ceases to bear relation to forces that we see in the economy.

08-01:32:16
Telser: Right, it isn't enough. And then there's an extreme. You can go too far in one direction. There's a balance, somehow.

08-01:32:36
Burnett: Well, one of the things that also takes place in the 1980s, speaking of misinterpretations, there was the crash in 1987, on Black Monday, and you were involved in a task force to work on this and figure out what went wrong, and I'm wondering if you could talk about that, because there are some great figures in here: Robert Barro, Allan Meltzer, Gene Fama. So can you talk about what it was like to work on that project?

08-01:33:21
Telser: It was very interesting. The crash was on Monday, and there was a meeting, I think it was Tuesday night, in Chicago. I'm pretty sure it was Tuesday night. And from the U of C, Gene Fama and I went out, we were driven downtown to the meeting, and there were senior employees—let's put it this way—senior employees of some of the biggest firms. And some of them, I would say, were in a state of complete shock. And it was very surprising, the drop in one day. It's the worst one-day drop in the stock market. Even in 1929, in October, '29, it took two days for this sort of thing to happen, and on Monday, it all happened on one day. We were then supposed to get together as a formal group to try and figure out what had happened. Then there was one very important fact—I don't remember if I mentioned this before or not—about Alan Greenspan. Did I, talk about Monday night, when the market had collapsed? And there was somebody present on Tuesday who knew about this sort of thing, told us that Greenspan was on the phone, calling the heads of the big banks, and he told them that, "whatever the clearinghouse wants to borrow, you give it to them, no questions asked, and I'm going to back you."

Now, technically, what he did is illegal. He really did not have the authority to do that sort of thing, but he did it, and I think he saved the day. And fortunately, they did respond. I mean, the senior guys in this situation really did respond, and what it does show? One of the things, the lessons to me is—I wish Hayek would've been there. Oh, I should mention, by the way, once—this is much earlier; it's an aside. Ronald Coase was stuck with being the host of Hayek, and he didn't want to be the host of Hayek. Anyway, if Hayek had been present, he might've learned something, although I doubt it, but maybe he would've. Anyway, it's very important that you have people who are courageous, and know what to do, and do it, and I think Greenspan really did. And then, we had a succession of meetings, and there was a problem. I think some of the meetings were in Santa Monica, so we had meetings in California and Chicago. And then, a number of people: Allan Meltzer, I think, was involved; I think Merton Miller. I don't remember all the names, but those are the names I can think of offhand.

08-01:37:31

Burnett: What was the structure of the meetings? Was it in a roundtable? Was it organized around topics to address particular aspects of it? Was it more open and unstructured than that?

08-01:37:44

Telser: Well, the first thing was that we had very long studies that had been made, or testimonies by people who were involved, transcripts and so on. So we had a lot of that that we read, and we wrote papers, and we discussed it and criticized each other. In the later stages, we had written papers that we discussed. In the earlier stages, I think we were talking about what was going on and how did this fit, and how did it happen, and so on. And there were some heated discussions among us, about what was happening. Now for instance, I remember Merton Miller got very sore at me because I said that there was a bubble, and some people, including Merton, said, "We don't have bubbles, that's impossible." I don't know how you can argue. Of course, I'm biased in this. If you have rational expectations—by the way, Lucas was not a member of this group when I say rational expectations—I don't see how you could—well, like right now, the P/E ratio on Amazon for a business that's about thirty years old, I believe, it's very hard to understand, to me.

And there are models, and I think I've written about this, Keynes has the "greater fool" theory: I know it isn't worth it, but I'm going to buy it anyway, because a greater fool is going to come along and I'm going to sell it to him. So then there would be questions raised. Well, you're really talking about people that aren't behaving rationally, but, what's happening in a situation like this is, you're not really talking about the actual prospects of the company. What you're talking about are the reactions of those who are trading the stock, and you are making some assumptions about their behavior that may not be justified by the reality of the things that they're trading—of the companies represented by the things they are trading, so that you run into this

problem. Now, for example, I don't mind saying this: George Stigler, for example, when I said to him, "Well, you know, there was a tulip bubble," said, "No, there was never a tulip bubble mania." "How can you say that? If you read about it and you see what prices people were paying for tulips, and how you could sell this stuff, you can't believe that this had anything to do with the reality of the tulip market." "Well, no, there was no mania." And I find it very difficult to—I don't know how to handle this sort of thing.

08-01:41:21

Burnett: So his explanation was that they were rational to be investing in the tulip market, and those that got wiped out, deserved to get wiped out, and that cleared the market, and that's just a normal market operation.

08-01:41:43

Telser: Yeah, it just happens. And let's see, I don't remember his name, but what—well, for example, in the case—what you can do, you can conjure up a growth rate, for example, for Amazon stock, and then you could say, "Ah! It's a bargain." Well sure, you can think of a growth rate that would make it a bargain, but whether that makes any sense is another question. Anyway, if you look at this stuff historically, I think it took about two generations for the impact of the '29 crash to wear off, so that's sixty years. I hesitate to say this, but 1987, that means that we have about maybe thirty years or something, twenty, thirty years to go, before people go bananas again. I don't know, but I don't want to be committed to that. [laughs] There are some funny things that happen. One of the major effects, though, was that there was, certainly, in response, there was a very big drop in stock trading by the public.

But, the main point is that because of the actions of Greenspan and others, and the fact that things recovered—and again, I hesitate to say this, because in '29, it was similar. The market did recover, it didn't stay down, and the Great Depression did not result from the collapse of the stock market in October of '29; I think it resulted from very loose lending policies by the banks, and the loose lending policies by the banks in '29 was the result of loosening of legislation that loosened restrictions on commercial bank lending on mortgages, so we had a big spurt in mortgage lending. And then, if the economy goes sour, and there's a lot of people who are stuck with obligations that they can't manage, then there's foreclosures, and then, things really begin to unravel.

And here, by the way, is a source of disagreement between Milton and me. I think that the reason that we got into trouble in '29 is because of bank failures, and to talk about the decrease in the money supply is very, very misleading. That is not the problem; it's bank failures. But I think that lesson was learned, now. Both in '87 and in 2008, we did not have massive defaults by major lending institutions, and the Fed was smart enough to prevent it.

08-01:45:32
Burnett: And so we avoided the recommendations to let the market liquidate, [laughs] which would've been catastrophic. Is that just your view of the world, or is it rooted in your career of looking at how markets operate, and the importance of rules? And you've mentioned, it's not just rules. The rules are elaborated out of judgment, right? So you have the right kind of judgment that then gets elaborated into rules, but the rules, if they're too rigidly applied, can also lead to catastrophe.

08-01:46:22
Telser: Exactly. I think that what I'm talking about is sort of the—I'm not sure about this—it's sort of the approach of Confucius: You have to have the right people, and, the right rules. It isn't just rules. You have to have the right people so that they will know what to do, regardless of the rules. It has to be built in. Now, I think this is also Frank Knight's position, the importance of morality and all of that. He emphasized that a great deal, and that's very important.

08-01:47:04
Burnett: And you took that with you, when you saw that he incorporated a kind of a liberal arts training, in a way, into his economics instruction, that he wanted to cultivate the economist, and then that—

08-01:47:25
Telser: That's right, yes. The economist has to know enough, has to have learned enough and have the right attitude, well, especially in these sorts of situations where you're talking about the effect on the whole economy. It takes judgment, and there's something else that's involved here. It's not just knowing what series converges and all of that; it's a lot more than that.

08-01:48:11
Burnett: So, the task force then produced a report, *Black Monday and the Future of Financial Markets*, so that sounds like some recommendations. That comes out in '89. That's right around the savings and loan period, too, isn't it?

08-01:48:27
Telser: Yeah, and by the way, what's interesting about it is, there's a lot of typos in the hardcover edition. In the softcover edition, it was fixed up.

08-01:48:41
Burnett: Someone got a call, in [laughs] the intervening period.

08-01:48:46
Telser: My understanding is—we didn't get any royalties—I think it was widely read, and it has a lot of stuff in it besides the reports by the members of the commission. And one of the things that I learned, there were some people whose judgment I was led to doubt, and others, I thought came out pretty well. Oh, Bob Barro was in the committee, come to think. Yeah, so Barro, from Harvard—

08-01:49:29
Burnett: Yeah, that's right.

08-01:49:29
Telser: —and I think there were—I don't remember. Anyway, well, he was good, Barro was very good. The one guy that I thought was really terrible was Allan Meltzer. I thought he was a real jerk. I suppose that's going to—that's okay. That's not a secret. [laughter]

08-01:49:54
Burnett: Well, yeah, I think that the recommendations, did they play out in terms of policy? Were there reforms? Were there institutional reforms that were resulted from this?

08-01:50:07
Telser: Oh yes, and I think they're mostly very good, and I hope they stay in place. Now, for instance, there were some bad steps that were taken in the Clinton administration. As a result of the Great Depression, the commercial banks were not supposed to get involved in the stock market. There was an attempt to split commercial banking from the stock market. So if an ordinary person has savings, that, although it sounds sort of paternalistic, it's probably a good idea if they just put their savings in the bank and get a stable, decent return. It's not a good idea for them, for people who don't really know what they're doing, to go into the market.

08-01:51:02
Burnett: Right. Is that Glass–Steagall that you're talking about?

08-01:51:04
Telser: Yeah, so Glass–Steagall was, exactly, Glass–Steagall was essentially repealed, and that was a bad idea. And that happened in the Clinton administration, so you can't blame Republicans. There's a lot of things you can blame Republicans for, but that's not one of them.

08-01:51:25
Burnett: Yeah. Well, trade liberalization is a bipartisan, deregulation is a bipartisan project, isn't it, in that time?

08-01:51:32
Telser: Yeah.

08-01:51:33
Burnett: Well, perhaps we should pause there for a moment, and we'll take up next time. There's lots more to talk about, so, I'm looking forward to it.

Interview 9: October 19, 2017

09-00:00:00

Burnett: This is Paul Burnett interviewing Doctor Lester Telser for the Economist Life Stories Project, and it is our ninth session, and it's October 19, 2017, and we're here in Hyde Park, Chicago. And so we're still in the 1980s, at this point, and I do want to talk a bit more about the *Theory of Efficient Cooperation and Competition*, but in order to get there, I want to ask you about the angles and the themes that you chose to explore, and if there's any connection to the larger context of what's happening in the world in the late 1970s and into the 1980s—in the economics profession, more generally, and your specific field—that influenced and shaped your projects, with respect to this book.

09-00:01:23

Telser: Well, I think that one of the main factors that affected me was the feeling that standard economic theory was not very helpful in understanding the new problems arising in the economy, largely as a result of changes in technology, and that we had to adapt our model so we could understand things better, and there are a number of situations where the usual model would not apply. You would often have a case where it would be necessary to have two-part pricing, that is, instead of just buying a commodity at a so-much per unit, there would be a fixed charge, plus a variable charge that depended on how many units you bought. And this was an attempt made by business firms to handle the problem of covering their fixed costs. And this was a very important development that was pervasive in the economy, because we had capital goods—machinery, computers and things were playing a bigger role—and it was not really possible to figure out a direct relation between usage and the cost of these things.

So that was one of the major sources of problems. Closely related to this, there were practices that seemed to imply departures from competition. So for example, so-called rigid prices, administered prices, why were there administered prices? Why were there list prices? Why were all of these things going on? How do you explain what's happening in the economy? And the standard models really don't have anything to say about it. Some of the models simply assumed that, when there are rigid prices, that's some kind of evidence of monopoly or absence of competition. And there wasn't an understanding of how things really work, and there was a confusion, I thought, between what the end of an economy should be, where it's supposed to be going. And that is a point that was made much earlier by Adam Smith. There was a tendency to think that the end is always competition, that competition is, in a rather simpleminded form, always good. What bothered me is a lot of observations that couldn't be explained by the standard model, and so I would try to think about it.

Now for example, what really got me started thinking about the core in the first place is that, when you are in the standard economics course, you end up drawing a supply and a demand curve or diagrams, and there really is no explanation of where they come from. They seem to be just gimmicks to make it easier to see what's happening, so you can see with your eyes what's happening, but you're not getting any understanding of the underlying economic forces. So that is the sort of thing that bothered me. And of course, the main thing that led me into economics in the first place was the Great Depression, which is a common motive for many of the people in my generation. If you lived through that, it was an experience you would never forget, and you would wonder about it. How could things get so screwed up?

09-00:06:23

Burnett: Yeah. I imagine there's a visceral experience, and that does very much describe the experience of a lot of the major economists in your, loosely speaking, cohort generation. Gardiner Means wrote—was it 1934—published on administered—

09-00:06:45

Telser: Something like that, yeah.

09-00:06:46

Burnett: —published on administered prices, which is a phrase you came up with just now. Did he figure in, was he part of your formation in the backdrop, or did he continue to influence you? Or was he very much a product of his time?

09-00:07:04

Telser: Well, Gardiner Means was regarded as somebody who got it wrong. He didn't really understand what he had. Well, first of all, some economists at Chicago didn't believe that there really were administered prices, and they said that, "actually, prices are much more variable, and they adjust more to what's going on, and Means' results are taking us on the wrong road." But what bothered me about this is it doesn't really explain the fact that price lists are published, they're very expensive, they're not changed very often, and you cannot simply take the position that this is a figment of Gardiner Means' imagination. It must be real, and there must be a reason for it, and this is something that you have to try and figure out. In other words, it is an attitude I have that when you see something that doesn't seem to fit, you really have to think about it and try to see, well what's going on here? It's a challenge to the theory. So I was motivated, in many cases, in many of these examples, because I thought of them as a challenge to the theory that I should try to explain.

09-00:08:52

Burnett: And so that's what you attempt to do in all your books, and in this book in particular, and I'm thinking about audience. I think one of the phrases that you use—you do want to bring the invisible hand into your analysis, but it's after certain operations are conducted by different parties to arrange a market such

that the invisible hand can operate. Is that roughly characteristic of what you're describing?

09-00:09:28

Telser: Well, that's sort of putting in an abstract framework. Let me put it this way. There's a tendency to try and understand things in terms of slogans, and so the slogans are supposed to convey some kind of an underlying reality. But then what happens is, slogans become pervasive, and most people don't know what lies behind the slogans, and if you are a skeptical type of person, and you don't like to just accept slogans, you try to figure out what's going on, then you try to think about what are the reasons, what's behind all of this stuff? So the fact that here is a practice that seems to be expensive, there must be some reason why they're doing it, and this is typical of a lot of the situations.

Or else, it isn't even that. A very simple example would be in a stock market, people would say that "the way you can measure the value of a corporation is, you multiply the number of shares by the price per share." Well then you face the problem: How do you explain the fact that prices change? Well, you can think, people need the money or whatever, but the other point is, if it really does reflect the value of the corporation, then why are some people willing to hold onto their shares even though the price of the shares goes up? And especially, why are they willing to hold onto their shares when the price goes down? So that there are all of these problems that are raised when you look at economic phenomena like this, and they don't seem to be consistent with common beliefs or common sense.

09-00:11:59

Burnett: Well, maybe we can return to one other way of asking this question later, and save it maybe for tomorrow, and that is, in your reading of the history of economics, do you also have a different perspective of Adam Smith, for example, that you have a historical reading of what Adam Smith was saying that differs from the slogans surrounding Adam Smith today?

