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University of California
Berkeley, California

George Leitmann: Engineering Science, Risk, and Relationships at UC Berkeley and Beyond

Interviews conducted by
Paul Burnett
in 2018 and 2019

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Interview History

There are several reasons for conducting a life history of this size. First, the sheer length and density of a life lived demands it. Second, we look to individual narrators as witnesses to important and complicated historical events. We want them to speak about what they have seen, experienced, suffered, and accomplished. We want them to tell us something about the inexplicable, the invisible, or even the conventional and overwritten.

What do you learn when things fall apart? One of the themes that emerged clearly before we started filming at the beginning of 2018 was risk. George's life until the age of twelve or so had been pretty idyllic. Then everything got turned upside down, and a young boy had to become a man very quickly. The catastrophe seemed highly improbable in the prosperous and stable Vienna of the 1930s. His elders told him so. They joked about it. Then he saw the man that everyone was talking about drive into town, and he saw the other boys marching with crisp uniforms that had been brought in on trucks or out of hiding. His home was stolen and his family were split apart. With the adaptability of youth, George escaped with some of his family and he developed a new version of stability and relative happiness in a high school in New York. But he turned again to a world of danger by joining the US Army combat engineers in World War II. Combat engineers rebuild bridges and roads destroyed by a retreating army, and George performed reconnaissance for the engineers. In other words, he was in front of the group that was in front of an advancing army, often behind enemy lines, witnessing again and again the fates of the less fortunate. Risk.

I am a historian of science, and the bread and butter of my area of research is the relationship between the social and the technical, the study of the institutions, the people, and the politics that make knowledge. It has seldom been so clear to me the relationship between a scientist's lived experience and their research. After the war and a master's degree in physics, George was recruited to work on the theoretical foundations of rocket research for the US Navy. He turned this experience and knowledge into seminal contributions to something called control theory, basically the mathematical rules that govern a system in a defined state of optimality or stability. This sounds very abstract, but George's research ends up being used in rocket research, economics, fisheries management, seismic stabilization, wind shear of aircraft, AI, and many other areas. Many of our society's systems are organized around probability, what is most likely to happen or not happen. There is a danger in this design that George has lived before. One key element of his life's work is to account and control for highly improbable and catastrophic threats to a system, any system. Rarely has such an abstract theoretical research trajectory been so deeply informed by such a visceral experience of danger and catastrophe. This was one of the most striking themes of these interviews: plan for the improbable and the terrible. Don't look away.

Finally, and because of all of the above, we want narrators of these long life histories to be wise. When I asked George to be wise, he would usually shrug and say he was no expert, even when he was. So that was another piece of wisdom: be humble. We are not so much individuals as people who stand and fall by our communities. When we were planning our interview sessions together, I was constantly trying to get a handle on the vast range of his research interests, or to understand how to approach the monumental and global tragedies to which he was an important

witness, and George would always come back to people. Did he mention this person? Did he give enough credit or time to that person? Other people matter. Family matters. Family is bigger than family. Other people first. This is his way of living, and his secular way of repairing the world, about whose broken state he always worries. And this is one reason George and I both suspect he has been so celebrated and decorated by his peers, his colleagues, his country and many others. He stands as an example of how to move through difficult times and face problems without being consumed by them, to reach a state of gratitude and qualified happiness for the complicated gift of being part of something grander than oneself.

Paul Burnett, Berkeley, CA, 2019

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George Leitmann



Photo courtesy of Paul Burnett, 2018

George Leitmann is Professor Emeritus of Engineering Science at UC Berkeley. Born in Vienna, Leitmann's family immigrated to the United States in 1940. Dr. Leitmann subsequently fought with distinction in World War II for the US Army's combat engineers. After the war, he worked for the Counter Intelligence Corps where, among other duties, he was an interrogator for the Nuremberg Trials. After studying physics at Columbia University, Dr. Leitmann worked at the Naval Ordnance Test Station – China Lake, where he rose to become the head of aeroballistics. He completed his PhD in 1956 in engineering science and was hired immediately as a professor at UC Berkeley's College of Engineering until his retirement in 1991. Dr. Leitmann helped found the field of control theory, which then branched into seminal contributions to ballistics, game theory, ecological systems, economics, seismic research, wind shear, and counterterrorism, among other subjects. His 60-year career at UC Berkeley was also marked by academic service, including as the university's first ombudsman during the unrest around campus in 1969, service on the budget committee, numerous associate deanships for the College of Engineering, and as the head of multiple campus-wide programs.

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Interview 1: February 12, 2018

01-00:00:18

Burnett:

This is Paul Burnett interviewing George Leitmann for the university history series. It is our first session, and we're here in Berkeley, California. It is February 12, 2018. In oral histories, it's customary to begin at the beginning. I think it would be great to fill in as much detail as we can about your ancestry. Tell us about your parents, your grandparents, and if possible, your great grandparents.

01-00:00:58

Leitmann:

Very good. I was born on the twenty-fourth of May, 1925. It was a Sunday morning. Five o'clock, I was told. My parents were Josef and Stella Leitmann. My mother's maiden name was Fischer. They came, I think, almost every branch of the family, from a completely assimilated Jewish family for most of the generations that I dealt with, which is really primarily my two grandmothers. The attachment to religion was only two ways. They did celebrate the High Holidays, but none of the other ones, really. Particularly my father was—he was probably already an agnostic, given his world war experiences—so we felt we were Austrians. Now, of course, the previous generations, for the most part—my parents, my grandparents, and certainly before that—were citizens of the Austro-Hungarian Empire, which, I guess by 1800, was a small part of the real empire—Maria Theresa. They felt, even though they weren't all born in Vienna, that they were truly Austrians, a multinational state, of course. The monarchy—I don't know how many nationalities, but it was probably dozens, and all the major religions.

So we felt at home. My father was the oldest child of five, of whom one had died as a baby, born in 1894, to Alexander, my paternal grandfather, and his wife, Cecilia, sometimes called Cevia for some reason. I think it was probably the Polish name. They came from the Austrian part of Poland, which abutted the Russian Empire directly. In fact, the capital of that province was Krakow, and it was directly on the Russian border. So it was a military zone for most of that period, given the relations with Russia. My grandfather, Alexander, obviously started the military career very early. His family—in particular, he had two older brothers, and his father—had immigrated to Great Britain, in probably the 1870s or 1880s. So he was the only one of that part of the family that remained in the Austro-Hungarian Empire. He started working for the military, starting as a non-commissioned officer. He was born in 1864, so this must have been probably in the 1880s that he started this military career. I looked at his record. He went quickly to sergeant, and then was advanced to second lieutenant, first lieutenant, major, and finally, his highest rank was lieutenant colonel.

Now, it's interesting. I looked this up in connection with the interview, that the administrative services of the military had civilian ranks, as well as military ranks, or at least civilian titles. For example, medical officers were

called *Arzt*—*Oberarzt*, *Unterarzt*—and then had the equivalent military rank. So my grandfather's civilian rank was *Oberrechnungsrat* second class. If it had been first class, he would have been a full colonel. He was a very patriotic monarchist, even when I met him. I don't remember this, because he died before I was four years old. He was a true patriot. In fact, he was decorated with the highest honor of the empire that was available to their citizens, not the crown princes or people like that. The grandiose title of that medal was the Golden Cross of Merit with Crown, on the ribbon of the Medal of Valor. I have it. The family preserved it, and we were able to bring it with us.

I don't know very much about my grandmother's family. In fact, I know almost nothing. She must have come from the same part of the empire, because they were obviously married while he was probably a sergeant. They had four children—well, five actually. Four girls, and my father, who was the oldest, born in 1894. One of the girls died as a baby, so my father then had three surviving sisters: Aunt Rose, Aunt Adele, and Aunt Gisele. It's interesting that the two middle sisters—in other words, Aunt Adele, as she later got to be mine—and Gisele—married two distant cousins from the United States, from a family, the Kinspel family, from California. I think maybe that wasn't so unusual. They had come to the United States in the mid-1800s.

01-00:07:33

Burnett:

Eighteen fifties, I think?