09-00:12:33

Telser: Yes, as a matter of fact, although he wasn't my teacher, he certainly influenced the teaching at Chicago, Jacob Viner said, "The puzzle is, how do you reconcile *The Wealth of Nations* of Adam Smith with *The Theory of Moral Sentiments* by Adam Smith?" When you look at it, it's as if two different people wrote each book. How do you put them together? And I think that there are elements in *The Theory of Moral Sentiments* that a lot of economists should take into account, but many of them don't even know about it.

09-00:13:21

Burnett: So, the way the *The Theory of Efficient Cooperation and Competition* is written, it seems like you're really not making a case for a paradigm shift with core theory. You want to say that a lot of this is compatible with neoclassical

theory, but there's value added in looking at these phenomena in terms of core theory.

09-00:13:50

Telser: No, I don't agree with that. I think it is a paradigm shift. For example, in core theory, you do not emphasize a proposition that firms are maximizing profits, or consumers are maximizing utility subject to a budget constraint. You are really looking at the world in a different way. The way you are looking at the world is, there are many constraints, and people are trying to work their way through these constraints as well as they can, and they are not thinking of this explicitly in terms of maximizing this or that. As a matter of fact—we'll come to this later—in the von Neumann growth model, nobody is maximizing. What firms are trying to do is avoid loss. There are no consumers in the model. There are only firms who are selling things to each other so that they are making inputs that will become outputs, and the outputs will become inputs, but nobody is maximizing. The activities that are taking place are the ones that can take place without incurring a loss. You are not trying to do the best, but you are trying to avoid a loss. And that's a different way of looking at it, and it seems to be more operational and closer, or better, at explaining actual behavior.

09-00:15:43

Burnett: So, in chapter five, which is called "Equilibrium with Decreasing Average Cost," you're looking at contracting, and you argue that "there need to be upper bounds on plant output rates, for certain kinds of industries"—I think this is still talking about Viner Industries—"on plant output rates to get an efficient equilibrium, so," you write, "some restriction on contracting is necessary for an equilibrium." So the argument is, in essence, that the market is not self-organizing, or if it is, you'd have to be more specific about it, [laughs] but I think in your model, there's a certain amount of institutional iterative formation that takes place in order to have market equilibrium, that is already constrained by decisions that folks have made about how to structure a market.

09-00:16:57

Telser: Yeah, well, that's right. These things evolved over millennia, not just centuries, but over millennia, and even let's say, on the order of 1,500, 2,000 years ago, there were pretty sophisticated arrangements in transactions, in Babylonia, for example, and these changed in the course of time. In Rome, and in Greece, especially in Rome, they developed corporations and condominiums and things like that, and forms of ownership that were very important. Here's an example in my work, in the book on joint ventures [*Joint Ventures of Labor and Capital,* Michigan UP, 1997]. You might wonder, why is it that a corporation has limited liability but a partnership does not have limited liability? In other words, in a partnership, an individual who wants to join the partnership must be approved by the existing partners, and an existing partner who wants to sell out to somebody else, the person to whom he sells has to be approved by his partners. So that's a different kind of an

arrangement than in a corporation, where on a stock market, you can buy and sell to whoever you like, so that it is a problem to try and explain, what is the economic function of this concept of limited liability? How does it enter? And in core theory, these are natural kinds of issues that come up, and are intrinsic parts of the theory. They're not afterthoughts; they are essential to the theory.

Core theory really is, I would say, mathematical institutional economics, if you want. It's like that. It isn't looking at whether people maximize or minimize. Once you start talking about constraints, you are really introducing the society and its rules. Some of these are explicit, and enforced, third-party enforcement. Another important element in core theory is you think in terms of self-enforcing agreements; that is, the parties to the agreement are the ones who enforce the terms in one way or another. They do not necessarily rely on outsiders. But actually, in a situation like that, the norms in the culture of the society are really entering into self-enforcing agreements.

09-00:20:21
Burnett: And that gets in a whole other sophisticated range of analyses, I would think.

09-00:20:29
Telser: But I wouldn't say that core theory is just patching things up. It really involves a different way of looking at it, that accommodates more elements than the standard model.

09-00:20:46
Burnett: I think what I was asking is, was there something strategic—because at the same time that you're arguing, you're also marketing. This has been a lifelong pursuit of gaining acceptance, knowing that there is built-in—you describe in each book — resistance. You're describing that this is difficult, and I think what you were trying to say here is that this is not—and when I say paradigm shifting in the Kuhnian sense, this theory is not completely incommensurable with older theory. In fact, it explains better, in some ways, why you would have something like Adam Smith's invisible hand. But you have to understand these other institutional constraints first, and how they operate.

09-00:21:40
Telser: Yeah, you're raising a very important point, and that is marketing, in a sense, marketing a theory. I don't remember, somebody said that "a new theory gains acceptance one funeral at a time," [laughter] a very famous physicist, I guess. And what I am doing in writing this stuff, you're trying to keep it calm. Don't lose your temper. I'm not really destroying everything you believe in and all of that. People have said this before, so don't get upset. There's a balance between, "Well, why should I do the new stuff? I've got the old stuff," and then you have to say, "Yeah, that's true, you've got the old stuff, but the new stuff is better and we're not really throwing out the old stuff."

Now usually—here is an example. There was somebody who said that "the general theory of relativity doesn't really replace Newton's theory; it simply

modifies it." Well, in a certain sense, you could say, it's a pretty big modification. But that is not a useful way to think about it in order to understand what's going on. It's not even useful as a marketing device, although you don't want to make enemies, you want to make friends, but it's a very fine line that you have to take. Actually, Sylvia is much better at this than I am. I'm trying to keep things calm, you know, so you can appeal—and it's accurate. I mean, Edgeworth really was doing this along these lines. Frank Knight had a very high opinion of Edgeworth, and he was amenable to listening to new ideas. Some people are more amenable, and others are less so, and it's a problem.

09-00:24:24

Burnett: I think that's part and parcel, speaking of marketing, of the development of scholarly identity and schools of thought. There's this famous/ notorious "Chicago School" of economics, that it's a body of work that has adherents. There's a certain amount of mythmaking, too, but it does involve the development of a kind of sub-discipline, and discipline implies following [laughs], not constant iconoclastic revisionism.

I want to ask you—and this is definitely a research bias on my part, because I see things in terms of the scholars that I have looked at in the Chicago camp, or at least in the department, so Theodore Schultz, D. Gale Johnson, your thesis supervisor, and others as well. And one of the things I thought of when you write something like, "Some restriction on contracting is necessary for an equilibrium," it made me think of something striking that Ted Schultz wrote once, which is, "We may need more government in order to give the farmer greater freedom." And I think about his training. He was trained in institutional economics, and I think so was D. Gale Johnson, and so I'm wondering, is there, in that agricultural group at Chicago, an institutional-economic strain that you picked up a little bit of?

09-00:26:28

Telser: Yeah, I think that there's a great deal in that, because even though—well of course, D. Gale was the chairman of my committee, so there's an obvious, direct relation, and I would often discuss things with Ted Schultz, and we frequently would, very often would, walk home together. They lived not very far, west of us here, on Kimbark, I think.

09-00:27:02

Burnett: Yeah, that's right.

09-00:27:03

Telser: And Ted was not an ideologue. These are people who are, they're open-minded, they're willing to listen. That doesn't mean they tolerate nonsense, or that they won't challenge you or anything like that, but they don't take sort of a theological position on this stuff. If you think of these things as a science, which I think you should, then you have to reckon with evidence that seems out of place, and new evidence that appears, and you have to be adaptable. For

instance, now, one of my best friends I mentioned was Zvi Griliches, and his dissertation is on the adaptation of hybrid corn, and there's a very curious thing about hybrid corn that fits into what we're just talking about, and that is, the main development of hybrid corn did not occur in the major corn-producing states, but it was in Connecticut. I think I have that right, and at Illinois, certainly at Illinois, maybe at Iowa, there's sort of a lot of after-the-fact massaging of the data to give them a bigger place than they actually had, in the development.

Another example of this that struck me: When I was doing my dissertation, we had desk calculators; we didn't have computers. Friden, Marchant, and Monroe. So these are desk calculators. Then, computers were beginning to enter, and it always was a puzzle to me, why didn't these guys, who are selling their stuff to the people who are going to be using these computers, why didn't they adapt? There were only very, very limited adaptations by these companies, a little fiddling around with their stuff to make them work a little faster or use vacuum tubes or whatever, but, they were completely left behind by the digital computer revolution. It's an interesting topic. Now, one of the things that Zvi found—we tried to get reasonable, rational explanations of this. It turns out there are special problems connected with hybrid corn, and one is that you have to develop a seed that is suitable for a particular latitude. So the seed has to be appropriate for a latitude. You can't just say, "Well, these guys could just take over hybrid corn." It was a little more complicated than that. They had to have the right seed to grow in their particular farm. So there was a cost of adaptation that was not related to being stubborn, or stupid, or anything like that. There's some real factors that enter into it.

09-00:31:10
Burnett: Some constraints.

09-00:31:11
Telser: Yeah, exactly. There are constraints. And so you have to take that into account if you want to understand what's happening.

09-00:31:21
Burnett: And it seemed to be part of a question, and I would love to have been able to speak with Zvi Griliches, but we can't, obviously.

09-00:31:34
Telser: Yeah, he died, unfortunately. I think he was only sixty-nine.

09-00:31:39
Burnett: Yeah, really young. And, I think the interest that Schultz had in that research was this notion of a seven-to-one return on investment in this type of research, whether it was public or private, but the role of research in agriculture is a really important thing to Schultz which becomes important later in *Transforming Traditional Agriculture*, and in a sense that whole agricultural

orientation at Chicago, that that goes out around the world, expounding on the importance of investment in human capital with respect to agriculture.

09-00:32:21
Telser: By the way now, Zvi's thesis topic was from Ted Schultz. He's the one who suggested it.

09-00:32:30
Burnett: So, but all this to say that there's a concern with custom, with institutions, with the ways in which human beings shape how trade is undertaken, how organizations are structured, and that the evolution of constraints needs to be dealt with or reckoned with, almost before you can talk about an invisible hand, or if you can talk about these kinds of things. And you say at the beginning—now, I don't want to miss it entirely—it's in chapter four of *Theory of Efficient Cooperation and Competition* from 1987: "The standard neoclassical theory assumes the existence of a distinct firm, but remains silent about such questions as why firms are specialized, what purposes they serve, what determines the number and size of firms, and so on." And so, that, to me, is kind of an institutional framework, that would ask, "Why do you have a firm in the first place?" And would the answers to that question tell you something about the kind of microeconomics of the firm, or your theory of the firm?

09-00:33:51
Telser: Well, actually, it's interesting because that's very much related to the work I've been doing in the last ten years, where—we can't go into it, but it takes explicit account of the relations among the individuals that, instead of talking about a coalition as a sort of amorphous term, what corresponds to the basic organization in my new stuff is a circuit, and it's related to a certain branch of mathematics that was started by—I don't know if we want to talk about this, but it's a branch of mathematics, topology, that was started by Euler in an solution of a very famous problem called the Seven Bridges of Königsberg.

09-00:34:59
Burnett: We did talk about this in an earlier session, but not fully, so, maybe, do we want to put a pin in this until tomorrow's session?

09-00:35:11
Telser: Sure. But, just the main point of it is that, once you start talking about circuits—which is, there's a certain pattern among the members, you go from *a* to *b* to *c*, and then there's relations among circuits and so on—you have an explicit model or structure. It isn't amorphous anymore. It doesn't say, well, a firm, it's workers and capital and all that sort of up in the air. It isn't like that; it's very specific. And then you try to get results, and in particular, one of the major results that I got is that, in general, you get nonempty cores in that type of structure, which is very interesting, and not something I expected.

09-00:36:17

Burnett: Well, we should return to that and talk about how your research has evolved. And so, to return to this question of, what determines, I think, why firms are specialized, and that's not necessarily in the scope of the book, the project that you had, but what determines the number and the size of the firms is, and you're also interested in this problem of output rates. Can you talk a little bit about that, why that is something that requires advanced collusion or cooperation in order for what you say is Adam Smith's invisible hand theorem to obtain?

09-00:37:16

Telser: Well, it's related to the problem that you have to think of different kinds of variable costs and fixed costs, and this complicates things. The basic principle of Adam Smith is that you're trying to figure out how to run an economy so that the consumers can get what they want or need at the least total cost. So it is not a question of figuring out how to make firms better off. What you're really trying to do is to think of the welfare of the consumers, and how that can be best promoted, and because of the nature of the cost conditions, the variability, there are certain facts about how one should proceed, how one should produce, and so on, that makes things more complicated. And in particular, what is involved here is the need to have arrangements so that firms are not left at the mercy of unforeseen fluctuations in demand. Some of the risk has to be borne by their customers. They have to have suitable arrangements so that both parties will be better off, and this comes up in particular situations.

Well, first, it comes up in the case of price gouging. That's one way, but then there's another side of it, and that is that the firm may have made a commitment to customers to sell at a certain price, but in fact, there's a tight demand, and the spot price is very high, and yet the firm continues to honor a long-term commitment to their customers, or the firm has honored a long-term commitment to its customers, that they're not going to raise the price when things get tight. And the reason that you're doing these sorts of things is because you have sort of an eye on what the long-term situation is. You're not just looking at the immediate action that might be appropriate if this were the only thing that's going on, but you have to have a longer horizon, and take these longer-term factors into account. So the invisible hand, it's a sort of a conclusion that may work under certain conditions, but it's really not obvious when you start looking at things in detail. And there are some very famous classical economists who did not have a very high opinion of some of Adam Smith's writings on this subject, like, for example, Henry Thornton, who's a highly regarded monetary theorist, did not have a high opinion of what Smith had to say on monetary problems.

09-00:41:17

Burnett: But you might organize a market such that a kind of invisible hand might operate locally under certain conditions, right?

09-00:41:33
Telser: Maybe. These are not saints, or patriarchs or anything. Adam Smith was somebody who wrote about economics and published a book in 1776, but it's not sacred writings. It's not like a bible or anything. And none of these people should be regarded in the same way, let us say, as theologians would regard some of their heroes.

09-00:42:10
Burnett: [Chuckles] Well let's pivot to—

09-00:42:14
Telser: Including me, by the way.

09-00:42:15
Burnett: Right, of course. I think another piece of research that you explore in this book is the relationship between research undertaken by whomever, and competition and collusion. So I'm wondering, what's driving your interest in the relationship between research, innovation, and the core?

09-00:42:54
Telser: Well, the problem here is, it is a search problem, but when you find what you're looking for, there is going to be a general benefit. So the question is, what's the best way to look? Is it better to have one person looking, or should a lot of people be looking? And if a lot of people are looking, there may be wasteful duplication. On the other hand, if a lot of people are looking, somebody is going to find something, and it would be useful for that to be available to others. So that, if you just sort of think of this as a horse race, and one of the horses is going to win, that is not a socially useful way to regard the problem. What you are facing in these situations is a problem that requires, or where it's desirable to have sort of a pooled effort. You could see this in reality if you read a good study, for example, of the atomic bombs. The atomic bomb research was financed primarily by the US government, and as it happens, the person who was actually in charge, General [Leslie R.] Groves, was very, very capable, and he was always wrestling with this problem: How much do we tell people; how important is rivalry; do we have duplication? And I don't think we're really smart enough to come up with a good answer. And when the thing is really important, it's very tough, you know. For example, the government is going to allocate ten billion dollars to find a cure for cancer. Well, it's not obvious what they should do.