01-00:07:34

Leitmann:

Around there. It was all very obvious, because that was, of course, the emancipation in Europe. Those Jews who saw the pogroms going on in Russia and places like that, they wanted to leave. That family did very well, the Kinspels. They owned land in the Central Valley, and they also operated—I don't know for how long—a stagecoach between Fresno and Bakersfield. They sold that property—I never met Aunt Adele's husband—he died before I was born—and moved to San Francisco, I think maybe just in time for the earthquake. Gisele, the youngest of my father's sisters, died in the flu epidemic at the end of the First World War. Aunt Adele and Aunt Rose, who, of course, stayed in Vienna, were my two aunts. Aunt Adele and I hadn't met—except indirectly. She enrolled me in the United States Red Cross, and for every birthday during that four or five-year period, I got a gold five-dollar piece. I don't know what happened to those.

01-00:09:10

Burnett:

So your aunts moved out to California to be with the Kinspels, obviously. They met. Because there were family connections, they would visit—

01-00:09:21

Leitmann:

Very distant cousins, I understand.

01-00:09:24

Burnett:

And they would come and visit. You got to know their—

01-00:09:26

Leitmann:

They actually went to Europe to find brides. That's the story I got. This is all secondhand, of course. The Kinspel who married the youngest one, Gisele—of course they didn't have any children, because she died before that period, 1917, something like that. Aunt Adele, who married another Kinspel, her husband also died before I had a chance to meet him, so I did not know him. Aunt Adele and her husband had two sons, my first cousins. Charlie, who stayed home, was a mama's boy, and Alfred, cousin Al, who was one of my favorite cousins. In fact, he died, I think, maybe seven or eight years ago, at the age of ninety-four, and I was the executor of his estate four or five years prior to that already. He had retired to Long Beach.

That leaves Aunt Rose, who stayed in Vienna. She married Uncle Alfred—we called him Fritz—Gorwitz. They lived in a very nice house in the amusement park of the Prater, which is the huge green area, the emperor's riding area. A favorite place of mine, of course, because, A, they had a candy store with it, sort of a delicatessen candy store, and more importantly, they had three children. Kurt, who was the youngest—he was younger than I was—and two girls, Martha and Kay. Martha was the middle one. She was about a year older than I was, and Kay, cousin Kay. I always had to be beautifully dressed and groomed when I visited there, and that never lasted more than five minutes, because they had really a place of freedom for me, and I always loved to go there, in addition to, of course, the candy.

01-00:11:55

Burnett:

They were feral children? They were going out and then getting in trouble?

01-00:11:58

Leitmann:

Oh, yeah, and particularly Martha turned out to be a real tomboy. We were the terror of the neighborhood. As soon as I was going to be picked up, of course, I got re-dressed and everything and cleaned up. Just a wonderful place. My mother took me to the Prater quite often, because we lived about maybe fifteen or twenty minutes walking distance from there.

01-00:12:24

Burnett:

And that's a park, the Prater?

01-00:12:25

Leitmann:

The Prater is a huge park. It's where the emperor and the nobility went horseback riding. They had trails and meadows. All along that middle alley, with all the big trees, there were coffeehouses and restaurants and all kinds of nice—there was a stadium there. Then the amusement park was part of that. It was a very complete amusement park. It's known worldwide because of its giant Ferris wheel, of course. That was the entrance to the park. And there were merry-go-rounds there. There was a village of little people. They had their own post office and their own railroad in the park. It was a wonderful

place. The first part of my life, until 1938, I was a very happy child. For example, the first three or four years, my father, who had been a volunteer in the First World War—was wounded twice on the Serbian front. After the war, went to, I think, a commercial college, and became an accountant. He worked for a government concern. I think it was even a military concern, because I know that he had contact with the Afghan minister of war. I remember that he showed me once, that once he had communicated with him—that Afghan minister had, I guess, come to Austria to buy arms—the return letter said that, unfortunately, the minister had just been hanged. My father was a stamp collector, so he kept all these things, you see.

He was an avid fisherman. I was taught swimming before I was three years old, because he used to take me on his boat. People didn't wear life vests in those days. When I was a little older, in fact, I was drafted to row his boat at five o'clock in the morning. So I never became a fisherman, also because I didn't know how to take the hooks out. So I didn't. It was a happy time, until about 1931 or two. We used to go to—it's a part of Austria which is, I guess, northeast of Vienna—a little valley and stream that was a tributary of the Danube, where my father did his fishing. We usually took quarters in a farm. My father would commute. He would come up by train on the weekend, and then go back. It was a happy time. I have no bad experiences. I had my first girlfriend at the age of three or four. There are pictures available of that. It was great. That continued, actually, really almost to the end.

I was an indirect witness to many events that jump back into my mind. Let me, before I go to that period, say a few words about my mother's family. She married one of the brothers, the Fischer brothers, who had an apartment house and a dry goods clothing store, a very large one. It took up the whole corner of the apartment house on the ground floor. They came from southern Hungary, I think a city called Szeged. My mother, of course, was born in Vienna, but my grandmother, her mother, always lived with us because she had inherited that apartment house when her husband died, one of the two Fischer brothers. She had already, probably when they lived there, converted two of the regular apartments into a very large apartment on the first floor—not the ground floor, but the first floor—of that apartment house. I think it was four-story apartment house, probably from the early 1800s. Still, in a very convenient place. We lived half a block away from a large open-air market. The clothing store, of course, didn't exist anymore. It had been taken over by a paint and chemical supply place. I still remember the name of the people who owned it. Their name was Zalud. Some of these things come back to me.

It was a very nice apartment. Had a small living area—what we would call, I guess, the family room—next to the kitchen. We had two hallways, because the public hallways had become hallways in the apartment. So we had an indoor toilet, very important, because in those days, the toilets were in the hallway outside the apartments. We were the only apartment with a private toilet, and we had a very large bathroom. Important to my father, because he

was an amateur photographer and developed his own film, and he had his equipment in there. He always burned holes in his underwear because he was doing this, developing in his underwear. My mother, because she had a bad experience one summer when she was a child—when they went to the fish market in Italy, saw the worms crawling out of the fish and never touched another piece of fish, almost until the end, because she was fooled at the end, but certainly for her first ninety years. My father would cheat a little bit sometime. If he didn't catch fish, he would buy them from the other fishermen, bring them home live, and dump them into our bathtub. My poor mother not only had to live with that, but also prepare the fish, of course.

We did have a maid, who did some of the cooking, and we had two maids that came once a month to clean the whole apartment. I still remember them wearing felt slippers to polish the parquet floors. It was that kind of thing: high ceilings, double doors that were the whole height, with purple curtains, velvet curtains and all that. The formal room, which was used very little, was the dining room/living room. It had two of those double windows, with balconies, and a grand piano, where I took my piano lessons five years and didn't learn anything but scales. My grandmother's part of the apartment was far away from the rest of the apartment, the rest of the rooms. The entry hall that led to the kitchen had two double windows, one of which had been converted into a cage for my father's little parrots, budgies. I think there were about thirty of them. One tame one—I guess the mother had died—little blue and white one, and she always stayed under the furniture until we came home. Then she would hop up and give us a kiss. I remember that. We also had, when I was the age that I had begun to talk about, a dog, a wire-haired terrier. It's interesting, because I was able to bring along a Steiff toy of a wire-hair terrier, which is downstairs in my daughter's study.

To complete my mother's family, she had an older brother and an older sister. Brother was the oldest, Uncle Paul. Her sister's name, Elizabeth. Elizabeth had married a retired captain in the Austrian cavalry. So even in those days, Jews could have a career of that type. It wasn't possible in Germany. Franz Josef was a fairly liberal emperor. They had a daughter, my cousin Maria—Marie, in those days—who was seven years older than I was, I think born in 1917. I know this because I just looked at her obituary. She became a child actress first, with the Max Reinhardt School of Acting, which had its own theater group. Then went to the academy, and also to a dancing school. I won't jump ahead now, because that will be part of the regular story. They lived in a better part of Vienna than we did. They always had higher pretensions, I guess is the word I would use. Uncle Max, who on occasion couldn't pay all his bills—my father sort of was the paterfamilias for the whole family. He would cover his checks, I remember that. They lived in one of those fancy apartment houses, with an elevator through the middle of the apartment—outdoor elevator, with steps on both sides. Over the front door of their apartment, my uncle's crossed sabers hung. I remember that very well. And he smoked cigars, and he was bald.

01-00:23:09

Burnett:

He was a dramatic character in your childhood?

01-00:23:11

Leitmann:

Yeah, yeah. He had a motorcycle. It was interesting. Uncle Paul, I really didn't get to know, because he had been a lieutenant in the Austrian artillery in the First World War, and was captured in the first year of the war, when the Russians took over a fortress in that part of Austria, again, in the Polish part—Przemysl I'm not going to spell that for you, because it has P-R-Z-S. You know, the regular Polish words. So, interestingly, he did not return—because this is also before my time—until 1920. The war ended in 1918. He was given up for dead. One day—I know this from what I was told—the doorbell rang, and there was Uncle Paul. He had been in a prisoner of war camp in Siberia, and the revolutionary period extended long beyond the Russian Revolution—

01-00:24:18

Burnett:

The White Russians fighting the civil war.

01-00:24:20

Leitmann:

He fought his way across revolutionary Russia with the Czech Brigade, and then once he was in Czechoslovakia, or was it Bohemia and Moravia, he turned up in Vienna, two years after the war. He enrolled at the university, the Technical University of Vienna, and became a chemical engineer, got a master's degree that was required for a professional degree. In the 1920's, the economic situation in Europe in general was very, very bad. He spoke, of course, perfect Russian, having been there for almost five years in Siberia. The Soviet Union, in those days, was building up its industry, and so they invited foreign engineers in particular to take on jobs. In 1929, he left his family behind, a wife, Alice, and a daughter, Elfrieda, and took a job and became the director of chemical plant in what was then, under the Soviet Union, called Sverdlovsk. It was at Ekaterinburg where the czar's family had been done away with. Then, in the early 1930s, maybe '32 or so, he was offered a bigger job in Moscow, and so he took that, again as a director of a chemical plant, and his wife and daughter, Alice and cousin Elfie, joined him there. Again, just to end that story, until a later time, in 1937, he was given a choice: become a Soviet citizen or leave. He maybe did make a mistake. At any rate, he decided to leave and come back to Vienna. That only lasted about four months, because then the Nazis came in.

Now, there is a little side story with my Aunt Alice. Her maiden name was Sobelsohn. Alice Sobelsohn. Karl Sobelsohn was a second cousin of hers. Later was Karl Radek. He was probably one of the major movers of the Russian Revolution, a great friend and protégé of Lenin's. Then when Stalin came in, Stalin hated him, because Radek had a sense of humor and Stalin did not. He eventually became a victim of the trials in Moscow. I think he was put on trial in 1936.

01-00:27:29

Burnett: Thirty-six, thirty-seven.

01-00:27:30

Leitmann: Something like that. He and his girlfriend. He had such a sense of humor. I read this in a historical novel by Stefan Heym, called *Radek*, that he wrote the script for his own trial. He was not condemned to death. He and his girlfriend were sent to Siberia, to a gulag, but only lasted a week, so that was all, of course, arranged.

So now I can go back to my early days, say the first four or five years. I entered grammar school at the age of six. It was very close to where we lived, maybe two or three blocks, so I was able to walk to school. There, again, I remember all these particular things. One of them is that I talked too much, and my mother was always being called in about that. I have photographs of me carrying my little lunchbox in a basket around my neck. And in connection with lunches, the milk that we got for lunch was kept in little water trays on the radiator to stay warm, because everything was in glass containers in those days. There were no plastic containers. And the headmaster's name stands out in my mind. His family name was Immervoll, "always full." Those are little things that somehow popped back into my mind.

There was an earlier thing that I just thought of it, remembering things that you wouldn't expect to remember. When I was three, four years old, my parents had enrolled me in a French kindergarten. Very small one with about eight or ten kids, and two teachers. There I had a bad experience, because they took us to the park, and I stood up in the taxi with another kid, the teacher, and the other three kids or so who sat down, and as we rounded the corner of the Ringstrasse, which is the big boulevard around the inner city—going very slowly, fortunately—he opened the door. He was holding me, and we both fell out. He fell on top of me, and I fell onto streetcar tracks, and the streetcar was coming along. The only thing that saved my life at the time was that streetcars in Vienna, to this day, have cow-catchers, and it was able to slow down sufficiently and scoop me up, with the other kid on top of me. I was out for about a week.

01-00:30:13

Burnett: You were injured, weren't you?

01-00:30:14

Leitmann: Well, I fell right on my face. I broke my nose and whatever. I was dis-enrolled from that kindergarten. That was something that stuck in my mind.

01-00:30:31

Burnett: So the portrait that I'm getting is, on the one hand, tremendous stability. In some ways, this is kind of an idyllic childhood, where you're surrounded by family. There are cousins down the street, and you can go to your uncle's

candy store, and these beautiful parks. I think about the larger context. This is the jewel in the crown of empire. This is Vienna.

01-00:31:06

Leitmann: No longer an empire. But to this day, thinks it's the capital of an empire.

01-00:31:13

Burnett: But it certainly has that feeling. And in those days, and in the days of your parents and your grandparents, a center of imperial action, prosperity, technological advance. And when I was going through your autobiographical pieces and the other interviews that you've done—we'll list them in the oral history transcript—that so many of your male relatives were military men. Your father, your grandfather, two of your uncles, officers in the Austro-Hungarian military. Fought in World War I, fought before that. Your grandmother, born amongst the cannon fire in the Austro-Prussian War.

01-00:32:11

Leitmann: In Moravia, yeah.

01-00:32:13

Burnett: So I think about this contrast of a stable, prosperous, peaceful environment, and a violent, hostile, unstable, dangerous environment, all in the same place. I'm not sure I have a question about that, but I suppose the question could be something like, as a child, how did you absorb that? Was it perhaps just that your uncle had these swords that were crossed at the entrance, and it was a romanticized—

01-00:32:48

Leitmann: I certainly had no notion of being in that milieu. It was my daily life. I certainly had no sense of history at that age. That came much later, of course. But I did experience historical events at a very early age. For example, in the early 1930s, we stopped going to the farmhouse in—the little river was called the Kamp. My father bought a piece of land at a lake called the Old Danube, which, to this day, is a very popular place. We built a little house, a little country house, which my father designed, and then got an architect to help him. We spent our summers, even after I started school, at that place, because it was within reach by streetcar at one end of the lake. My father had a big boat, so he would go to his place of business by boat, get on the streetcar, and then row back across the lake, that kind of thing. That seemed all perfectly natural. There was nothing esoteric or special about that. As you say, it was certainly, for the most part, not unexpected of me. I know we were fairly well-off. I had friends who weren't quite as well-off, but certainly I didn't know any indigent people. They obviously existed. I think, at that age—and I'm talking up to the age of maybe twelve or so—I was aware of events. For example, in the winter of 1934, the country was split, as it very often is, politically. The cities, the big cities, are usually left-wing, and the countryside is right-wing. In Austria, right-wing meant fascist. We had a Fascist party.

01-00:35:06

Burnett:

Italian-style.

01-00:35:08

Leitmann:

Right, sort-of Italian style. It was Count Starhemberg, who was the head of that Fascist party. They had their own militia. Vienna was—I don't know the percentages, I probably could look it up—but certainly, for the most part, Social Democratic. There was a Communist party, which was relatively small. But the Social Democrats were the major party. They had built huge blocks of apartment houses for the workers, Karl Marx-Hof and all these things, all still in existence. Then the Fascists took over. The first Fascist chancellor was a little guy by the name of [Engelbert] Dollfuss. So there was an uprising, because they were beginning to introduce legislation that was clearly along the Italian Fascist model, Mussolini's model. The government called it a civil war. It lasted only about three weeks. The Social Democrats and left-wing allies of the Communists, they called it the workers uprising. It depends on how you look at it. It was brutally defeated when, first, the Fascist militia started—the workers had built up fortresses. If you look at these very large apartment blocks that still exist today, they always were a whole block with an outer wing, and then an inside wing.

01-00:37:06

Leitmann:

You can still see, today, the effect of the civil war. The patches that had been—they were painted over eventually, but you could tell that they'd been heavily—because eventually, the army took over to put this down, and they brought in artillery and aircraft both. That, of course, nobody could resist that. So that civil war ended within about a three-week period. My direct involvement was that, just at the corner of our block, where the open-air market was, they had set up a machine gun nest, with sandbags. I could hear it chatter once in a while, so I guess they were shooting at people. The Jews of Vienna—I don't, again, know the percentage—overwhelmingly were Social Democrats, because the alternative was a Fascist party, essentially. Of course, the Viennese, in general, were left of center. The main support for the Fascist party, and eventually for the Nazis, really came from the countryside, which was, at that time, about two thirds of the country. Vienna was almost a third of the population of the whole country. It's two million, and maybe six, six-and-a-half million people. It's pretty much the same now.