So those are the issues, and what I was doing here was actually something that's very simple—well, or maybe it's a simplified version of the problem. Suppose you're trying to figure out how to lower the cost. Well, if you can lower the cost and it's certainly going to benefit everybody who uses the product—and then there seems to be a formal difference in the model, if you say that "it's just a question of spending money and trying out different alternatives," or, if the outcome is some kind of a random variable, so you can't really say that "so-and-so is more likely to stumble on it than this one."

So this is just a way of looking a little bit at the process, but I don't think there's really much we can say about it that might be generally useful.

09-00:46:56

Burnett: Well, one of your questions is, in your observational work, "What do firms do to spur innovation when free riding is a problem?" Right? So you're thinking in core terms about this problem, and you black box this in your—because you're only talking about free-rider problems with respect to private investment, but you mention that there are other solutions to free-rider problems beyond the scope of research, and that's when the government funds the research. And so that's something that was happening, certainly happening during your career, but it begins to kind of accelerate in the eighties and into the nineties and 2000s, and that is that there is this kind of grand divestment from research on the part of private corporations. The industrial research labs are kind of winding down, and they're spinning off their research divisions, basically, to research universities. Now, was that pattern evident to you at the time, or was that something you were thinking about? It was beyond the scope of this project, but was that something that was making you think?

09-00:48:23

Telser: Well, there was a very practical effect. I had an NSF project, and Lyndon Baines Johnson decided, for whatever reason—he did give reasons—that he would just stop funding, so the U of C was left with a commitment, but with no money coming from the government to support their NSF commitments. So they were stuck. They could not walk away from things the way the federal government did. So this was a practical—and it certainly affected me and others—this was a practical illustration of what goes on. I was also a member of the—I don't remember—was the NSF Advisory Council or something.

09-00:49:18

Burnett: Yeah, in the early seventies, is that right?

09-00:49:20

Telser: Yeah, in the early seventies, and so we get research proposals, and we look at them. We met four times a year. Everybody read the proposals, and then at the meeting, we would decide, well, who should be funded and who shouldn't be funded, and you can read all kinds of stories about this. Now as a minor point: if somebody from your institution had a grant then, you had to leave the room for obvious reasons. And I remember that once, there was a proposal that came in to support research for supersonic transports in Seattle, Washington, and there was a letter that came with it from the chairman of the Council of Economic Advisors that "the president would like you to approve this proposal." I will not describe the terminology that was used in the group [laughter] to reject it.

09-00:50:31

Burnett: You had mentioned this, yeah.

09-00:50:34
Telser: Yes, I did have some practical experience with this sort of thing, and it's very hard. And of course, in business firms, it's very hard, and we earlier did talk about an example with the pharmaceutical firm.

09-00:50:58
Burnett: Well, whole new avenues for research and innovation are opening up at this time. So there's *Diamond v. Chakrabarty* in 1980, which allows for the universities to profit from the patenting of living organisms; there's increased venture capital research due to the changes in the tax structure in California and other places; and then there's the general tax reform that unleashes, according to some, the floodgates. And so it's a new investment and innovation space at this time, and so I'm wondering, when you're thinking about this research, if that's impacting your work in this book.

09-00:51:53
Telser: It is. Well, there's an official problem here, and it comes up, in fact, in one of the papers I wrote in an example I gave on the development of the chronometer to measure longitude. And the British government—this is in the eighteenth century—offered a prize, and I don't remember, I think his name is John Harrison, the person who worked on it for forty years or so, and did perfect the chronometer, and they tested it, went on voyages to Jamaica and back. It was very accurate and so on. And then what happened is, as you might expect, that the government reneged, and it was interesting that in that case, the one who intervened and paid off the inventor, it was George the Third, who was interested in this. So that contrary to this kind of stuff that we are taught about George the Third in schools, he actually did something that was very good. He probably did other things that were very good, but this is a very dramatic example.

So the problem is that when the government does something, will they stick to it? Can you count on them when they're tempted to back out of an arrangement? So that there are problems in doing these sorts of things when some kind of a group is responsible, or some bigger organization is responsible for putting it all together. This is the type of problem that core theory wrestles with. It becomes a formal problem. You can write it down and so on, and when you do that, you can see problems in solving it.

09-00:54:29
Burnett: Absolutely. You bring in Schumpeter into this work and the alternating cycles of research: a phase of research, and then a phase of maturity. How did that influence you or shape this research?

09-00:54:51
Telser: I consider Schumpeter one of the great economists. The main insight he had in one of his early books, which he wrote in German—and it's harder to read than his later books, which I think he wrote in English. But I think he understood things in a better way than all of the economists who were writing

at the time, and I think his ideas have had an important effect, in fact, in particular, for example, on Joe Stiglitz, who wrote on innovation. See, you want to give people an incentive to do this sort of thing. So what happens is, they get a patent, and then you get into problems about, is it really an invention? Is it really new? How long should they get the patent? They're controlling the price, so not as many people are using it as they should, so you have to balance the incentive to discover by people against the disadvantages if they get a patent, and then there's a Harvard economist, [Frank W.] Taussig, who said that "most inventors aren't motivated by profit anyway; they like to do this stuff." So we really don't know. We don't know enough about humans to make any definite pronouncements about how this stuff should be done. We do know that, in some societies, it's very difficult. In our society, it's not difficult. But there are societies in this world where you really can't be an innovator.

09-00:56:53

Burnett: It becomes part of a huge industry in economics during the period of your career, is this interest in innovation and its relationship to growth, and so you have growth theory, and it's particularly acute in the case of developing countries that have a really limited amount of resources to devote to this. And there was tremendous competition among economists and other types of experts to lay claim to the correct formula for producing innovation. But that's what economists say. It is this kind of random event, or it's unpredictable which investment is going to lead to a particular innovatory outcome.

09-00:57:50

Telser: Yeah, and then, the most recent example is in the Nobel Prize for the detection of gravitation waves. That was a search that went on for I think forty years? Billions of dollars, and I am sure that you could find many senators who would have told you back then, "It's a waste of money." I don't know. Why should we worry about this sort of thing?

09-00:58:20

Burnett: Well, and it's difficult to price certain things. We very clearly need innovation in antibiotics research, but it's very difficult to price that, because the value of antibiotics, of new antibiotics, lies in not using them, [laughs] and so you can't sell them, right? And so we end up with these kinds of market failures in areas that are most urgent, that are clearly the most urgent. And so we do have these problems as a society, defining what our priorities are in terms of innovation, and so that is a kind of institutional and perhaps moral problem, I suppose. And so, could you talk about the reception of this book? How was it received?

09-00:59:21

Telser: I think it had a pretty good reception, and let me see. It came out in a paperback edition. It is now available print on demand, so I think it was pretty well received, and I think that Cambridge is happy with it, and I'm happy with it. It's not a bestseller, and it's certainly not easy reading, in parts, but I think

it does have things in it that are important, and that economists should learn about, and it seems to have had an effect.

09-01:00:07
Burnett: I picked up just a couple of themes; there's a lot in this book. So, are there other aspects of the book that you wanted to highlight? Because I picked just a couple, and there's a bunch. [laughs]

09-01:00:19
Telser: So, you know, it's hard for me to remember these things, so I don't know. I'll think about it, but right now, I don't have anything to say offhand. I think we've pretty well covered it.

09-01:00:34
Burnett: So, the year after, there's a new edition of *Competition, Collusion and Game Theory*, which came out in '72, and there was another edition, I think.

09-01:00:54
Telser: Well, I think Macmillan published; yeah, besides the North-Holland edition, I think there's—I'm not sure about this; I don't remember. Maybe Macmillan published it. The main thing that did was just correct some errors and typos and so on. It's not really that different from the first one.

09-01:01:19
Burnett: Yeah. And you tightened up the title; you called it *Theories of Competition*.

09-01:01:24
Telser: Right. Yeah, that, I think is significant.

09-01:01:31
Telser: And that's because of experience with some consulting.

09-01:01:38
Burnett: [laughs] Well, I think it does get you away from the problem of confusion around "collusion." I see "cooperation" slipping more and more into your article titles, as opposed to "collusion," and this is around the time when that starts to happen.

09-01:01:57
Telser: Yeah, in fact, I think in one of my books, I use "alliance." I thought they were euphemistic. They are substitutes, yeah.

09-01:02:08
Burnett: I think it might be a good time to talk about this. One of the things that we haven't addressed so far is your role on editorial boards for journals, and you did a lot of service on those, and you were on the board of the *Journal of Futures Markets* from 1980 to 1995, and others you had long terms on, as well. I'm wondering if you could talk about that service, and how that affected your work. What are the advantages and disadvantages of doing that kind of service work?

09-01:02:51
Telser: Well, it's hard to think of any advantages, because if the author's paper is published, then that's okay. If it's not published, then it's rejected, and he gets sore at you, so that, since most papers are rejected, it's not a good way to make friends. It is useful in the sense that you read about new material. It keeps you in contact, and so forth and so on, but it's, I would say, it's sort of a mixed blessing. It's something that you have to do, let me put it that way. You really have to do it, and I try to do a conscientious job. And I have had, in my own experience, I have had some very good referees and editors, but, in more recent times, that has not been the case. And I think the BE Press and the ability to publish stuff online and so on is a great advantage, because I don't think that editors and referees take their job as seriously as they used to. For example, Edgeworth was the leading editor of the *Economic Journal*. There's nobody like Edgeworth around now, who's an editor of a leading journal. Harry Johnson was the editor of the *JPE* for many years, and he was a terrific editor. Earl Hamilton was editor, also a terrific editor, and I'm trying to think, Avinash Dixit, terrific editor. So there have been very good ones, but I think that it's becoming more and more the exception.

09-01:04:48
Burnett: Well, the nature of that publishing market has changed, and the publishing houses and the homes of journals are under the gun, I guess, in terms of resources.

09-01:05:00
Telser: Well, another simple example that makes the same point, and that is the requirements for a PhD. It used to be, in the Economics Department, that the whole faculty had to attend the final orals. They had to read the dissertation. A dissertation had to be published. You had two foreign languages, and then, when I was a student, you could substitute mathematics for one foreign language, and so on. All of that stuff is gone now.

09-01:05:37
Burnett: It is.

09-01:05:38
Telser: Yeah. Faculty doesn't attend. It's that the standards are much reduced, and I think that is simply correlated with the slack standards now also for editors in journals.

09-01:06:00
Burnett: So, well, this is maybe a good time to talk about the evolution of the Economics Department. So you studied in the Economics Department, you spent almost ten years in the Graduate School of Business, and then you returned as faculty to the Economics Department.

09-01:06:21
Telser: Let me see. Was it ten years? Yeah, '58 to '64.

09-01:06:26
Burnett: Yeah, so it's six years, but a tremendous experience with the Economics Department at Chicago. Can you speak about the evolution? We could talk about the lowering of standards to some degree, but the evolution of the department in terms of how the graduate students were trained, the various leadership roles in the department. So, D. Gale Johnson was chair twice, right? I think '71 to '75, or '70 to '75, and then '81 to '84, I think.

09-01:07:09
Telser: Yes, and Al Harberger was the chairman for a long time. And of course, Ted Schultz was a chairman. Now, I can't say anything about the chairmen after Al. I didn't really have much to do with the chairmen after Al. There's a difference between a chairman who really takes a positive role, and is out there trying to do his best to improve the department, and a chairman who considers it a chore, and it's something that you have to do, but with no driving interest in raising standards. I wouldn't say that they're uninterested, but they do not consider this on a par, let's say, with their own research or their own publications and so on. That is, the public aspect of this tends to diminish relative to the private interest. So that changes over time.

I actually stopped having an active role in the department now for maybe eighteen or nineteen years. This is partly for health reasons. Of course, getting older is one factor, but earlier, starting in 1994 when I was diagnosed with a very serious illness and I was told that the life expectancy was three years, that changed my attitude on what I'm going to be doing in the next three years, and so on. The diagnosis was changed later, or they revised it. They said they made a mistake. The reason they said they made a mistake, of course, is because I was still alive. But, [laughter]—

09-01:09:39
Burnett: It's a good mistake.

09-01:09:40
Telser: My impression, while I was in the department, we had, I think we had very good students. I think things began to slip in the nineties. Until the nineties, I think we were doing okay, and we were attracting very good students, it was very tough. I don't think that the same kinds of standards then prevailed after that. And my impression is, I don't think that this is something that's special to this economics department—I think that there's a general slackening in economics, generally, in the US, and maybe elsewhere, but that's my impression.

09-01:10:31
Burnett: What do you think has happened? It's an important field of research that governs a lot of our world. It's enormously important, and you would think that people would be drawn to it or animated by it. What's going on there?

09-01:10:53

Telser: Well, one thing that may have caused a change is, starting in 2008, when there is the perception that there was a crisis, and the economy was in tough shape. Then the other factor that I don't think is appreciated yet, but I think the appreciation and understanding is growing—realizing that the digital revolution is unprecedented, and is having a huge impact. A column George Shultz had in the *New York Times*, on Tuesday. It was interesting. There were three points raised. There's also [Pedro] Aspe as co-author—one of our Latin American PhDs. The first topic in his article has to do with the implications of the digital revolution. He is the first major statesman, that I am aware of, that actually put this right up front, and I think that it's beginning to percolate. I thought that this is happening. My views on this are not recent, they go way back, because I was involved starting with the UNIVAC I. I could see all of these things developing over the last sixty years. And people really don't appreciate, yet, all of the effects of this, and especially, the economic effects, and that may change the standing of the profession.

What has caused the drop off, I think before, is that when the economy is doing well and prosperous and so on, interest drops, especially when you can make a lot of money in finance, or something. Or if you have more intellectual interests, there are fields in the sciences that are more attractive, for example, working in artificial intelligence, so that the mix of forces may be changing to favor economics more now than was true, say, twenty-five years ago. So I don't know. I don't think that there's a trend. I think there tends to be cycles, so there are periods when the interest goes up, and periods when the interest goes down. I think we're in a period now where the interest is going up.

09-01:14:22

Burnett: We're moving into the 1990s now, and you were joking earlier about your ten-year average for the production of a monograph, but ten years later, you do publish *Joint Ventures of Labor and Capital* with Michigan in 1997. There are again a number of cases that you look at, and it's along this theme of what happens when there's an empty core, and you do basically write about using core theory to deal with gaps in the theory of the firm. So can you talk a little bit about what is going on with respect to the core and the formation of joint ventures, for example?

09-01:15:29

Telser: Well, let me put it this way. Most of the work on core theory before that has to do primarily with market exchange. In this book, the theory is applied to suppliers—that is, corporations directly—and to workers who are the employees of business firms. Now, in the case of corporations, economics really had very little to say about it. It was left to people in the business school in finance to study finance. So that, the issue of preferred stock, common stock, bonds, all of that, was put aside and separated from what it was that a corporation actually did. And so the focus now, in my model, is on the investors' in corporations. Who are the investors? Can core theory distinguish among corporations according to the kind of investors they attract? When I

say corporations, I'm including mutual funds, partnerships, and in corporations. Does core theory have something to say about it? And the answer is yes. Standard economics totally ignores this, whereas it is a very important aspect in the application of the theory of the core.

So that's one part of it, and the other part of it has to do with workers forming teams. Now, the puzzle with workers is that when people do studies on ability, they find that, according to the standard measures of ability, with IQ tests et cetera, it's a normal distribution. But when you look at the distribution of wages of workers, it's a skewed distribution. So then, if you assume that marginal product is somehow related to ability, how do you explain the fact that the actual wage distribution is so different from the ability distribution reported by the psychologists? So that was one of the problems.

Now it was interesting that if you make assumptions about abilities, teams of workers, interactions, and things like that, and you apply core theory to that, you actually do get results that, for example, show a relation between the remuneration of the workers and the size of the firms. And there are other aspects of remuneration that are revealed by core theory that the standard theory seems to ignore. And going back, I already mentioned that, in core theory, you can explain partnerships differing from corporations by the existence of limited liability.

But there is another interesting aspect of this, when we're talking about corporations, although this to some extent came up before, and that is the theory will actually tell you something about the distribution of stock prices, as a result of exchange among the traders. That's pretty remarkable. And in the theory, what happens is that the focus is on the fractiles of the distribution. The advantage of looking at fractiles of a distribution, as distinct from either the mean or the variance or something like that, is, every distribution has fractiles, but with the other stuff, it depends on whether you have fat tails or skinny tails, or the shape of the distribution, in other words, that sort of complicate life. Whereas here, you get a much more straightforward theory of what the distribution of prices will be by working with fractiles. And fractiles are a natural way to describe the interaction between buyers and sellers, and it takes into account, in the case of corporations, the following, what I think is a pretty obvious fact.

There are people who own stock, and there are people who are either buying stock, or selling stock. Now the people who own stock must think that the actual value of the stock to them is not the same as the current market price of the stock, which is governed by those who are either buying or selling the stock. In a way, it's kind of obvious, because, if you own a car, you don't look at the newspaper every day to see what the price of the car is to decide, should you keep your car, or sell it and buy another car? Well, actually, for most people who hold stock, it's the same thing. They treat holding the stock sort of like holding the car. In fact, this is based on their views of the capability of the

corporation and not about what's going on in a market. And of course, the ones who are more going on in a market—you know, where is the price going, up or down—by the way, today is October the nineteenth. It's an anniversary, it's an unfortunate anniversary day. This Black Monday, 1987, was October the nineteenth. Anyway, so, in the book, I am talking about corporations and the remuneration of the employees, and I'm using core theory to explain things that the standard theory seems to ignore.

09-01:23:23

Burnett: And there are implications, and you write that "there are implications for both the owners of corporations and for the owners of mutual funds," and so, basically, you're suggesting that some of the conventional measures for the value of a corporation are either under- or over-estimated, on the basis of this.

09-01:23:44

Telser: Right. Once it's brought to your attention, it's pretty obvious. The owners of mutual funds are usually a more diversified group than the owners of corporations, that is, if you do surveys of this sort of thing, and there used to be surveys of this sort of thing. But when interest by the theorists or economists in these questions drops off, then the surveys disappear, which is not a surprise. You're not going to get people to start looking for things if there's nobody interested in what you're going to find.

09-01:24:30

Burnett: That is a problem. It's a refrain in our sessions, the data, for economists, and the problem with data, the problem with private data, and you mention the reliance on court cases as these tremendous treasure troves of data for economics, because companies are just forced to divulge information about their operations, and time series, and so on. And so there is that part of the Graduate School of Business that was this interdisciplinary engine, at least under the [Allen] Wallis restructuring, and there is a branch of, they call it economic sociology, and so this has, I think this has become more popular post-2008, when people are wondering, how did we let this happen, and what's really going on when we talk about these financial arrangements? So there are, again, social scientists and scholars going out to do these kinds of investigations to understand what's really going on. It's a hard problem to work around –

09-01:25:55

Telser: Now, there were more studies of this, actually, in the thirties under the TNEC [Temporary National Economic Committee]. There were a lot of very useful and important studies under TNEC and in that stock. Temporary National Economic Committee, one of the major New Deal programs.

09-01:26:19

Burnett: Right. So, perhaps we need a new one. [laughs] So there are a number of conclusions that come out of this. One is that you note that bonuses are better than profit-sharing for principals, right?

09-01:26:45
Telser: Yeah, and there, too, it's kind of an obvious point. If you don't think about it, you say, "Well, if the manager owns stock in the company, then you align the interests of the manager and the company." But of course, most managers' fraction of the company is infinitesimal, so you're not going to get anywhere that way. The way you can really inspire devotion is by, in effect, offering big prizes. Some of this stuff, it's so obvious, I'm almost embarrassed to say it, but it is pretty obvious when you think about the incentives you offer people, that there are certain common-sense factors that enter.

09-01:27:42
Burnett: Well, we've recently become enamored, and that's something about science, right? A lot of the attractiveness of science is coming to counterintuitive conclusions, right, that it is almost a different form of sense, and that providing scientific validation for common sense is not necessarily [laughs] as attractive, but this is not *Freakonomics*, in other words. [laughs]

09-01:28:11
Telser: No. Yeah. I think we can—well, okay. No comment. [laughter]

09-01:28:20
Burnett: Okay, [laughs] fair enough. And then there's one other interesting case, the example of the old motion picture industry, and that's another case of a Viner Industry? That has a multi-product –

09-01:28:39
Telser: Yeah.

09-01:28:40
Burnett: And when you talk about U-shaped cost curves, it means that, at lower quantities of production, the costs are high, and then it reaches a middle point where it's maximally cheap, and then the costs begin to rise as output expands.

09-01:28:54
Telser: Right.

09-01:28:54
Burnett: Okay. So, what happens when the case of the old motion picture industry—what is that story, effectively?

09-01:29:04
Telser: Well, there's a political angle here. There are people who criticized the motion picture industry because they didn't like the movies, for whatever reason, and some of these are—well some of the objections were sort of based on religious, or allegedly on religious objections, so that there were criticisms of the movies. And this, I think, is one of the factors that impelled the Justice Department to bring suits against movie industry. Now, when you bring an antitrust suit, you have to have some kind of a reason for your complaint, and in their case, it was block booking.

Well, first of all, there were only six major studios. And what they would do is offer a package of films to the theaters. And the idea of the Justice Department is that this interferes with competition, because you're forcing the distributors to take this block whether they want it or not. And so there was no real attempt made to understand the economic purpose of having a block. Why do you have a block? And there were some economists who wrote articles which were sort of sympathetic to the Justice Department view, and they said that "the block represents sophisticated kinds of monopolistic price discrimination, so they're bad." And, I did not think that this was a plausible explanation, and there's actually no evidence to support it. It's just one of these things. You can write down equations in mathematical economics that don't make any sense. The fact that they're in the form of mathematics does not sanctify them.

Well, if you look at this stuff, and try to get at the facts, the first thing that you notice is, in those days, there's no television. So one of the major forms of entertainment was either to go to a play, a live production, or to go to a movie. Now, a live production was relatively more expensive than going to a movie, and there were lots of movie theaters. And some movie theaters would be open literally twenty-four hours a day; others, for not such a long period per day, but seven days a week; and then there were fancy theaters that showed films downtown—

09-01:32:42
Burnett: Movie palaces.

09-01:32:43
Telser: —at very high prices. So there's first-run theater, second-run, third-run theaters, and so on, and there's fifty-two weeks in the year, and they have to have, I don't know, two, three films a week or in a first-run theater, maybe a new film every two weeks or something like that, so then you're talking about putting together a package of films for your customers. So that's one part of the problem, just taking into account the mechanics of the industry. The other part of the problem is, well, you've got to make these films. You have to produce the films, and there are good actors and there are not-so-good actors, and good directors and not-so-good, and writers, et cetera. F. Scott Fitzgerald, by the way, has a very good novel, *The Last Tycoon*. He was one of the writers, by the way, in Hollywood, and he didn't finish it, but it's still very much worth reading. So you can find out, and I remember, he said that there was a very famous English writer that was hired by the studio, a great novelist, but he didn't know how to write a movie script. It's entirely different.

So what you have to do is you have to have good writers, directors, and all of that, but when you do that, and you have a script and you make the movie, you don't know if it is going to be a hit. Is it going to be a flop? You don't know. And the idea was that if you hire the most high-priced talent, there's a

higher probability that you'll have a hit than if you don't, if you hire the lesser ones. So what you do is you put together a package, and you can sort of estimate how well the package will do, because there will be good ones and bad ones and so on. So, it is the interest of the producers to have some kind of a standard flow of revenue to meet their expenses and so on, so they put together a package, and they lower the variability of the outcome, and they can kind of predict what the stream is going to be. And so, they don't know what's going to be a hit, and the distributors don't know what's going to be a hit, but you put together a package and you fill up the theaters, and everybody is happy. And then of course, you also know that there is a demand for certain kinds of films. They had categories: thrillers, musical, love stories—

09-01:36:01
Burnett: The genres.

09-01:36:01
Telser: —so there would be categories, and you put films into these categories, and you figure out how many will be showing this week and that week and where and when and all of that, and it's got nothing to do with any kind of fancy theories about monopolistic price discrimination. Just, how do you handle a problem when the demand is not very well predictable in particular, but is easier to predict if you can sort of package it? The same thing, I would guess, is true for other consumer products, where there's a variable taste, you don't know what's going to be in fashion this year and so on, the same sort of thing. But, there was a political angle in the motion picture industry that is not as important, I think, as it was in some of these other industries, where fashion is important, or even in the publishing industry.

09-01:37:18
Burnett: Right, wanting to punish the film industry for its transgressions.

09-01:37:21
Telser: Yeah, and then of course, there are other little problems that come up. The studios would have guys driving around to make sure that the theater wasn't cutting the price on the tickets and making it up on the popcorn price. There were all kinds of little details that some of these economists apparently didn't know anything about, in other words, enforcement problems, which I also talk about in various books, where, what we call principal-agent problems, where, you have people working for you, and they can try to skim. And so you have a problem of making sure that people are doing what they said they would do, and they're not cheating you, and then there's also the problem for the agent that the boss isn't making claims on you, without a factual foundation, in order to collect penalties and so on. Anyway, so I had an explanation for block booking, which the publisher, by the way, really liked, and he said, "Well why don't you do more of this?" Well, sure. [laughter]

09-01:38:48
Burnett: Well, you didn't respond directly to that, but you kept it in mind, I imagine.

09-01:38:53
Telser: No. Yeah.

09-01:38:53
Burnett: But that is something, the principal-agent problem, that is something that becomes important in game theory, right and in economics more generally.

09-01:38:59
Telser: Sure.

09-01:39:00
Telser: It's very important, yeah. And by the way, and these are problems that people like me, who are using game theory—these are very important problems. They are also, by the way, very important problems in national defense when you're dealing with countries.

09-01:39:24
Burnett: Well, yeah, I imagine, since game theory really comes out of those kinds of strategic games, right? Well, we can maybe use this to introduce a topic in the next session, and that is, so game theory's first major client, I suppose, is the state that is interested in problems of national defense, and that is, when you need to model or game scenarios that cannot be worked out in practice without serious global consequences. And they're interested in what an economy is going to look like after a nuclear holocaust, and these kinds of things. And so, they are designing virtual economies to some degree, and those virtual microeconomics, that don't have any necessarily real-world application, God forbid.

So, what then begins to happen is that game theorists not so much, not you and not at Chicago, but game theorists start to work on, not just describing markets, or using game theory to do what you do, which is to look at how markets operate, and then use game theory to explain why; they take it a step further and enter the economy itself by designing markets, designing auctions. And so, I don't need you to answer that question right away, but let's leave that for next time as a way to talk about the advent of what's called market design by economists at other institutions.

09-01:41:27
Telser: I'll mention only one point on the defense department, or the Air Force as the patron at the RAND Corporation, and that is, although it is true that fortunately we haven't had practical experience with some of the things people are talking about today, at the RAND Corporation, there were very careful studies made of things that happened in World War II, especially the effects, for example, of strategic bombing. That was very carefully studied, and that case, as I recall, the conclusion was, it wasn't as effective as the proponents thought, or said at the time.

09-01:42:18
Burnett: Yeah. And I think John Kenneth Galbraith was involved in the first—

09-01:42:24
Telser: Pardon?

09-01:42:25
Burnett: John Kenneth Galbraith was involved in the first research, not at RAND, but actually in 1945, in the war—

09-01:42:33
Telser: Could be.

09-01:42:35
Burnett: —as part his experience with the Office of Price Administration, going over and—

09-01:42:40
Telser: I don't remember. I think there was a monograph on it, studying strategic bombing. And then, he definitely was at RAND and he was working on the stuff at the time, and I remember, he was telling us that it didn't work as well as people thought.

09-01:43:02
Burnett: Yeah. Well, let's leave that for next session, but maybe before we break today, this edited volume called *Classic Futures: Lessons from the Past for the Electronic Age* in 2000, it's somewhat of a historical anthology of literature. It's a compendium about futures trading, and so, excerpts from Frank Knight's *Risk, Uncertainty, and Profit*, work from Marshall, Holbrook Working, Keynes, Houthakker, but also more recent work, including a lot of your key, "greatest hits" on futures markets, and including the one with Higinbotham, and William Baumol. It's an extraordinary book, because it's quite slick, and it's huge, and you could see it as kind of a textbook, but I wonder about that. It's got photo plates in the middle of it, with black-and-white and color photos of the Chicago Board of Trade. Can you talk about this book and what sets it apart, and why it was done? What was the motivation behind it?

09-01:44:29
Telser: Well, I think Knight might be mentioned in the beginning. The company, the publisher, Risk Publications, is a British firm. I think it went bankrupt, but the successor owns it and they have an office in New York. My impression is that the book has done pretty well, and it's highly regarded, but I understand from reading this from business schools and so on, that it's pretty widely read and widely used. The one in particular I mention, it was somebody at the Wharton School who uses it, and I assume there probably are others. So it's a highly regarded book, and it really does tell you a lot of pertinent things and views about futures trading. I think we have something by Alfred Marshall in it, don't we?

09-01:45:49
Burnett: That's right.

09-01:45:50
Telser: Yes, Alfred Marshall, and Holbrook Working, and all the big writers on the topic.

09-01:45:58
Burnett: Yes, and so, it was to serve that purpose. It's for the folks who are going to be working in finance, people at business schools. It's used in those courses that are focused on futures markets.

09-01:46:10
Telser: Yes, so that they should have some idea about the background, how did all this start, and what did important people think about it, and what did they write about it, and so on. And the fact that it's slick and it has photographs and all of that, that wasn't due to me. It was due to the person who recruited me.

09-01:46:40
Burnett: It's really interesting, because it's the material culture of the business, and it has photo plates of the tickets or the stamps that they would use to establish trades, and that are part of these kind of contract booklets, and there's a [photo of a] kind of chamber with the traders who are standing in a circle, and they're making trades with one another, and the technology and the changes in the technology. So it's quite a remarkable book, in that sense, that it covers this survey of history, and it was interesting.