So the alternative, at that time, as far as my family was concerned, they weren't happy with the Fascist government, and in fact, neither were the Nazis, because, in the summer of 1934, there was a Nazi coup. They stormed the chancellor's offices, which were in the imperial castle. I remember we were, at that time, already in our country place. I think it was June, maybe. We listened to the radio, and there was a report first that there'd been an attack on the chancellor's office, and then next news that came a couple of hours later said in fact they had taken over, but the chancellor was fine. Then we knew what had happened, because they started playing funeral music. So that was sort of the scenario. That is really engraved in my mind for some

strange reason, and other things. The importance of the military was—even when the Social Democrats were in charge—was pronounced. For example, in physical education, we used to throw fake grenades as exercise, climb rope and all that. That kind of disciplinary attitude was very strong.

Then the next chancellor, a man by the name of Schuschnigg, took over after the murder of the first chancellor. But he was threatened by Hitler, because the whole movement was—first of all, they outlawed the Nazi party. That didn't sit too well with Hitler, and he put a lot of pressure on Schuschnigg. We're talking about '35, '36, '37, those three years. The Fascist government of Austria felt protected by Mussolini, because they were—it shows you how evil on one hand, and how clever Hitler was. In 1937, I think it was the fall of '37, he invited Mussolini to witness army maneuvers of the German army, and it scared the hell out of Mussolini. Now, there was no more protection of Austria, so that had disappeared.

I, in the meantime, had enrolled in the *Gymnasium*. My father felt that either I was going to become a professional, or maybe even a teacher. So I went to a *Realgymnasium*. There are two branches, even in the current system: a technical one, science and engineering, and a humanistic one. So if you went into law, or the social sciences, you got eight years of Latin and eight years of Greek. I was only destined for eight years of Latin or Greek, so I started Latin. That was the most difficult subject for me, actually. You start at the age of ten. I enrolled in the *Gymnasium*. I passed the examination in '35. That was, in a sense, a less happy period, because it was very highly disciplined, and there was open warfare between the teachers, who were called professor, and the students, who were called, already, mister. At the age of ten, I became "Herr Leitmann," and I remember that they called me a four-fold ass in Latin. "Mr. Leitmann, you are *asinus quadratus*."

01-00:43:03

Burnett: What did you do to warrant this?

01-00:43:05

Leitmann: I don't remember what I did.

01-00:43:08

Burnett: Or perhaps nothing.

01-00:43:09

Leitmann: We were also beaten.

01-00:43:11

Burnett: Oh my goodness.

01-00:43:12

Leitmann: It was the kind of thing, put out your hand, and then you got whacked on them. They didn't pummel you or anything, but that was the kind of punishment—

01-00:43:21

Burnett:

It hurt enough, right?

01-00:43:22

Leitmann:

Yeah. There was open warfare. On the other hand, the students did their best to disrupt the teacher. Put chalk on his seat, and put all kinds of nasty things on the floor so he would slip, thumbtacks on his seat, that kind of thing. I remember that very well, because there was a little store where we bought our school supplies, paper and pencil and all that kind—oh, it also sold little toys, like little jumping things. You could rub them and they jumped all over the place, blew up or something. It's something that really stuck in my mind. The only major thing that happened—I think it was probably the second year—was that there was a girls' school just across the courtyard, next to ours. This was an all-boys' school, about six hundred students. We did our best to be able to peek into the girls' part of the place. We thought maybe we would see them changing their clothes or something. It was a hope. We stood on the toilets and tried to peer into that, and we got caught. So my whole form—there were two classes in that particular form—was expelled for a whole semester. And so we had to take private lessons. My father taught me everything but the Latin, and my old Latin professor, Professor Epstein, was hired to tutor me in Latin, and actually I learned a lot more Latin being privately tutored for a semester than in class. Those are things that do stick in my mind. It's strange, because it's sort of little episodes that—I don't have a whole sense of the period, but it's—it was the Archduke Rainer Realgymnasium, which, by the way, before I forget, is now called the Sigmund Freud Realgymnasium, because he was their most renowned graduate. He graduated from that school in 1873, Sigmund Freud.

01-00:45:47

Burnett:

He was a denizen of your neighborhood, was he not? In Leopoldstadt.

01-00:45:52

Leitmann:

He lived in the second district, which was the largest part of the Jewish population, until he became very famous. His office was always in the central district, which was only across the Danube Canal. He was pointed out to me, walking in a park called the Augarten, which was about three or four blocks from our house. There was a skating ring there. I learned ice skating there. Always part of the famous Professor Freud. He was already quite known by that time, of course. He always walked with his hands behind his back, the professorial walk. Had a little mustache and beard. That sort of sticks in my mind. Again, how much of that was then later reinforced in terms of information, I can't tell. All I know is I was exposed to it originally. Then there were the additional things, that I could go ice skating, ten-minute walk. Got to be pretty good on ice skates. Twenty-minute walk to the amusement park, that kind of thing. It's interesting, because it's really apropos to my whole history. My father had a very sharp temper. He blew up reasonably readily, and immediately regretted it. The worst punishment I ever got was he'd give me one of these slaps. There wasn't anything worse than that. But

when we were kicked out that one semester for trying to look into the girls' part of the—he didn't punish me. Because he remembered his own boyhood, I think. [laughter] All I know is that I was told not to do that, but I wasn't punished.

01-00:47:56

Burnett:

This absolute, rigid segregation of the sexes, it breeds a curiosity, doesn't it?

01-00:48:00

Leitmann:

It was true until the eighties.

01-00:48:03

Burnett:

It was not coed until the 1980s.

01-00:48:05

Leitmann:

Nineteen eighties. There was a woman minister of education, because I looked it up. The school was still only about six hundred students, but it's now coed, and in a different building. They moved into another part of town. It's very interesting. I never had girl friends in that sense, because that's where you get to know girls, in school. It was a big revelation when I came to the United States, that girls were really very nice. [laughter]

01-00:48:43

Burnett:

Indeed. I wanted to just sort of think about this a bit more. Austria-Hungary, end of World War I. Your father serves, both of your uncles serve in World War I. The Central Powers are defeated. My history is bad for Austria-Hungary, but in Germany, there were tremendous reparations payments to be made to the French and to others, and it was crippling the German economy in the twenties. There was hyper-inflation. I don't know how much or to what degree that kind of instability also impacted Austria, if it was subjected to the same kind of thing.

01-00:49:234

Leitmann:

I don't think that they got those major reparations, but I think that certainly inflation, unemployment was—throughout Central Europe, certainly worse among the defeated nations. It was bad. My uncle took a job in the Soviet Union for that reason.

01-00:50:01

Burnett:

Right. He was a graduate of Vienna Technical University. As a chemical engineer, you'd think you'd be able to find work with that.

01-00:50:15

Leitmann:

I could not discuss this with my father, because I saw him for the last time when I was fourteen. I think he got this job for this armaments corporation, run by the Austrian government, may well be through his father's military connections. I mean, I have no evidence for this, but it certainly didn't hurt. This is certainly part of it. The other kind of influence of the military, I only felt—again, we had this home in the country where we spent the whole summer. I just dug up a nice little book that they put out, *The Austrian*

Military Forces. It was published in 1937, still under the Fascist government. When we had a house in the country, there were maneuvers in the neighborhood. It was an open countryside. I somehow walked into the field kitchen area, obviously, and I became a pet [mascot]. That day—it was a single company—they were laying wires and all that. It was fascinating. They adopted me as a pet. I didn't go home that night. My parents, the next morning, were looking for me, and I wasn't there, so they called the police. Took about a day and a half before they found me with the military, because they adopted me. [laughter]

01-00:51:57

Burnett:

It's the story of belonging, of your family, your extended family, participating in the imperial state project, right? So advancing high in the military ranks. This is a high-technology, industrial nation. Austria is the fourth-largest machine-manufacturing state in the world at this time, behind Germany, the UK, and the United States. So it's right up there in terms of armaments manufacturing—

01-00:52:31

Leitmann:

Bigger than France.

01-00:52:32

Burnett:

Bigger than France. Manufacturer and exporter of high-technology goods, such as cars, railroad engines, aircraft. Its early oil industry is important. So it's at the center of modernity, speaking of Sigmund Freud. It's exporting goods, high-technology goods, engineering marvels, science, and—

01-00:52:57

Leitmann:

Well, in medicine, they were the leading nation, certainly under the emperor. Literature, music.

01-00:53:08

Burnett:

The list goes on, doesn't it?

01-00:53:13

Leitmann:

I wasn't aware of that. I was aware only when I was maybe already ten or twelve years old, and my parents would go to the opera, for example, that kind of thing. I think as long as we're on that topic, I was really brought up by my maternal grandmother, Fanny, who lived in the same apartment, owned the apartment house and everything. She was a very strong, smart lady, and very funny. I still remember we always had the latest radios and gramophone equipment because of my father, who had opened, after he stopped being an accountant, an electrical supply place. My mother helped him in the office there, so my grandmother really took care of me. She would say things like, "Turn on the radio. Maybe we'll hear something," that kind of sly thing. When she dropped a cup, she said, "You see that? It jumped out of my hand." She had little sayings for this. She was a very bright woman. She always lived with us, until the day she died. She died, in fact, at home, many years later, in the United States, in 1947. I remember that she was already in a coma. Her

son, Uncle Paul, and my mother were present. Her other child, Aunt Elizabeth, had already moved to Hollywood with her daughter, so they weren't there. But she opened her eyes. I sat there next to the bed, and she said, "Am I still here?" And she died.

01-00:55:06

Burnett:

Oh my God.

01-00:55:09

Leitmann:

I'll never forget that. By that time, of course, I was a college student. These little—call them anecdotes—these little bits jump back into one's mind as one begins to think about it.

01-00:55:27

Burnett:

Her strength of character, and yet a lightness.

01-00:55:33

Leitmann:

She was witty. Even when I started being a college student at Columbia University later, and I was dating girls, she also had little off-color things to say, which I won't repeat now. She had a real sense of humor. She's certainly one of the most important people in my early life. There's no question about it.

01-00:56:06

Burnett:

You were raised by her, in part, right?

01-00:56:08

Leitmann:

Yes. The other grandmother immediately moved to her other daughter in San Francisco, so I had very little contact with her, Alexander's wife, Cecilia. Aunt Adele was living here. I didn't know her as well. Now, it is true that we lived with her for a while. That's a period that I haven't talked about. But we were going to leave that until we talk about the Anschluss.

01-00:56:47

Burnett:

I was just thinking about the—it's such an advanced nation, and your family is at the center of it. You're at the center of it geographically, and your father goes into the electronic supply business. So the very cutting edge of technology.

01-00:57:07

Leitmann:

He was always interested in science, technology, and in expeditions and explorers. He did, in fact, just before he got married, the summer before, he was a volunteer for a dig in, I think, Egypt, or somewhere in the Middle East. Then, to make up for maybe giving me a slap or something, he immediately made up by—the two main things I remember, and both of these places still exist—a science, and really astronomy center, called Urania. It was a big building. It had lecture halls. It had—

01-00:57:52

Burnett:

An observatory, right.

01-00:57:53

Leitmann:

Observatory. It was all rebuilt after the war. It's in beautiful shape now, right on the Ringstrasse, which is the big boulevard around—and on the Danube Canal, right there. I used to be taken there, first of all, to see movies. They showed a lot of movies about exploration and science. I still remember one explorer that he always talked about was Sven Hedin. I think he was an arctic explorer. Norwegian, I think. He [G. Leitmann's father] had an affinity for foreign countries. I think the only foreign country that he went to, aside from Serbia, was France and Great Britain. He never got beyond that. He and my Uncle Fritz used to go to the football games. In fact, in 1934, they went to the international football game, the—

01-00:58:52

Burnett:

The World Cup?

01-00:58:53

Leitmann:

World Cup. Brought me toys back from there, that kind of thing, and always interesting toys. I got a little generator, and it could generate enough power so that you could really feel it, but not kill yourself. A little airplane. A crystal radio, where you fooled around with—

01-00:59:13

Burnett:

Could you get broadcasts from your—

01-00:59:16

Leitmann:

I could get broadcasts just with earphones. It was a lot of fun, because I used to hide under the blanket with a flashlight and do that kind of thing. These are things that stick in my mind. Just as an aside, because it's something that you always read in either the biographies, the autobiographies, of people like Schnitzler, who was a famous author, that the sex education of the middle-class boys was usually performed by the maids, in some way. Maybe it was oral, maybe it was just manual. Whatever, it was that kind of thing. In the summer of '37, we had a young lady who—she must have been about sixteen or seventeen. We lived in adjoining parts of the veranda, which was enclosed with netting, because we had mosquitoes. She taught me certain things, and we were caught in flagrante by my grandmother, Fanny. She was fired and had to leave within one hour. That young lady came from the same birthplace as Hitler, Braunau am Inn, which is the connection in my mind.

01-01:00:37

Burnett:

Oh my goodness. That's terrible that she got sent down for that.

01-01:00:46

Leitmann:

It was called improper behavior. I still remember the German—improper behavior.

01-01:00:51

Burnett:

Well, you had, in many ways, a kind of standard, middle-class education that had this emphasis on technology. Even the toys were educational. A dynamo,

getting interested in the fundamental principles of electricity. A chemistry set, which was a staple for boys, at least from that time, until my day.

01-01:01:18

Leitmann: Which I misused. I almost blew up the apartment.

01-01:01:22

Burnett: I misused mine as well. But I understand you literally almost blew up your apartment.

01-01:01:26

Leitmann: Well, I dropped—I still remember what—I dropped sodium permanganate crystals into hydrochloric acid. It made a nice little red cloud, then it went *whoosh*. What saved my eyesight was my grandmother, who immediately rushed me under the shower. She was important to my life, very important.

01-01:01:50

Burnett: A guardian angel, definitely. Outside of the school system, your father is showing you things, introducing you to ideas, introducing you to technology. You also took English as well, which I found—it wasn't part of your regular curriculum.

01-01:02:17

Leitmann: Both my mother and my father had already had English in school. My mother also went to some kind of commercial college. In fact, the last communication I had from my father—he always wrote to me in English. He worked for the Red Cross there for a while. They had foresight. Certainly my father was very stern, but had a soft heart. Later on—we talked about this before—what really did I absorb from my parents, other than, of course, from Grandma Fanny? I could spend five minutes on this, because I tried to, in the last few weeks, at night, think about what strange character traits do I have, and where did they come from? For example, I'm an extremely controlled personality. I never lose my temper. I pout once in a while, but I never lose my temper. That came into play later on, when I was at the Nuremberg Trials. I never lost my temper. I had good reasons to do so, but I didn't. I think it had to do with the fact that my father did lose his temper, and I said to myself, I'll never let myself do that. I'm sure that this was something that I learned by osmosis.

Another thing I learned was my father was an early collector of lots of things: paintings, artifacts. He had Etruscan axes and such. He had a cabinet even at his mother's apartment, who lived about twenty, twenty-five minutes from us, on the Danube Canal. To this day, I suffer from that. I put stuff all over the house. I still, to this day, go to internet auctions and buy things that I'm going to leave to my daughter, who turned into a painter. Those are certainly things that one learns, either in a negative way, by not doing something, and then in a positive way, like collecting, going to auctions, stuff like that. I'm sure that that was a direct influence. My mother less so, except that she was a great

cook, and so maybe the love of good food. Now, my mother never drank a drop of alcohol in her whole life. Not one drop.

The fact that we were not religious, starting with my parents, I learned later—and that was verified by some of my cousins, the ones from the delicatessen candy store, cousin Kay and cousin Martha—the youngest, the boy, Kurt, was a statistician in the states, and later became head of the public health system in the state of Maryland. For some reason, they wanted a statistician in that job. We sort of learned from each other, and my cousin Martha was a real tomboy. We used to climb up on the roof deck in our little house in the country, and there was a freeway, or highway, anyway, maybe six, seven blocks away. We used to blind the tourists with mirrors, that kind of thing. Oh, and she once gave me an inverted Mohawk haircut. Right through the middle, she shaved my head. [laughter]

01-01:06:31

Burnett:

She said, it will look great. Just hold still.