09-01:47:22
Telser: You could actually see right in front of you, an organized market, what it looks like. And I'm not sure, I don't get sales records or anything like that, but I believe it's still available.

09-01:47:47
Burnett: It is. I've checked; it's definitely still in print. It's very different from anything you had ever done before, so, it was quite interesting. Well, let's pick up next time, and talk about it in the last session.

09-01:48:02
Telser: Okay.

Interview 10: October 20, 2017

10-00:00:17
Burnett: This is Paul Burnett interviewing Doctor Lester Telser for the Economist Life Stories Project. This is our tenth and final session on October 20, 2017, and we're here in Hyde Park, Chicago. So, we're at the end of the 1990s, and it's at that point that you retire, in '98. Is that officially retired?

10-00:00:46
Telser: Yes, '98. I was officially emeritus in '98, yes.

10-00:00:54
Burnett: And, it's common practice for many professors, particularly at Chicago, for, retirement doesn't mean stopping doing what you were doing as an economist, so things continue. And, you worked towards the publication of *Core Theory in Economics: Problems and Solutions*, in 2007, and there are many different subjects in this book and different cases, but there are new topics that you become interested in as a result of the changes in the economy. This is something you talk about a lot, that the economy changes underneath the economics. And so, can you lay out for us some of the subjects that you wanted to apply core theory to, that you felt core theory could address?

10-00:02:00
Telser: Well, there were several. First of all, in this book, I made much more use of a computer than I had ever done before, and there are very complicated problems that arise in the economy in certain industries, such as producing electricity. In the case of electricity, the generators run continuously, and they produce alternating current. So that in order to adjust the supply of electricity to demand, you do not speed up or slow down a generator. The generator is always operating at exactly the same frequency, and in order to change the output of electricity, you start or stop a generator. Now, some of the generators are spinning, but they are not contributing electricity online, and the reason they're spinning at a somewhat slower rate is so that, if necessary, they can be put online more rapidly. However, the main implication of running the generation of electricity is that it poses combinatorial problems, which are ideal for core theory. In order to do that, however, you have to modify linear programming so it can handle what we call a binary variable. A binary variable is either off or on, zero or one. So when you're figuring out the least-cost way of satisfying the demand for electricity, you need a program that will figure out which generators should be running, and which should not be running. And so, that was one big problem that I studied in this book.

Then, more generally, in other industries, for example, in the automobile industry, a similar problem is posed. The unit of output in the automobile industry is an assembly line. I don't remember if we talked about this before. Well, an assembly line is set to work at a certain rate of output, at a steady rate of output, for a certain period of time. And there, too, it is similar to electricity. In these multi-product industries that I write about in this book,

you have problems very similar to the problem of generating electricity, a little more complicated, because the product variety is different. An assembly line will be producing one model of car, but then it can be changed to produce another model.

However, they generally do not mix very different models on the same assembly line. In an assembly line, the car moves along the line, and at various points components are put into the car, and then it comes out at the end a finished car. So that also raises difficult combinatorial problems. You could have one assembly line or two assembly lines, one plant or two plants, and so on. And it not only is complicated in order to make the car, but it's also complicated to arrange the correct number of various kinds of components that are going into the car as it moves along the assembly line.

So, part of the book studies multi-product firms, and it was based to some extent on what I had learned about the automobile industry. So that part of the book deals with electricity, another part deals with multi-product firms, which is a generalization of what I called before Viner Industries, except that the auto industry, if you really study it carefully in detail, isn't like the model of firms that appears in the textbook treatment under "Viner Industries." It's more complicated. And one of the points that I try to make in this book for people who will take the problem seriously is to understand that the technology changes, and has a very important effect on the economy. We have a different kind of economy as a result of this that we did not have before.

And the whole notion of an assembly line and standard parts and all of that, which required a considerable amount of planning, actually began—and I think I talk about it here—the historical background begins around 1800. In 1800, muskets were made to order. And one of the major innovations, I think it was in the US Army, is to have standard parts, so that instead of having muskets made to order, they were produced on an assembly line—or the parts were standard. The muskets were not produced on an assembly line, but the parts were standard, so it was much easier if something happened to a musket on a battlefield, for example, to replace it from an inventory of spare parts, and you didn't have to have a highly skilled expert on hand to replace weapons that had been partially damaged. So that, in this case, the inspiration really began to satisfy a practical need.

Then, the next important stage was in the production of harvesters, and International Harvester, in the middle of the nineteenth century—1830s, '40s, and '50s—built mechanized harvesting machines. Although at that time, the mode of power came from oxen pulling plows and stuff, and later the oxen were replaced by tractors. So all of these things were examples of industries where these kinds of techniques and methods of production were developed so things could be produced at lower unit costs.

I also discuss in this chapter the special problems of software. And in the standard economic model, we think of fixed costs and variable costs, and we think about a change in the variable cost, we call it marginal cost. So, the whole theory of supply and supply conditions really depends on having in mind a technology where the variable cost increases with the output of the industry. Software is different, because there, what happens is, the industry is producing something that will be used by a very large number of people, but, the cost of the thing that individuals use—that is, the cost of giving them access to the software—is virtually zero. There are some complications we can ignore, but the main point is that you cannot think of these industries in the ordinary way of "price equals marginal cost." If a firm sold software at a price equals the marginal cost, it would be bankrupt. It could not cover its total cost.

In addition, there's another kind of industry, although this is not so much in software, but it's in transportation. Passenger trains have empty seats, so that once the train is going and it's not loaded up to capacity, the incremental cost of adding a passenger is zero, so you get a more complicated situation there than is usually envisioned in the standard model. So this also poses a new problem for economists who are interested in seeing how to modify their theories to accommodate new technology. And then, another aspect of this that is, although it wasn't entirely ignored, it began to enter formal consciousness in the late nineteenth century by Thorstein Veblen, who invented the term "conspicuous consumption." But the main point, I think, the relevant point is that many consumer goods depend on a demand that comes from a lot of different people. That is, the reason that you may use or participate in some kind of a consumer good is not necessarily because you, yourself, want it, but because you do it as part of the group, so that there are social aspects to these commodities, and that's another set of problems that is a more natural area for core theory to handle.

10-00:14:37

Burnett: There's a whole bunch of material here on the table, and I want to go back to talking about electricity, and we can walk our way through. So, I think one of the things that you're pointing to are these structural features of production, for example, that there are problems of cost, and the nature of the technology dictates that there must be a continuous flow of production for it to be economic and efficient. So it can't just respond then to the variations in demand, so is that part of it? Let me reverse it and go back to talking about the standard model, which is a feature of this book. You begin and end your chapters, typically, by talking about this standard model, and the standard model is to do with supply and demand, and these questions of incorporating fixed costs into the estimates of the costs of a company. In the case of software, the marginal cost is not a function of increase in output, and so it places a burden then on the companies to properly estimate demand when they're pricing the cost of their operations. Is that what it does? So there are

these features of the technology that force us to reconsider the operations of the standard theory.

10-00:16:27

Telser: Yes. Now, for example, one major difference between automobiles and electricity is, it is prohibitively costly to store electricity. It is possible, using storage batteries, but they have a limited capacity. You could not service the demand for electricity of a city by storing it up in batteries and then releasing it as it goes along. However, for automobiles, it is possible to have an inventory of cars, and there, the problem is to try and estimate what the demand will be for different kinds of cars. So that, in the case of electricity, there's a bigger burden on the engineers to figure out how to adjust the flow of electricity to satisfy demand, and you don't have the alternative of storing electricity. To some extent, you might try to handle it by allowing, well, by allowing people to sort of subscribe to a steady flow. But electricity really isn't amenable to that. The consumer needs a steady flow, and so it's not only that the producer has to make a steady flow available, but also, the consumer has to have a steady flow because it isn't really possible for the consumer to store electricity to handle differences in the demand, or at least, only to a very limited extent. So there are other kinds of complications, and when you get into these sorts of complications, it means that more elaborate arrangements become necessary between buyers and sellers in order to accommodate their needs.

10-00:18:53

Burnett: Right. And the development of an electrical grid, this is something that, it's partly like the problem of the railroads. You've got a grid system that, a big part of it is a base load that has to be on all the time, and you deal with the variations by having these others, ones that, as you say, get switched on and off. But you're not contracting individually with each customer, or at least, the customer is not sending price signals that would enable that to be adjusted. Or even if they could, it couldn't work that way.

10-00:19:30

Telser: And there's another complication. It is much harder to control the flow of electricity through a grid than it is to control the flow of rail traffic on a rail system, because there are laws of physics that apply to the flow of electricity through a grid, and all the engineer can do is to turn on or shut off parts of the grid. But the electricity, given the part of it that's open, the electricity is going to flow through, in a sense, where it wants to go, and you have to accommodate. Things like Kirchhoff's Law operates, and if you're an electrical engineer, you can't do anything, really, about Kirchhoff's Law. It's a natural law on the flow of electricity. You have to adjust to it. That also has implications for people who think that they can figure out how to have a market in electricity, say, between a generator and a consumer, and the problem is that the electricity, on its way from *a* to *b*, might affect things going on at *c* that have no part of the contract, so there are what we call externalities that are intruding.

10-00:21:12
Burnett: Third-party problems.

10-00:21:14
Burnett: Yeah. And I think there's also a continuous energy problem with industry, as well, when you're talking about mass production, and there's a supply of energy, and shutting down the line is something that you don't want to do, and so that's something that factors into the fixed costs of operation of industry, more generally. So you are putting on the hat of the historian. You are going back and looking at these historical examples, and thinking about how the core can help elucidate some of these very important cases. They're not highly idiosyncratic; they are the bedrock of modern civilization, these industries that characterize our economy, effectively, and so you want to explore that.

When you were talking about the standard economic model, there are a number of things: "it employs assumptions"—this is on page 230—"about the nature of the technology. It assumes diminishing marginal rates of substitution between any pair of inputs," and you conclude, "Criticism of the standard model is not acceptable unless there's nothing better. Fortunately, there is something better: the theory of the core. Core theory is especially useful for showing when prices can and when they cannot clear the market." We've talked about that already. "It does so without ad hoc appeals to mysterious transaction costs." So that's a more specific—and to this end, a simple electric grid is an instructive application of core theory. So you're mentioning transaction costs, and that's Coase, right? Can you talk about this? You wanted to convince the profession that this is a better way of explaining things. So transaction costs are mysterious?

10-00:23:35
Telser: Well, there are certain circumstances, in a way. The simple case that some of these people have in mind is: I have a stack of cash, and I pay for my purchases in cash. So then when my stock of cash runs down, I have to run to the bank, make a withdrawal, and build up my stock of cash again. The reason I hold the stock of cash, or an inventory of cash, is because I can't be running continuously to the bank every time I make a purchase. And it may be at some earlier date that this was a reasonably good description of what's going on. It is no longer true now, because, for example, if you have an iPhone, in effect what you have is a portable bank. You carry it around with you, and you can use your iPhone whenever the mood strikes you to buy something, so that there really isn't the same kind of transaction costs in a modern economy as a result of inventions of new technology that would seem to be important in the past. And I would bet that if Ronald [Coase] were sitting here now, he would agree. He's a reasonable person. He created models that would be useful in studying the kinds of situations that he could see around him, and now, if the circumstances are different, then you have to change your tune.

Now, there are circumstances where there are advantages in building up an inventory, and there are storage costs. A better term would be storage costs

rather than transaction costs to explain certain phenomena, and when I say mysterious, what I had in mind is that nobody really sat down and seemed to have thought through where something that we call transaction costs really enters the picture and is worth thinking about.

10-00:26:41
Burnett: Well, it seems to be part of your interest in trying to deal with what could be called shibboleths in economics, right? That we take certain ideas, and we take them as given, even though they were developed to describe a certain set of relations, and they are taken from the singular case to be applied universally, and they acquire a life of their own. And I think others would agree with you. There's a cluster of scholars, Varouj Aivazian, Jeffrey Callen, and Susan McCracken, out of University of Toronto and McMaster University, on the most recent paper, but they've been writing about core theory and a tension between the core theory framework and Coasean transaction costs. And I think they agree with you, that Coasean efficiency breaks down in an empty core. And so they are taking this stuff up, and you should know that they have contributed an article to the latest *Encyclopedia of Economics*. The publisher eludes me right now, but it's this year, 2017. [V.A. Aivazian and J.L. Callen, "The Coase Theorem and the Theory of the Core," *Encyclopedia of Law and Economics*, 2017] There is an entry called "Transaction costs and the theory of the core." So you now have become immortalized, at least in terms of encyclopedias. [laughs]

10-00:28:21
Telser: Yeah.

10-00:28:23
Burnett: And so, you're interested in looking at important cases and using core theory to illustrate what you think is going on in these important cases that can't really be explained in terms of the traditional standard economic models.

10-00:28:45
Telser: Well now for example, if you try to describe in the standard model—one of the main effects of the computer revolution, the way I would describe it is, there has been a dramatic decrease in the cost of communications. And so it is now possible for people to communicate very rapidly at a very low cost, and we are still adjusting to all the implications of that. So if somebody, say, a hundred years ago, had talked about what would happen in an economy if we assumed exogenously that the cost of communication went down, and we go through the models and try to figure it out, that wouldn't be a very sensible way to spend your time, because nobody would be really smart enough, in say, 1917, to figure out all the things that would happen if the price of communications dropped to practically zero.

10-00:30:09
Burnett: Yeah, I think they were interested in flying vehicles. [laughs] So, there's another feature of the software revolution and the digital revolution, and that

is the tremendous increase in processing power, which has affected your profession. It's facilitated the kinds of sophisticated mathematical analyses, the analyses of huge swaths of data over long periods of time, and it's facilitated the solution of a number of different combinatorial problems that you've described.

There's another feature of that, too, and that is that, I guess, cousins of yours in the profession, game theorists, who have a common ancestor in von Neumann, have ceased to become solely the assessors of the market, standing on the side of the market and looking at how things are arranged, and have actually used their expertise to begin to structure markets and submarkets. And so that's another feature of things, to the point where, bringing the software back in, you have the automation of certain features in the economy, so the automation of trading, for example, and those are algorithms that are developed using a lot of the techniques that I think would be familiar to you. Is that what's also happening in the economy, and do you have any reflections on some of those changes?

10-00:32:05

Telser: Yeah, well, there is, a lot of computerized trading, for example, in the stock market, and how sophisticated it is, or that is, how successful it is, is not entirely clear. Now, it is now possible to have almost continuous trading of shares in a corporation. And because of the huge decrease in the cost of communication, you can have people sitting around and looking at their computer screens, and doing this, and then, some of them are tempted to say that "maybe what we can do is we can write programs that will do all of this stuff on the basis of patterns, and prices, and so on." Now, that's the kind of trading, it may be making the market liquid, but I don't think that it necessarily improves things. It may actually be making things worse. I think we might be at a stage where we're really not smart enough to have good algorithms, and we're too trusting of whatever it is that we do have.

Now, one of the things, for example, that I discuss in my book, is that when you do use algorithms to trade, you have to provide a certain amount of information to the computer in order for it to use its algorithms. For example, you might tell the computer the highest price that you're willing to pay, or the lowest price at which you're willing to sell, or how many shares you want to sell, or buy, or whatever. Now, one of the problems this raises is, it's not only whether you're telling the truth—you may be telling what you think is the truth or not— but then there is the problem, what incentive do traders have to tell the truth, given the kind of algorithms that are being used to trade? The fact that you have algorithms that are trading does open the possibility that there may be some smart traders around who can figure out ways to take advantage of it.

10-00:35:32

Burnett: To game the system.

10-00:35:33
Telser: Yeah, right. And so, there are advantages and there are disadvantages. Let me just give you a very simple example. I don't have any hard information about this, but it is interesting to watch the price of two stocks, Google and Amazon, and if you look at it, you will see that the range stays pretty constant. It changes a little bit—

10-00:36:07
Burnett: Between the two stock prices.

10-00:36:08
Telser: —between the prices, yeah. The difference in the price between the two shares stays roughly constant in that what you see is, sometimes it widens, and sometimes it narrows, and it's hard to explain that on the basis of ordinary transactions. It looks like, to me, it looks like there's something funny going on here. When you see things like that—see, now one of the terms—this is pretty old in America—it's called arbitrage. Is there some kind of arbitrage going on, and what is the basis for it? Are there people in the market who don't have anything better to do, and this is sort of an exciting way for them to gamble at a relatively small cost? And is this something we should be worried about? And there are some who would say that "it encourages the appearance of amateurs, and they might be screwing things up." And I think there was some evidence of this, in the case of commodity futures markets. So that, now that we have computers and so on, the temptations are even greater.

A little story. The Merrill Lynch broker is very well known on the floor of the exchange, and so some of these traders would say—I don't know if this is still true, this was in the days when I was more involved in this sort of stuff—"Aha, here comes a Merrill Lynch guy, so the boobs are trading," and then they would take advantage of the situation. But then the really smart guys would place their orders through Merrill Lynch. So you get into these sort of complicated little interplays, and it's not so obvious who is the boob and who is a smart fellow.

10-00:38:28
Burnett: Right. [laughs] Well, it does seem, to the lay person, that certain things, bedrock things that we took for granted as being markers of value in an economy are shifting, and just off the top of my head, I was thinking this morning: Twitter, it's been around for ten years, never turned a profit, and yet it grows. So it's not profitable, but it grows. I'm sure there are all kinds of explanations for this, and then the question is, what are they selling? They're selling a marketing platform, effectively. Tesla, its market value is bigger than General Motors. General Motors sold ten million vehicles in 2015. Tesla sells 80,000, something like that. Amazon, you were talking about the price earnings ratio of Amazon, and what, 50 cents of every new dollar spent in the United States goes through Amazon. These enormously important companies in our economy that signal something, and yet when we look at our traditional benchmarks of value, we find [that the markers appear to have shifted]—so, is

core theory a way to reexamine some of these things? Because it seems to be particularly useful for discovering the variables that are significant, whatever core theory looks at.

Telser: What you do in core theory is, you have a framework to describe the constraints, but core theory doesn't really tell you what the numbers are in these constraints, and they may be sensible or they may not be sensible. It's no more than saying that, when you write down a quadratic equation and you try to find the roots, it'll give you the roots, but you don't know if that's good or bad. It depends on what are you going to do with the result, and where the numbers come from, and so forth. I think the main thing that core theory is doing is it's trying to figure out what should be relevant, or which things you should take into account. Now how you take it into account, or where the numbers come from and which ones make sense, and how they are used and so on—to some extent, core theory does have something to say about it, but there's a lot of other things that are entering into it.

Let me give you an example in economics that I have been thinking about. Frank Knight drew a distinction in his famous book between risk and uncertainty. And risk is a measure of things that can happen that seem to have a solid foundation. So for example, if you want to know what the distribution of coin tosses will be if you toss a coin fifty times or a hundred times, you can use a theorem, the binomial theorem, and it'll tell you what the distribution of the number of outcomes will be. You get the bell-shaped curve, and in the limit, you get a normal distribution. Well, Knight calls this risk. But now you ask, "What is the probability, for example, that so and so will be the candidate of the Republican Party in the next election?" Well that's something of an entirely different caliber than what happens in coin tossing, or what happens in a lottery, or so on.

Now, for instance, as a more concrete example, there is a book written by Dubins and Savage called *How to Gamble If You Must*, very famous book. And what they are talking about is a casino, where all kinds of different probability prospects are offered to you, roulette wheels, et cetera, and for those things, the mathematician can calculate the outcomes of different strategies, and I call that artificial risk. But natural risk says, "Well, what's the probability we'll end up in a nuclear war with North Korea? Can you say anything?" There isn't any formal model of anything that you can use to describe that, and that's sort of what we're talking about here. You could have a formal model of artificial risk, but we don't, at this point, we don't have a formal model of what I would call natural risk, or that Knight calls uncertainty.

10-00:44:28

Burnett: We don't know what the game is, I suppose, if you're—

10-00:44:31
Telser: Or the rules.

10-00:44:32
Burnett: Right, if you're in the casino and they say, "Here's blackjack," you could say, "Here are the rules." You can calculate probabilities on the basis of that. But if you go in and say, "Which game is the most important according to some unknown variable?" that's what you're talking about in terms of uncertainty. So we're dealing with that right now in a changing economy. Certain things, we got blindsided. Maybe most people, most economists, got blindsided, and others saw things coming down the pipeline. But there was the Great Recession in 2008 and there's a whole bunch we could talk about right now, but this is generative of new work for you. This provoked you, in some ways. Can you talk a little bit about your encounter with this Great Recession, and how it stimulated your research in new directions?

10-00:45:42
Telser: Well, what really started me off on this was Black Monday, although, I was in Milton Friedman's Money Workshop almost from the beginning, from 1953, and through the futures trading and so on, I had an interest in macroeconomics, but it was a shock. The crash in 1987 is what really changed my mind about things and I became very interested in, or re-interested in old problems, and I actually have written a lot of things about this, and looked at it.

And one of the things, by the way, that I haven't mentioned is, a lot of the stuff that I have done appears online in the Berkeley Electronic Press site that I have [https://works.bepress.com/lester_telser/]. I think I have sixty or seventy articles on it, and most of them, maybe fifty or so, really deal with these macro issues. I was very interested in seeing what was going on causing the troubles in 2008, and it led me to think a lot about various things: the stability of the banking system, the effects of deposits shifting from one bank to another, and the huge surges and decline in the amount of credit. And in particular, in 2008, there were spectacular things that happened in the housing market that led up to it, and I found that it was similar to some of the things that led up to the Great Depression starting in the early thirties, so that credit got out of control, especially mortgage lending. There are people who disagree with me, but it seems to me that this was a major factor, and it's a major factor I think that underlay what happened in 2008.

So I started looking at credit and the role of the central bank, what can or should they do about it. Now one of the big problems in this area is uncertainty, because when you're talking about the credit market, you are concerned with whether people who borrow will be able to repay their loans. And the problem that you run into here is that a debit for somebody is a liability, but for the person who has extended the loan—in other words, the lender, regards the debt as an asset. And so what you have is something that's sort of good for one person and not good for the other person, and if

something happens to upset the relation because of all of these connections among people—borrowers and lenders and so on—when there's a crisis at one point, it can spread and screw up the whole system.

So then it becomes very important that there should be genuine understanding by the ones who are in charge of what could happen and what they should do about it. And right now, for example, it's very discouraging to read in a newspaper some of the considerations that are being discussed about who should be the new chairman of the Fed. Janet Yellen's term is over in February, so there's already all kinds of talk about, what should the Fed do and who should replace her, should she be retained. I personally think that she did a very good job, and the vice chairman, who is an old friend of mine, Stan Fischer, also did a very good job. In fact, Stan and I worked together way back once on an antitrust case, so we know each other pretty well. Anyway, these are very good people, and they're very sensible, but when you read about the way this thing is being discussed by members of Congress, I find it quite appalling.

10-00:51:14
Burnett: Do you think there's an economic literacy problem, especially when we're talking about legislators?

10-00:51:21
Telser: Yes.

10-00:51:22
Burnett: The typical qualification for a legislator, if they have any qualifications, is a law degree—I don't think that's controversial to say—is a law degree not an economics degree, not necessarily a business degree of any kind, or necessarily any solid experience with finance.

10-00:51:47
Telser: And whatever economics they do know is very superficial, and many of them haven't had any experience in business, or, the experience that they have had in business, in my opinion, should disqualify them from being—

10-00:52:09
Burnett: Some of them, yeah.

10-00:52:10
Telser: —in the government. [laughter]

10-00:52:12
Burnett: Yes, we can think of some notable examples, I think. So there's so many things that are happening. One is an economic literacy problem; another problem that we've talked about earlier is the transformation of innovations in theory that describe either new phenomena or re-describe old phenomena in new terms, the transformation of those ideas into shibboleths, into slogans, as you called them. And so, they're simplifications of the economy that then

acquire a life of their own, and so that seemed to be happening as well: financial innovations, new financial instruments, mortgage-backed securities, for example, which was a solution to an earlier problem back from the sixties. And so, the securitization of this debt that then leads to problems, and I don't want to necessarily pursue any particular track, but how do you understand some of the sloganization of some economic ideas, and how that then operates in the political economy of some of these catastrophes?

10-00:53:48

Telser: Well, it's an interesting question. Part of the problem has to do with the quality of education, what people learn about this, and the extent to which they get exposed to basic models and principles of economics as distinct from what they might pick up by watching television or Facebook or whatever. There's a certain amount of formal training that you really should have to be a useful citizen in a republic. You have to be sophisticated, and be able to distinguish among different promotional appeals that really don't have any sense. This is a very tough question. Now the principle that we have in government in the United States, or the way it used to work—or let me put it this way: The Founding Fathers of the Constitution did not have a very high opinion of the general public. This is partly because they were Englishmen, and they had some either firsthand or secondhand knowledge of events that had taken place in the United Kingdom that were pretty bad, to say the least, so there was a certain amount of distrust of letting uninformed opinion rule. And so this really creates a problem.

For example, a senator was supposed to be elected by the state legislature. A senator was not supposed to be popularly elected. The president and the vice president were supposed to be chosen by the electors, and the electors were supposed to be chosen by the state legislature. So that the idea was that the representatives would be much better informed than those who chose them, the people who voted had to be sufficiently well informed so that they could make good choices, but there was some kind of an attempt made to make sure that there was enough knowledge in the public, an emphasis on public education—all kinds of things were done to try and deal with problems that had arisen. And now, because of the decrease in the cost of communication that we were talking about and so on, and other factors, the controls that we relied on are no longer working, and you don't hear much discussion about what should be done about it.

Now there's one interesting implication of core theory in some of my work, to go back to core theory, and that is, it is not widely understood, let us say, by kids who have finished high school—even though in Illinois, a course in civics is compulsory, and maybe US history. In most cases it isn't taught very well, and most of these kids really haven't learned anything about how things actually work. For example, most legislation in Congress, until recently, was passed by huge majorities with bipartisan support. First of all, that isn't true to the same extent for the past eight or nine years, that's point number one; and

point number two, there are very few writers in the newspapers who make clear, or seem to be aware of the history and the background.

Now, I haven't read it, but I understand that [US Senator John] McCain made a very important speech on Monday trying to describe some of the basic principles that underlie the way that Congress is supposed to work. But when you look at the actual numbers, you will see, on important legislation, that there wasn't really conflict. Now, the implication of core theory is, basically, that you try to get stuff passed with the largest possible majority, and in order to do that, you have to give something to everybody. It is sort of a cooperative endeavor, and that sort of thing seems to be lost sight of. There are certain basic principles that should be held in common about what is good for the country, the kind of basic morality and principles, by the way, for example, that Frank Knight would stress when he's writing about ethics and morality and so on. And some of that, it isn't properly appreciated, even, I would say, among some professional economists.

10-01:00:36

Burnett: Well, we seem to be in an age of fracture, as the historians say, a rise in populism that goes back to the 1960s. "It's always been there," as Richard Hofstadter said, even in the fifties and earlier on. It's always been there, but it always was under some control, let's say, and that there was a fairly technocratic approach to the running of the economy, and the running of government, until the fruits of this kind of strategy that originates in the sixties comes home to roost. And you get, in the last ten years, you get people who want to blow it up, who just want the government to grind to a halt, because they will not budge a single inch on an agenda, even though there is consensus on a wide range of issues among the American people.

10-01:01:41

Telser: Well, there is a lesson in history, and that we have to be aware of, that it is possible for a minority to take control. There is a book by Solzhenitsyn, *Lenin in Zurich*, that makes that pretty plain. The Bolsheviks never had a majority in Russia, and in the French Revolution, it was similar. It is possible for a well-organized, ruthless minority to do things and get away with it.

10-01:02:22

Burnett: Especially if this cost of communication and the cost of information is extremely low, and your ability to disseminate information, or disinformation, widely, which is, as we now understand, an explicit military strategy by major military powers, such that the technology is changing the economy. It's changing strategy. It's changing politics.

10-01:02:53

Telser: And it puts a bigger—and if you want to be a good citizen, it's more costly now than it used to be. It was easier to say, "Well, let George do it." But now, [laughter] it's tougher.

10-01:03:12
Burnett: It is, and perhaps the silver lining is that there's more engagement now, regardless of one's political persuasion. There's more engagement now by the American people in their political space, and I think that that ultimately is a positive thing.

10-01:03:31
Telser: Yeah, that is, the illness may bring about the cure.

10-01:03:36
Burnett: Yeah. Let's hope. [laughs] As you said, the BE Press has kind of been the home of your publishing output in—

10-01:03:51
Telser: Recently.

10-01:03:52
Burnett: —recently, and you've uploaded a number of papers to that site, and I wanted to ask you about your paper on the von Neumann growth model. Can you talk a little bit about what this means? It's confirmation of von Neumann's theory of growth, that there is a necessary equality roughly between the growth rate and the real interest rate. [Lester Telser, "John von Neumann's Growth Theorem Confirmed by Empirical Evidence II" Unpublished 2009 https://works.bepress.com/lester_telser/] So I'm wondering if you could start by talking about, for our audience, the nature of real interest rates and what's significant about that in this story.

10-01:04:44
Telser: Well, the first point about interest rates is that the nominal interest rate—in other words, the interest rate that you see quoted in a market—takes into account what people think is going to happen to the price level. The simple idea behind it is that when you lend something to somebody, and you are expecting to be repaid, you would like to be repaid with purchasing power equivalent to what you had loaned. But if you lend somebody a dollar and the price level doubles, and they repay you with a dollar, you will only be able to buy half as much as you could've done at the time of the loan. So that, what the interest rate is supposed to do—the nominal interest rate, what it does do is, it adjusts to the expectations of the price level so that it tends to maintain the real value—that is the purchasing power—of the loan itself, as a constant.

So that means that when you look at the nominal interest rate to figure out what the real interest rate is, you have to subtract from the nominal interest rate your estimate of the rate of inflation. For instance, the nominal interest rate might be 10 percent, but the rate of inflation might be 5 percent, so 10 percent minus 5 percent is 5 percent, and that would be the real rate of interest. And what I found—actually, the way I found this was not by having read the von Neumann article or anything like that, but in studying what was going on in the macro economy after Black Monday. What I found was that the growth rate of the economy, if you measure the rate of growth of industrial

output in the economy, that was comparable to the real interest rate—in other words, the nominal interest rate adjusted for the price level.