01-01:06:36

Leitmann:

She only died last year. It's very interesting.

01-01:06:47

Burnett:

From collecting, what's happening there, when you collect art? The impression I'm getting, from what you learned from your father, is this admiration and awe at the world. That you're going out and you're appreciating—

01-01:07:05

Leitmann:

Yeah, the fact that he was interested in exploration. He had books, which I had until fairly recently. When our daughter moved in here, I got rid of a lot of books. There was a book in German called *Cowboys, Gauchos, and Vaqueros*. I'm sure he would have loved to go to South America, for example, that kind of thing. He was in touch with people. The minister of war of Afghanistan. He had a noble friend—we never had a car—who had a car. In fact, I have a cigarette case that this count gave my father. It has the little crown on and everything. It's in that cabinet. Maybe they went fishing together. I know he was picked up by a car, and they went off. Maybe just drinking beer, for all I know. But on Yom Kippur, when—I, to this day, fast, to prove that I'm not irreligious because it's inconvenient—and our son—we're from a very ecumenical family—he used to go down and visit Uncle Fritz on Yom Kippur, while my grandmother and maybe my mother also went to the synagogue, just those two holidays. I was told that they drank schnapps and had ham sandwiches. [laughter]

01-01:08:40

Burnett:

So definitely in the agnostic camp.

01-01:08:42

Leitmann: Yes, I think so. I think so, yeah. It's very interesting how one can be religious, and then suddenly become agnostic, or atheist even. I'll touch on that when we talk about the Nazi period and thereafter.

01-01:09:01

Burnett: Yeah, go ahead. Sorry.

01-01:09:04

Leitmann: Yeah, so I won't mention this now. It's, again, something that occurred to me later in life, something that I learned from my father. That's what you do in opposition, so it's very interesting. Of course, at the time this happens, you have no idea that you're learning something. It just somehow sinks in, and it stays for a long time. I've forgotten lots of things. If I look at papers I wrote, I don't know how I got from here to there now, but I do remember—for example, I remember, just to really go back, a little jingle that I learned in that French school, when I was in French kindergarten. I can recite it to you. "*Le bœuf, der Ochs; la vache, die Kuh. Fermez la porte; die Tür mach zu!*" See?

01-01:10:00

Burnett: Mix of French and German.

01-01:10:00

Leitmann: It popped into my head just the other day. And I can sing all the Nazi songs.

01-01:10:06

Burnett: Oh my goodness. It was that present in the public domain? I mean, it's unforgettable, of course, for you, because of the change.

01-01:10:19

Leitmann: Yeah, but I mean most people, I think, can consciously suppress these things. As I get older, of course, it pops up more, because I guess the circuit has become empty and this other stuff comes back. [laughter]

01-01:10:38

Burnett: I don't know about that. One of the things that I'm curious about, I guess because I keep harping on the Austrian imperial side of things, is this question of belonging. There is this insider/outsider status for your family there in those years. You're, to some degree, the ultimate insiders, in that you're in the center of this nation that is doing well, [a family] that is welcomed and accepted, has received some of the highest honors for service to the state. You said your grandfather and your father were patriots, and believed in this place and in this people, and they saw themselves as Austrians.

01-01:11:39

Leitmann: Oh, absolutely. Absolutely. It's interesting. There was a book called *The Pity of It All*, that deals with relations between the Jews and the Germans over the last three hundred years or something like that. When emancipation came about, middle nineteenth century, the question was—in Germany, this was particularly so—I didn't encounter that, at least not consciously—were they

going to be called German Jews or Jewish Germans? It was very important to be called a Jewish German, not a German Jew. There were big debates about that.

01-01:12:30

Burnett:

And there were different responses, because there was kind of an ongoing—Jews would be welcomed into a positive relationship with an authority. It could be an emperor or a king or whatever. That was one strategy, and that involved assimilation to some degree, it would be linguistic, cultural, and all of that. Then another response, I guess further east, would be the kind of more radical separation, so the Hasidim movement, the Hasidic movement, to sort of have a kind of separate existence.

01-01:13:11

Leitmann:

Yeah. Also, I think the involvement of Jews, say in the Russian Revolution, again, in response to what had been done to them for a couple of—more than a couple centuries.

01-01:13:33

Burnett:

There's a tremendous stability to Europe, to some degree, in the period leading up to World War I. There's this prosperity and commercial advance and global trade. You had this enlightened emperor, Franz Josef, who was very, if not completely welcoming, very tolerant of the presence of Jews in part of the state apparatus.

01-01:14:10

Leitmann:

Yeah, they even ennobled Jewish families, like the Rothschilds, people like that, became barons. It's interesting, because under Kaiser Wilhelm in Germany, Jews could become non-commissioned officers in the army, but not commissioned officers, until the First World War. Then enough officers got killed, and that's when they could become commissioned. I don't know how high they could get, but at least they could become lieutenants or captains or something like that.

01-01:14:44

Burnett:

So in the aftermath of World War I, there was unemployment, there was inflation, there was instability, and also this Wilsonian self-determination principle. That accelerates a notion of nation matching with peoples, and peoples being defined ethnically and racially. That leads to all of the acceleration of anti-Semitism of a very particular kind. Do you have any recollections of an experience of anti-Semitism that you would recognize—of course, you're a child—

01-01:15:30

Leitmann:

Certainly not until the Nazis came. We had one family member who actually converted. My mother had a second cousin, maybe a third cousin—but it was a Fischer—who was a tenor, fairly well-known and regarded tenor, Ernst Fischer, who was also a member of the La Scala Company, where he was known as Ernesto Piscatore. I always remembered that. Again, this is not

firsthand. I found this out later on. He decided that it would really do him a lot of good professionally if he converted, and he did. But at that time, the archbishop, Cardinal—his name was Innitzer, so this was in the early/middle thirties—he was able to approach and wanted to know if the archbishop cardinal of Vienna would be the converting priest, the one who would—I remember the line was, “Mr. Fischer, I know that you’re not doing this because you want to be a Christian.” He said, “You will have to deal with an ordinary priest.” But that’s the only even story that I know that has any bearing on this kind of thing. Now, there were anti-Semitic politicians in Austria. One of the mayors of Vienna, long before my time, was. But beyond that, I really had no idea about this kind of thing.

01-01:17:20
Burnett:

Leopoldstadt was the second district, which is across the river, across the canal, from central Vienna. You wouldn't describe it as a Jewish ghetto, because it was only—

01-01:17:35
Leitmann:

No, no, it was never a ghetto. Even though the majority of the Jews lived there—and this has, I think, a lot to do with, of course, if you go two or three hundred years earlier, they were still burning Jews on a pyre in Vienna. Then a better emperor would come along and suddenly—so Leopold was the predecessor of—I don’t know, Leopold the First, I don’t know—of Franz Josef, and that part of Vienna was called Leopold City. I don’t know whether this was because he did good things or bad things. I don’t know. I don’t remember it. There was certainly never anything in school that indicated that kind of thing.

01-01:18:25
Burnett:

I imagine there was prayer in school, or not? Was there any kind of chapel?

01-01:18:34
Leitmann:

There was prayer. In those days, there were still crucifixes in the classrooms. But that was true until fairly recently in Austria.

01-01:18:43
Burnett:

It’s a very Catholic place.

01-01:18:47
Leitmann:

First of all, Jews were always excused from that, and there was Jewish instruction on Saturdays, which was voluntary, that kind of thing. We did not have school on Saturday, for example. In Austria, you could have had three holidays a week, because Friday was the Muslim holiday, so you only worked four days a week. [laughter] I don’t know.

01-01:19:15
Burnett:

Sign up a lot, and then you can—

01-01:19:16

Leitmann:

Because, you know, if you look at the composition of Austria-Hungary, it was really a very multinational state. You look at the Balkans, almost all the Balkans were part of Austria. Even during my parents' time, the Italian border was at Trieste, so the Austrian navy, which was one of the largest navies in the world, they had to be in the Adriatic and go out into the Mediterranean, and the rest of it through the Adriatic. The whole Adriatic east coast was Austrian, down to Albania. It's very interesting. I know, for example, later on my Italian friends used to tell me that their parents always took their honeymoons in Austria, in Vienna. But then you have the enmity, even to this day, about South Tyrol and the Alto Adige region of Italy. That's still under discussion. Is it Austria? Is it Italy? Right now, this is a main political point with the current government of Austria. They want Alto Adige to be reunited with—now, I do remember that we used to sing patriotic songs. Andreas Hofer was an Austrian hero, who stood against the Italians. In other words, he was the leader of the people from South Tyrol. We used to sing songs about him, Andreas Hofer. Again, the name popped into my mind. So that kind of thing still exists.