And so there seemed to be almost equality between the two, and this is not something that you would ordinarily expect according to economic theory, and then I remembered that von Neumann had written an article. The article was published in English in 1946, in the English journal *Review of Economic Studies*, but it was translated from a lecture that von Neumann gave at Princeton, I think it was in 1932. And in that lecture, he described a model of the economy in which firms are producing outputs and buying inputs from other firms, so a process is going on. It doesn't include consumers; it only includes firms that are producing things, selling them to other firms who are using the inputs to produce other things, so that things are going back and forth, and you have technical relations between inputs and outputs. And he used, in fact, a theorem called the minimax theorem, that was derived from work he had done in the "Theory of Games" published in 1928 [An article, not the 1946 book in English, John von Neumann, 1928. "Zur Theorie der Gesellschaftspiele," *Mathematische Annalen*, v.100, 295–320].

So he used that theorem in order to deduce the result that, in equilibrium, the real interest rate would equal the real rate of growth of the economy. And here I had, entirely fortuitously—I hadn't expected this— and I see that in fact it works, and I was using monthly data, I think it was from about 1920 to 2007 or so.

10-01:09:52
Burnett: That's right, yeah.

10-01:09:53
Telser: So that's a lot of data, but with a computer, you can do this stuff fairly easily. And I found this relation to be true empirically, and having found the result, this led me to read the article. Now the article uses some fairly advanced mathematics, in fact, a modification of what is called a fixed-point theorem, that von Neumann developed. And when it was translated and published in English by the *Review of Economic Studies*, it has an appendix written by the English mathematical economist [D.G.] Champernowne. He tried to explain the von Neumann model in terms that would be easier for economists who were not mathematically sophisticated to understand.

10-01:11:33
Telser: And the appendix came right after the von Neumann article. Champernowne was pretty young in 1946. I think probably he was in his thirties or early thirties, because he had been a student of Keynes in the thirties, so he must've been pretty young, maybe barely thirty years old. And Nicky [Nicholas P.] Kaldor, who was the editor of the journal, wanted somebody to write something. A fixed-point theorem was a relatively new idea in mathematics, and I think one of the first versions of it was published around 1910. So it was pretty new even for mathematicians, and von Neumann had a fancier version

of a fixed-point theorem. Now, what is interesting about it, in addition to the fact that it predicted a relation—something that he wrote or explained in 1932—that I found seventy years or so later that he certainly would not have known about. It's a very dramatic example of a model that is verified empirically, and in fact, it's pretty close. The relation between these two numbers is astonishingly close, given the way things work in economics. Now the other thing I wanted to say is that the minimax theorem that von Neumann presented in '28 and then used in 1932, is the foundation of linear programming, which is very important, and has been responsible for huge cost savings, real cost savings in the economy. So the whole thing is really quite astonishing. I have a lot of readers for my stuff in the BE Press, literally thousands.

10-01:14:34

Burnett: So you're getting circulation for these ideas, but you are using these tools that have become so central. Linear programming shows up in pharmacokinetics. It shows up in the sciences, all over. These are extremely powerful tools that have been kind of widely disseminated through all manner of disciplines.

10-01:14:59

Telser: And I should add that, in addition to the empirical stuff, it turns out that it's not just a straightforward linear programming problem. It's actually a non-linear programming problem, and figuring it out, computing it, turns out to be difficult. It's not as straightforward as you might think. But with a computer, you can do this.

10-01:15:38

Burnett: So, and I think the growth rate and its equivalence to the real interest rate is somewhere around 2.66 percent or something, per year. And when I was reading that, and also thinking about your meditations and your research on the causes of the Great Depression. This is something we could talk about as well—I think about the post-crash solutions and our tremendous concern about inflation, and how we have this succession of quantitative easing, and the interest rates remain very low, and they've been persistently low for a very, very long time, and it's unusual, I think, in the history of economics. Is this another case of shibboleths? I mean, the concern with low inflation, for example, when TARP was being debated, there were economists like Bob Barro who warned that there might be hyperinflation, if you—

10-01:16:53

Telser: Who said that?

10-01:16:54

Burnett: Bob Barro.

10-01:16:55

Telser: He said there was going to be hyperinfla—

10-01:16:56
Burnett: That was one of his predictions, yeah.

10-01:16:59
Telser: Lately?

10-01:17:00
Burnett: No, no, at the time.

10-01:17:01
Telser: Oh, I see.

10-01:17:02
Burnett: But that was the consensus, I think, of a sector of the economics profession at the time, was that this kind of stimulus package was going to result in runaway inflation, not hyperinflation in the Weimar sense, but a high rate of inflation—and it didn't happen.

10-01:17:26
Telser: It didn't happen, right.

10-01:17:27
Burnett: So, a number of things: the crash wasn't predicted, a lot of the response of the economy to the solutions or the reaction to the crash didn't happen. And so, can you talk a little bit about—economics took a hit, I think—

10-01:17:50
Telser: Yes, they certainly did. Well, certain economists [laughs] took a hit. Now, what happened after 2008 is really very similar to what happened in the late thirties, and it's what Keynes called the liquidity trap. The problem here was that there was actually an increase in the money supply in the thirties, but there was not much of an increase in the activity of the economy until the late thirties, 1938 or so, with the onset of World War II, and huge government spending. So that's sort of the standard story for that period. Now here, what we had is a tremendous increase in the amount of liquid assets in banks. I think the last number, I think I saw this morning, it's about four trillion dollars, attributed to [US Federal Reserve Chair Ben] Bernanke, but actually, I think it was mostly [US Secretary of the Treasury, Henry Paulson].

Anyway, there's a huge increase, and yet there hasn't been a commensurate increase in real output. We are in a situation where there are a lot of people who are actually not in the labor force and are getting disability payments through Social Security, so that, in a way, the working labor force is relatively small compared to what we might think of as the total labor force. So that the unemployment rate, which at this point looks like it's on the order of 5 percent, it depends on how you count all of this other stuff. It would not be difficult to make out a case that the true unemployment rate is, in a sense, closer to 10 percent than it is to 5 percent. And so, why is this happening? What's going on?

I can quote Al Harberger on this, it's a very clever remark that he made. I think he told me—it was a couple of months ago—he said, "Well, Milton Friedman knows how much money the people were going to hold, but we don't, you and I," Al and me, "we don't really know, but Milton would know." That's sort of funny, but in a way, it is a realistic answer. We don't know why. We can think about reasons and all of that, but there is something going on here. There is sort of a desire to be safe or something that is impeding a brisker recovery. The way I would put it is that people are worried.

And this sort of thing is important, and I think that serious economics should focus on this sort of thing and try to understand what's going on. It's all very well to talk about some of these not-as-important problems—and interesting, I have not looked at the new data on the rate of growth in interest rates, but, my impression is that, since interest rates are relatively low now—I think the long-term treasury rate is about 2.8 percent—it's not out of line with the rate of growth of the economy. Although I haven't actually done, as we say, the number crunching, I don't think that the recent experience is evidence against the von Neumann theorem.

10-01:22:50

Burnett: Well, and so there are folks in the United States and in Europe who are—and this is directly relevant I think to this criticism of the conventional wisdom of let's say, the Washington Consensus, or what a lot of folks call neoliberalism, is that there's this obsession with keeping inflation low, and it has led to this kind of deflationary economics. You see that in the debates around austerity measures between Greece and the EU. But I think what you're talking about is more sociological. There's a sense that—let's say, for example, that we did have a stimulus approach to the economy. You've got people who have, in essence, dropped out of the economy in large numbers, because of disability, because of the fact that their age cohort and their skill sets are not such that they can take advantage of the real gains in certain smaller sectors of the economy, in different places. And so, labor mobility is an issue. Disability is an issue. The whole sociological framework of the labor force is a problem that, changing a single number in the macro sense, will not produce necessarily the desired outcome.

10-01:24:13

Telser: Yeah, what I think, I think that what's underlying this is not really a macro issue. I think that we are in the midst of a revolution in the technology, a computer revolution in the technology, and that we are experiencing tremendous growth in the power of computers and artificial intelligence and all of that, and we are in the process of adapting to it. And one of the ways it shows up is that new skills are required. You have to have some kind of understanding of the problem, and the standard models don't really apply here, because you have a technology where variable costs are essentially zero. What we're doing is, we're creating things that can, to some extent, substitute,

and to some extent, complement, human beings, and we have to try and figure it out, and to do it in the sort of terms based on just a naïve view of the past isn't going to work.

Telser: And here, I think, to understand the technology using core theory is really very important.

10-01:25:50

Burnett: And you did, and one of your latest pieces, the BE Press, is "Where Prices Come From," and that's exactly what you're talking about, is that core theory can help solve new economic problems created by digital computing.

10-01:26:08

Telser: I had a direct experience with one of my students who commented on this piece, and he really missed the point, and I had to make it clear to him that this isn't a variable cost, that this is what we call an avoidable cost. In other words, if you're active and you're producing, you incur this cost, but the amount that you sell depends on how well you were able to predict the acceptance of the program that you created in the population. That is, there are some companies that have been very successful, very few, and some not so successful, and you can see that the successful ones are doing very well, but this does not mean that they're hiring a lot of people relative to how well they're doing. Although of course, they are hiring more, but the increase in employment associated with the increase in the returns is not one to one.

10-01:27:35

Burnett: Right. We're still struggling with a basic message that education is crucial, and learning how to learn is crucial. So, moving off the message that working, doing manual repetitive work in a factory is not going to give you a good wage, that we're still wrestling with that. There's almost, in the current political climate, there's almost a wish fulfillment going on, that people want that sector of the economy, or that aspect of the American economy, to be great again, and it won't.

10-01:28:25

Telser: It's not that foreigners are stealing jobs or anything like that; that's not the problem. So you have to understand what the problem is before you can say anything sensible about what to do about it.

10-01:28:38

Burnett: Right. But also, we can't necessarily point people in any particular direction. We can just—I mean, when I say "we," I can talk about economists. The message from economics is what, in this climate? What message do economists need to get out? Regardless of their particular orientation about this or that theory, what's the general message that needs to come from the profession about what we need to do to prepare for this kind of transformation that you see as pretty unprecedented?

10-01:29:19
Telser: Well, it does mean, I think, a certain change in the way you think about education and training, some kind of apprenticeship system, something that would allow people—some of this is already happening, but something that would allow people to be more adaptable to what's happening, a way to train them to be more adaptable to what is happening. And that means that you have to focus more on substantive things, and not about, let us say, frivolous topics. There's a lot of stuff—an incentive for going to college, that is not based on a genuine need or interest in learning—is a waste of resources.

10-01:30:34
Burnett: Well, in this country and other industrialized countries, we've decided that the four-year college degree is this basic thing that people need to do, but we don't specify what specifically we want people to get out of higher education, for example. And in Germany, there is much more—I've talked about this in other oral histories—there is much more of an emphasis on vocational training, but I think about what you said about Frank Knight's approach to education, that he was training in people in economics, but he was also cultivating the person. He was trying to get them to think, to reach and range widely in their exploration of the world and their exploration of knowledge.

What other aspects have you taken from a long Chicago tradition? Because now, you've had this long exposure over several generations of economics at Chicago, and economics in terms of the profession generally. You study with Abba Lerner. You study with Alvin Hansen at Harvard. You almost studied with Schumpeter, [laughter] and you chased him the rest of your life. And you studied with Frank Knight, and D. Gale Johnson, and all of these figures, and learned so much from others as colleagues. What is it about your formation that you would say was most valuable in terms of your being a member, a productive member, of a profession, and a productive member of society?

10-01:32:21
Telser: Well, it's a peculiar combination of interests. It's sort of a broad interest in many different things that they kind of fit together: an interest in, let's say, formal, logical, mathematical stuff; an interest in history, so that you know something about what has been going on in the past. But it also involves practical experience. I probably had more practical experience in business than most economists. We never talked about it, but, for example, I worked for some time with a firm that imported diamonds, and so, I was somewhat involved in negotiating with people. We had customers who were manufacturers of jewelry, and we had diamonds. There was an office in Chicago, an office in New York, and there had been, before the war, an office in Antwerp where the diamond-cutting business is very strong.

And so I learned certain things. For instance, when you're negotiating, you'd have a packet of diamond—this is not really answering your question, but I'm trying to get across an idea, the kind of experience I had. You have a customer. You have a packet of diamonds, let's say twenty-five pointers, a

quarter of a carat, maybe there's ten of them in the package, and you quote a price to the customer. Now one of the things that you learn is, you actually never argue about the price. It would be considered insulting to tell somebody, "Your price is too high," or anything like that. That's not the way it works. The way it works is, you would say, "Well, why don't we split this into two parts. Here, I'll take this part, and I'll give you this much for it. Would that be okay?" In other words, you sort of start negotiating on the terms of the agreement. There's a certain protocol that's involved that is very different than the standard way people in an economics course would learn about how prices are. For a lot of economists, the only thing they know about the market, let's say, is when they go to the grocery store, and let's say they buy a loaf of bread or a gallon of milk. They don't know anything about the economy in many cases.

So that there's all kinds of combinations of things that go into you that somehow has an effect. But there's no doubt that there are outstanding teachers; in my case, I have had outstanding teachers that have had a very big effect on me, and so, if you ask, "Well, what do we need?" We need more outstanding teachers. But there has to be more recognition. Well, you can educate people, people can learn, but it has to be appropriate and suitable. The learning has to be appropriate and suitable for the person, and there also has to be a desire for learning by the person. And all of this has to sort of work together, and a lot of these mechanical things and advice don't really seem to take that into account. You tend to hear slogans. It's not very often that you get in the public thoughtful discussion of this sort of thing. There are very few, for example, in politics now, or in leadership positions now, that really take a thoughtful position and have an audience for it.

10-01:37:22
Burnett: Yeah, you need both, don't you?

10-01:37:24
Telser: Yeah.

10-01:37:25
Burnett: How does one teach curiosity? You were also a teacher for many, many years, and we've talked about your use of stories or film. You'd use film or book plots, and *Treasure of the Sierra Madre* is one of them, *Prizzi's Honor* is another, and you use them to engage students, because you are facing an uphill battle with, not only teaching your students in your classes, but in teaching the economics profession about what you're doing, because some of it is enormously complicated. And so, I know from reading your texts how clear and important the theory of the core is, and you would take great care to explain why it's important and what it does, and to get people excited about it. But it's a struggle, and it's something that you are faced with when you're trying to move any group of people in a new direction.

10-01:38:45

Telser: Yeah. I think that students that are really interested in the subject, the question is whether they come to you and they already have the interest, and you can stimulate it—and of course, it's also possible that they have the interest and you kill it by the way you mishandle it, but a lot really does depend on the quality and the interest of the students. You try to arouse their interest, and you don't want to be boring, and you have to have a pretty good idea of what it is that you want to teach, and this varies. In my experience, some people in teaching were very good at this, and others were very bad at it. It's sort of like platitudes. I don't have any smart answers or anything to offer.