The enmity, during and after the First World War, was also directed against the Italians, because in those days, of course, they were allies of the West, and they were called perfidious Italians, just as the Germans called the British "Perfidious Albion." It was called "Perfidious Albion." That kind of thing happens in multinational—particularly when nationalism is just rising. Germany was unified in the nineteenth century, Italy was unified. There were lots of principalities and that kind of thing, but really a unified national government didn't exist in many of those countries.

01-01:22:08

Burnett:

What's that phrase, or that term, irredentism? There's this sense that, if there's a minority within a country, and there's another nation that has that ethnicity or that linguistic group, they want to sort of be united with that, or they want to redraw the borders, to conform to some idea of nation.

01-01:22:32

Leitmann:

Certainly, the Balkans are a main example of that. Maybe even the Middle East now, I don't know, but that's another story. This is the great danger of extreme nationalism, is that it leads to that kind of thing. It makes you feel superior. I think a lot of it, there's a psychological basis for that. Human beings like to be superior. I don't know why. It has puzzled me for a long time. What is the necessity for—it was Greater Germany. Now it's going to be Greater America, I guess. You get that kind of thing, you're better than they are, therefore you deserve more. I think a lot of that has to do with it.

01-01:23:28

Burnett:

Yeah, selling entitlement, and creating a scapegoat.

01-01:23:34

Leitmann:

Yes, and very often there's an economic basis, because sometimes that subgroup somehow is in a lower economic class, and that immediately, of course, is a point that begins to rub, and is a ready part of the population for extremism—whatever.

01-01:24:01

Burnett:

Certainly what's happening during that time period, if you look at the United States, the cities are doing better than the countryside, almost everywhere. For the longest time, it was the opposite. In the cities, you'd get sick and die, basically. But with the transformations, the technological transformations, there's just a greater, greater gap between the cities and the country in terms of income and prosperity and comforts and all of that. The extra layer, in the case of Austria, is you have this largely Christian, Catholic countryside, and you have a polyglot, multiethnic—

01-01:24:49

Leitmann:

Multi-religious.

01-01:24:50

Burnett:

Multi-religious cities that have connections across the globe and are doing well, and are doing relatively better. I don't want to over-interpret what's happening in Austria, but there's a rural/urban divide that is increasing after World War I in particular. So there's all kinds of opportunities to take advantage of that politically, and that's something that—well, let me put this as a question. We can only draw from your youthful experience, and what you learned after the fact from what other people told you who were adults at the time. Was there a sense that what began to happen in the 1930s was an infection that came from Germany, or was it finding fertile ground in Vienna, in Austria?

01-01:25:57

Leitmann:

There had been a movement, even in the 1920s—after all, Hitler lived there for a long time—that the Austrians really belonged to the German race. Hitler took advantage of that in a very clever way. Austria was essentially the bulwark against the Huns. They were essentially the buffer that protected the German nation in a larger sense. There was a group of people who bought that very early on. They were obviously economically deprived. They weren't the only ones, but there they found fertile ground. That started already right after the First World War. The veterans, for example. Anti-Semitic propaganda flourished in those days, because there was this whole myth about the Jewish merchants who were making a lot of money on the war.

01-01:27:16

Burnett:

The Rothschilds, right?

01-01:27:18

Leitmann:

Yeah, and just in general. And the—

01-01:27:21

Burnett: [Protocols of] The Elders of Zion? Is that a tract that came out? A bit earlier [1903, first published in Russia].

01-01:27:25

Leitmann: That's a story that existed already, I think, in other countries, of course. Even in Great Britain, there was that kind of thing. A very interesting thing happened. It just occurred to me. This was really internally very sad and divisive. There were the eastern Jews and the western Jews, and the western Jews always felt superior. In Poland, in eastern countries like Romania and those places, where the Jews lived in little towns, and were usually poor—they were tradesmen and that kind of thing—there was always a certain amount of—wouldn't call it enmity, but certainly a resistance to the eastern Jews. They became also the caricatures. I think it's very sad. You had the two groups of Jews, the Ashkenazi and the Sephardic Jews. The Jews in Spain already were superior, because after all, they were the philosophers and what have you. Then it led to really sad things. This is why I always felt, and certainly feel, for a long time that we've got to judge individuals and not groups of individuals. It's very scary when people act *en masse*. I see this in political rallies, whether it's in Nazi Germany or the United States, people shouting slogans. Hitler was a genius. He took advantage of all that. He wasn't stupid. He wasn't stupid. I remember that, in the thirties, when we always had the best radio shortwave and everything, we used to listen to Hitler's speeches. I even became a mimic of his speeches, and used to amuse people at parties in my parents' house by making a speech à la Hitler when I was twelve years old. I could shout like that, and say nothing. Because if you listen to many of his speeches, it was slogans.

01-01:30:12

Burnett: So you would mock that, and it generates this laughter.

01-01:30:19

Leitmann: It was, in a sense, humor of the gallows, because we had even relatives in Germany, but there was still the belief that it isn't going to happen to the assimilated people, or certainly there was no idea of being annihilated. Maybe we would have to leave. That was always a possibility and a fear. But also, I think, in my family's case, my father, not until Kristallnacht did he realize that this was really—there was only one way, and that was to leave. Until that time, he wasn't so sure. It could go away. We had anti-Semitism before; this will go away kind of thing.

01-01:31:09

Burnett: I guess that is a part of it, that anti-Semitism has been with the Jews for such a long time, and it's come in waves, as you say, and it's gone away. There was a pogrom, but we persevered, we survived. Not the—

01-01:31:29

Leitmann: In France, they had the Dreyfus Affair, for example.

01-01:31:33

Burnett: Right, turn of the century.

01-01:31:33

Leitmann: So even in the most progressive, democratic countries, that can happen. But it does lead to this false hope, you see, when it begins to creep in and on you. There's always this sort of—*sub rosa* hope that this is temporary.

01-01:31:59

Burnett: You mentioned one thing which does really become ubiquitous, not just in Germany or Austria, but in the United States and Canada and other places, which is race science. That really takes off at the turn of the century, and just accelerates. It begins to shape and govern the social sciences, and it's at the heart of the sciences, or what we think we know about human beings, is that there are these races, and they have these characteristics.

01-01:32:28

Leitmann: After all, the racial policies of Germany, Nazi Germany, were made scientific. There was their philosopher Rosenberg, who wrote a big tome on this, and the Nuremberg Laws were enunciated. You know, what percentage of a Jew are you or not kind of thing. But part of it was already in existence in Great Britain, in the 1800s. These things don't just spring up. Just as in the sciences, we stand on somebody else's shoulders kind of thing. It was very insidious, but it was done very cleverly by these people. If you look at the scientification of racism, was really brought to a crux, because there's racism, there was racism, there's racism, but it's a much more diffused kind of thing. It's a feeling, and it isn't really made lawful. In Germany, it became the law. To give you [a sense of these] stupid ideas—and I was still involved in this personally, because we look at my German passport, there's a big "J." I have the swastika right on the passport, but over it is a big "J." All Jewish men, by 1939, the middle of '39, had to have middle names, namely had to be—what? The women had to be called Sarah, and the men had to be called—

01-01:34:21

Burnett: Israel? Israel, yeah.

01-01:34:24

Leitmann: Yeah.

01-01:34:26

Burnett: Just a mark, to have a mark that identifies—

01-01:34:29

Leitmann: Then, of course, there was the yellow star and all that. Another clever move.

01-01:34:38

Burnett: To distance. There was such concern about urbanization—I'm not speaking of Europe, I'm just talking about the United States. This concern about degeneration, racial degeneration. They thought about maintaining racial

health in terms of intermarrying the best, not altogether different from cattle breeding, so you want, like, the best—

01-01:35:14

Leitmann: No, that's right. But you had it at both ends. You had euthanasia at one end, to purify the race by getting rid of people that weren't normal, on the one hand. That's right. It's interesting. We will get to this maybe a little bit later on, but I made really deep, deep friends with Germans of my own generation, which is much more difficult—it's not a problem with the current generation—and then find out that, way back in their own experiences—sometimes they only found out fairly recently—they suffered exactly from the same kind of racism, or idiotic kind of thing. There were people that really believed, I think, it wasn't necessarily a racist thing in the sense of, say, anti-Semitism or whatever. There were these scientists, looked at facial features, that kind of thing.