10-01:40:12

Burnett: But isn't that the value in and of itself? What's exciting, I think, is when someone says the conventional wisdom is wrong, what you always thought was true is not quite true in different cases, and to complicate something. That has tremendous value, and I think that that's a climate that you grew up in intellectually, dealing with these kinds of conventional stories, and it's changed over the decades. You were concerned with assumptions about speculators and assumptions about monopoly, and now that some of the sort of more Chicago-like ideas have become more popular and have become ingrained, they've become important narratives of their own, on their own, and so you start poking at those, as well. [laughs]

10-01:41:12

Telser: Yeah, they become slogans. Well, I think one of the interesting—you can try to tell people this, but I don't know how successful it is—that when you go out, you will observe things in the economy, or things that are happening around you, and then it could lead you to think about it. Here is a sort of a simpleminded example, and in fact, I wrote a note about it: There are coins of different denominations, and then, the facile answer is, well, two nickels are a perfect substitute for a dime. Well, why do we have both dimes and nickels if two nickels are perfect substitutes? Why do we have these denominations? Is there any way we can explain this? Here, it doesn't seem like it's a big economic problem, but it's a puzzle. Why do we have this?

Well, it turns out that there is an explanation for it, and it was thought about in a different connection by a French mathematician, Bachet, and it was called the *problème de Bachet*, and it had to do with a two-pan balance. You have an unknown object that you're trying to weigh, and you have the known weights in the other pan, so what he wanted to know is, what's the smallest number of weights you would want to use in order to weigh this unknown object? Well, it turns out that it is a problem in number theory. If you can put the stuff in one pan, and the weights in the other pan, it turns out, the weights should be powers of two: one, two, four, eight, sixteen, thirty-two. However, if you can put the weights in both pans, not just in one pan, so you can put a known weight in the other pan, then you can get away with a smaller number, powers of three: one, three, nine, twenty-seven, eighty-one, 243. And what is remarkable is that, if you look at the actual denominations for example, of US

currency, it's not that far off from the *problème de Bachet* with weights in both pans. So you think, why do you do it this way? Well, because if you assume that a weight is costly, then it is to your advantage to do the job to find the least-cost way of doing the job, and there are certain denominations that would serve the purpose.

10-01:44:53
Burnett: And there's a path dependency to things that represent solidity and stability, so, we have a hard time retiring our pennies. They have no economic purpose today, and that cost, it's two cents' worth of copper to make a single penny, and so it's completely uneconomic at this point, but we are wedded to these, and we do gradually phase things out, but as a sign of stability, that increases the path dependency of the way things are done in that way. I did want to ask you, back to this question of the Great Recession, and before everything had fully unfolded, you wrote a piece called "The Fed's Real Job," and I'm wondering if you could talk about what stimulated that, and speaking of slogans, what you are reacting to, with respect to—it has to do with your research on the Great Depression.

10-01:45:54
Telser: Well, the thing that struck me is what led to that. "The Fed's Real Job" came out before 2008; I think it was 2007.

10-01:46:08
Burnett: That's right, yeah.

10-01:46:09
Telser: And, I had been interested in the Great Depression, going over it again, as a result of Black Monday, so I was reading about it and reading what happened. And what struck me was the impact on the economy of bank failures, and that what should've been done, and wasn't done, would've been either to—well, it would've been possible to have prevented the failure of the banking system, and one way of doing it, for example, would've been to increase the importance, let's say, of postal savings accounts. That is, instead of just letting the bank collapse and leaving people high and dry, it might've been possible to use—I mean, it seemed to me that the collapse of the banking system, and leaving people with a loss of savings, was a very serious problem, a very concrete, serious problem, and you can't really think of it in terms of increasing the money supply and all of that stuff.

The way to think about it, it seemed to me—and this is also, by the way, the way Henry Thornton and Balogh, who wrote about the money market in London in the nineteenth century—that what you really want to do is to have a stable banking system. If the Fed had been ready and willing to bail out the banks without the kind of restrictions that they were subject to, it could've helped. See, for example, because of the experience during the Civil War, when the money supply was increased in both the North and the South in a sort of a tricky way, in order to finance the war, but that would avoid putting

the blame on the government, what they did is, they had the money supply actually issued by the government in exchange for, in effect, lending to the government. So it wasn't as if the government was actually increasing the money supply; they were using sort of not-very-sound bonds in order to do this.

So what happened is, in the Federal Reserve Act [1913], in order to prevent that, they had a gold reserve requirement. They said the liabilities of the Federal Reserve Bank, which is, in effect, the money supply of the country, had to be backed by a 40 percent gold reserve, and this is a legacy of what happened during the Civil War. But by having the 40 percent gold reserve, it was almost impossible for the Fed to conduct open market operations, because if they tried to buy, let's say, government bonds by giving credit to the US government at the Federal Reserve banks, they would have to have forty cents in gold for every dollar that the money supply increased. So we had this kind of a restriction. Then the other problem that we had was that they couldn't lend to the banks unless the banks had sound security. Well of course, when you're in the Great Depression and there's a lot of failures going on, the banks that have sound security aren't the ones that have to borrow from the Fed. It's the ones that are in trouble that have to borrow from the Fed, so that we had a sort of a perverse system operating on the Federal Reserve, partly as the legacy of the way the Civil War had been financed.

And then on top of it, what aggravated this, given the gold reserve system that we had, is that both France and Belgium, for their own reasons, caused a huge drain on the gold reserves of the UK, the US, and Germany. So the Bank of England had to go off the gold standard, and credit became very tight in the US because of losing gold to these two countries. So the problem seemed to be that you have to have a way of keeping the banks afloat under circumstances that would seem difficult, and this, actually, was a situation that came up in 2008. There were some major banks, one of them was bailed out, but another one, I think Lehman Brothers, was bailed out and I forget the name. The other one, one of them, was allowed—

10-01:53:02
Burnett: AIG.

10-01:53:03
Telser: —almost to go under, and one of them was bailed out, and there was a lot of criticism of the Fed for bailing out and saving one of them. And then, of course, and then one of the big insurance companies was on the verge of collapsing, and the government saved them. Well, they actually did, in my opinion, the right thing and they saved the day. But, in the early thirties, during the beginning of the Great Depression, they didn't do the right thing. And so, banks went under and everything just collapsed. There was a huge increase in the number of bank failures, and a loss to the public of assets, so that that had an effect that took decades to recover.

10-01:54:04
Burnett: The emphasis of Milton Friedman—I think it's, yeah, in Milton Friedman's work—is the contraction of the money supply is the operating problem, right, in that period, and you challenge that to some degree. The emphasis is a bit different, so it's the solvency of the banks, and that that's the real role of the Federal Reserve, which had implications when it came to the 2008 crash. I think there were—there are still, today—folks who said, "We should've let these banks twist in the wind"— [laughs] And so there was sentiment about the moral hazard of the kind of lending practices that they were undertaking, so that there needed to be some kind of accountability. And there's this moral issue that people have with it, but there are real practical considerations for the health of the economy overall in the solvency of banks. That begs the question of then, in exchange for this guarantee for banks, what can the public expect in return?

10-01:55:15
Telser: Well, I think that what they expect is that we're not going to run into the same problem again. And the argument that was given to criticize the banks, if you think about it—and of course, I don't know if people did think about it—they said, "Well, the public is being stuck with the cost of bailing out these fat, Wall Street bankers." But what they're not thinking about is, well, the cost to the public if they hadn't done this would've been far greater. The notion that you're subsidizing or giving things to people is not the right way to think about it. In fact, one of the points, and an economist I mentioned earlier, Henry Thornton, writing in 1804 or '05 or so said, "The most important thing to think about"—he didn't use the term "central bank;" I think he was saying "Bank of England." By the way, the Bank of England was privately owned. I don't know at what point it became some kind of a government entity, but anyway, it was a private bank. But the main point that Thornton made is, the Bank of England is not motivated by the same kinds of considerations as a private banker. Thornton, himself, by the way, was a private banker. But he said the purpose of the Bank of England—in a sense, in putting it in modern terms—the purpose of the Bank of England is to keep things going and prevent collapse.

10-01:57:09
Burnett: A lender of last resort.

10-01:57:10
Telser: The lender of last resort, exactly. And in fact, at one point, I think this came up at the beginning of the Napoleonic Wars around 1790-something. The banks and the private banks, under the gold reserve system, you could, if you had a gold certificate, you could withdraw gold from the Bank of England. The gold certificates were redeemable in gold, and the bankers got together with those who ran the Bank of England, and they said, "We are suspending convertibility," which meant that Britain went off the gold standard, and I don't think they went back onto the gold standard until about 1820. So they were off the gold standard for more than twenty years, until the wars had been

over for a long time. You have to have—well, it goes back to what I said about Greenspan. You have to have people who are courageous and willing to do what is right, and know what is right, which then goes back to the question that you were raising earlier. You have to somehow teach people. They have to learn. Morality and all of that, and judgment, these are things that have to be acquired somehow, or taught.

10-01:58:56
Burnett: Right. And for the general public to acquire these things is a challenge and is going to be a challenge as something we face.

10-01:59:07
Telser: I read, I'm a regular reader and have been a regular reader of the *New York Times* for decades, or scores, and you don't really see columnists saying this sort of thing, for instance, about what would be the cost to the public if we had just let Lehman Brothers go under. Why should the public bail out these Wall Street banks? And even now, there's a lot of discussion about revoking—or what do they call it, not revoking—and there was legislation—

10-01:59:56
Burnett: On Dodd-Frank, you mean.

10-01:59:57
Telser: Yeah, and so there's a lot of talk about, "No, that's bad; we should get rid"—I don't know. These people don't seem to understand that the banking system is a very different creature in the economy than regular business firms. Somehow, we want private banks. We don't want to have a government bank, and the reason we don't want a government bank is, we already know from experience that when the government is in charge of doling out credit, you have a very serious problem in maintaining a free society. So you have to think about all kinds of things, and yes, you could have a private banking system, but you also have to have the right kind of rules.

10-02:00:55
Burnett: This is a theme that's come up repeatedly – the right kind of rules. So, you are not against rule setting; in fact, you see them as crucial to the proper structuring of markets, the spaces in which a kind of free market can operate in a somewhat circumscribed sense. So with Dodd-Frank, there is some regulation. Certain things are still outside of that. Derivatives are still not regulated in that sense; they were outside of the Dodd-Frank provisions, as I understand it; maybe that's not right.

10-02:01:31
Telser: I don't know how long that's going to last. I personally think that, well, see, it really depends. If the derivatives were not very important and not widely used, and the people who did use them seemed to know what they were doing, and so forth and so on, then you could leave it alone. But when they begin to take on some of these public characteristics, and are a source of danger, then I think regulation is appropriate. For example, there is forward trading among

business firms, and it's just subject to the normal rules of commerce. There's nothing special about it. But in an organized market where the public can trade via members of the exchange and brokers and so on, then there is a public interest, and the exchanges recognize this, and there is an appropriate set of rules. And with some of these derivatives, my understanding is that there are some places where derivatives are traded on an organized exchange, but not in the US, and I don't know what goes on in some of these places where they allegedly have an organized exchange in derivatives.

A derivative is simply another kind of a contract that's sort of like a futures contract, except that, instead of being tied to some kind of a physical commodity, or something else like a futures contract, a derivative could be derived to a futures contract, so you get sort of a multiplier effect, a liquidity effect. It's like spreading the amount of margin that you need in a spread, because you have two contracts that offset each other, and the prices move in the same direction, so the volatility is reduced, you get a lot more leverage in a spread than you would in an individual futures contract, and the leverage of a derivative on a futures contract would be even bigger than the leverage of a spread. You begin to get sort of a multiplier effect. So if there's a lot of derivatives, and you have this high leverage, and something goes under, you can have big effects, and that could be dangerous.

10-02:04:46

Burnett: But as you say, rules are important, and now we're facing deregulation of some of these sectors, are people—I think Dodd-Frank was on the chopping block in the 2016 election, and I don't know how far they were able to move on that. The party in power now seems to be having trouble mobilizing its agenda, or fulfilling it. But this is something that we're dealing with, and your work over your career has been to try and understand the patterns in markets such that we understand reasons why there are certain kinds of rules, certain kinds of structures, and to reexamine those, and think critically about them.

10-02:05:39

Telser: Yeah. It isn't that rules are bad. There are good rules and there are bad rules. You have to figure out what are the good rules, what are the bad rules, and what do we need, and so on.

10-02:05:54

Burnett: Right. And how do we make this part of a larger public conversation that is informed and effective? There's one other piece of your story that I think is worth talking about, and it's come up a couple of times, and that is the importance of Sylvia in your life. And so, if we can pivot to talk a little bit about Sylvia, and you've acknowledged her in your books as someone who is an astute editor, and contributor to the process of your work. Can you talk a little bit about Sylvia and your family? We haven't touched on that in a while.

10-02:06:42
Telser: Well, I don't think I would've been as successful as I have been without her. She has been absolutely essential and necessary for me, and she is a remarkable person. She has very good judgment. I rely on her views on things very much, and she has prevented me from doing foolish things, and she has encouraged me to do sensible things. I wouldn't be here if not for Sylvia, let's put it that way.

10-02:07:32
Burnett: Yeah. And you have two kids, and—

10-02:07:35
Telser: Right, a son—

10-02:07:35
Burnett: —how many—

10-02:07:36
Telser: —and a daughter.

10-02:07:36
Burnett: And how many grandkids now?

10-02:07:38
Telser: Two. My daughter has two daughters; my son has no children. I have to say that there is a certain, at least, in my case and I think in others—it's very hard on kids if they have an academic parent, because there are things that—although it wasn't as much of a burden I think as with some, but we did move around. There was one year I spent at Yale at the Cowles Foundation, and then another year, and I think that was a more difficult year, that I spent in Belgium at CORE, in Louvain, or as they would say, "Luhvin." [sic] We had a choice. We could've been in Brussels, and then I would have commuted, and they would've been in an English-speaking school, but we didn't do that. We lived in Louvain, and the kids went to a French-speaking school, in fact, a Catholic school named Virgo Sapiens, the wise virgins. Now let's see, Tamar was, let's see. I guess she was six or so. Wait a minute, what grade? I don't remember now, first or second grade, and Joshua was older. It was harder on her. Some of the kids were nice, but I have to say that, at certainly there, they were not as nice to visitors as we are to foreign visitors in the US. And there were other foreign visitors at CORE, not just me, others, so that it was a very interesting experience, but I thought it was difficult for the kids. But they survived.

10-02:10:20
Burnett: And they learned, yeah, and they learned from new experiences—

10-02:10:26
Telser: So it gave them, I guess—well, Joshua, his French is pretty good and he picked up other languages. I'm not sure how about Tamar's French. We haven't tested it. [laughter] We don't speak to each other in French. [laughter]

And the girls are, let's see, twenty and twenty-two, so they're both in college now.

10-02:10:59
Burnett: Right. So the education process continues. I want to thank you for taking the time to speak with us; it's been a real learning experience.

10-02:11:11
Telser: You're very welcome.

10-02:11:12
Burnett: I wish we had twenty more hours, but, twenty is a good start, isn't it? [laughter]

10-02:11:22
Telser: Yeah.

[End of Interview]