01-01:36:40

Burnett: Phrenology.

01-01:36:41

Leitmann: Phrenology. Breeding. This whole idea was really done in the best way by the Nazis. This whole idea of the Aryan race. Hitler may have been a little man with black hair—

01-01:37:03

Burnett: Brown eyes.

01-01:37:04

Leitmann: Right, so there had to be excuses. But blonde and fair skinned and that kind of thing, and then—

01-01:37:14

Burnett: Nordic.

01-01:37:15

Leitmann: Nordic, and the whole harkening back to the [German] kind of thing. There's a long history. These things, they didn't just come up. There was a very famous writer and humorist by the name of Wilhelm Busch. I don't know whether you came across him. Wrote wonderful books with rhymes and stuff, which were proper for kids. I had all his books. But then he also had the anti-Semitic tracts and stuff like that, again based, essentially, on purity of race. He was nineteenth-century. This springs up everywhere. Then the excuse, very often, is that you pick on a particular person, who did bad things, and then simply spread it out over the whole group. For example, the pogrom of the Kristallnacht, which extended over a number of days, it was a Polish Jew who shot a German diplomat in Paris.

01-01:38:30

Burnett: Yeah, [Ernst] vom Rath, I think.

01-01:38:32

Leitmann: Von Rath. It got to be clearly a directed pogrom, when you looked at what came out of it. The physical manifestation of burning synagogues and whatever, that came out of the propaganda, but the preparations for it are so clear, because within days, they imposed, for example, a penalty, a financial penalty. It all worked out to a T. This wasn't something that just happened. It was just very convenient that this guy shot the German diplomat, and then you have a little—

01-01:39:22

Burnett: A tax.

01-01:39:23

Leitmann: In Germany, the idea of having the Reichstag fire blamed on a Communist. So within a day or two, you could pick up all the Communists and throw them in concentration camps. This is all very cleverly planned. These people, most of them had doctorates, after all, like Goebbels and those people. They weren't stupid in that sense.

01-01:39:51

Burnett: And were experts in propaganda and—

01-01:39:56

Leitmann: Leni Riefenstahl. An amazing woman, absolutely amazing, obviously highly intelligent, smart. But she could overlook things, because the important thing was that Hitler gave a great deal of support to making movies. Now, if the movies happened to be propaganda movies, that's another thing. It's a piece of art. After all, she was really the inventor of the party rallies in Nuremberg. Even today, in places like China, for example, you get the same thing. Who directed the Olympics in Tokyo? It was a movie director. It was very nationalistic. It was a beautiful spectacle, until you sort of take it apart. I saw the Nazi rallies in that.

01-01:41:03

Burnett: In the Beijing Olympics?

01-01:41:04

Leitmann: Yeah. I said, "I've seen this before." It wasn't, I think, a happenstance that the person who directed it was a very famous movie director.

01-01:41:16

Burnett: The seduction of images, the seduction of symbols.

01-01:41:20

Leitmann: It's something that appeals to something basic in people, because in the sense we want to be together. The most terrible things happen because guilt is now diffused. If I do something bad, a million people did the same thing, so I'm only guilty a millionth of the guilt. That's a very powerful thing. Rallies, political rallies, here, there, everywhere, where people shout, "USA! USA!"

or “Hitler! Hitler!” it’s the same thing, and it frightens me. It really frightens me.

01-01:42:03

Burnett:

Since then, you have seen things in different countries, in different contexts, that remind you of that time?

01-01:41:10

Leitmann:

Yeah, absolutely. Absolutely. You get it in France, with [Jean and Marine] Le Pen. Again, the chanting. Because, first of all, you feel much more powerful, because you’re not alone. So the bigger the group, the more noisy they are—and spectacles. If you look at the 1936 rally, which is sort of the precursor to all this, there’s something in your stomach that says, “boy, this is terrific!”

01-01:42:49

Burnett:

Just taking the imagery, down to the uniforms, there’s something really seductive about—they paid very, very careful attention to what would be attractive to people, and they just amplified that to the nth degree. There’s also a sense, I think, of sowing confusion, sowing doubt, accusing your enemies of that of which you’re most guilty, to sort of distract. Someone says, “you’re doing that,” “no, *you’re* doing that!”

01-01:43:26

Leitmann:

That’s right. The sort of answer from people who either are Holocaust deniers, or at least poo-poo the Holocaust, or say it’s not a German invention.

01-01:43:43

Burnett:

It was fake news.

01-01:43:45

Leitmann:

But no, they go to the Boer Wars. Who invented the concentration camp? The British. I heard this story many times when I was involved in the [Nuremberg] trials, and then even interrogating people outside that: no, no, we didn’t invent that; that was really the British who invented that. To some extent, that’s true.

01-01:44:10

Burnett:

It is true, and it is an opportunity for reflection, that instead of saying only the Nazis did bad things, it’s an opportunity to say, look, in our history, we have these things that are bad. We’re talking around something that we’re going to get into next time, which is a real turning point in your life and the life of your family. But I think I wanted this session to be about those formative years: That family, those ancestors, who shaped your life, and their enthusiasm, their passions for learning, for art, for music. Even if you didn’t like your piano lessons very much, they exposed you to all that was—

01-01:45:06

Leitmann:

It’s an opportunity that I and people in my circumstances got that maybe a lot of people didn’t, and that’s one of the problems. That’s why it’s so important to support organizations or milieus or métiers, things that you can do, to allow people to do that and not pick up the worst things. Maybe pick up the best.

Some people will always pick up the worst things. Mankind isn't that great. That's why I like dogs. [laughter] No, it's true.

01-01:45:54

Burnett:

But educational opportunity—and that was something that your parents were deeply invested in, not just in the school, but outside, in taking you and exposing you to experiences. That was formative for you.

01-01:46:12

Leitmann:

Yeah, and I'm very grateful for that, of course. I hadn't given that conscious thought for a long time, because I was just too busy. But it's certainly true. This is really, I think, the, to my mind, main problem of minorities in the United States. Other places, too, of course. Those minorities, where the family is very strong and gives the kids opportunities to learn, do a lot better than those who don't. This is sort of the obvious denial of the racial aspects of those problems, is that it's really the opportunities, the history, that kind of thing. Sometimes people say to me, well, look at the terrible life you have had. Certain period of time, lost your father, all that kind of thing—and you still did well, so you must be a better racial stock. You begin to think about that. This is such complete nonsense. But it's appealing, because it's an excuse for not doing well.

01-01:47:41

Burnett:

Right. You can see that the patterns are there. Access to educational opportunity, a strong and safe family, a stable, safe family environment—

01-01:47:52

Leitmann:

I think that's maybe even more important than formal education, because a lot of the worst people in the world, among the Nazis, had very fine, formal educations. That's not enough. It's certainly one aspect of it, but it's not sufficient. As a professor, I don't want to poo-poo education, but I say it's not enough. It's not enough.

01-01:48:23

Burnett:

Maybe next time we'll talk, too, about the kind of Hannah Arendt banality-of-evil thing. It's not just someone's personal, individual trauma that leads them to do bad things. There is something about institutions, there's something about the direction that a collective can go that can make things worse. [phone rings] That bell is probably a good time—

01-01:48:46

Leitmann:

For you?

01-01:48:47

Burnett:

No, it's probably a good time for us to stop, and we can pause. And we'll take up next time.

01-01:48:52

Leitmann:

Terrific.

Interview 2: April 24, 2018

02-00:00:17

Burnett: This is Paul Burnett interviewing George Leitmann for the University History series. It's April 24, 2018, and this is our second session. In the first session, we had talked about your ancestry, your childhood, your immediate family, and we were beginning to talk about the darkening storm of the 1930s, the changing atmosphere, and the rise of Nazism in Germany, and then also in Vienna [Austria]. Why don't we start by talking about the coup, basically, on March 11, 1938, and the Anschluss of Austria into what is now being referred to as Greater Germany? You are how old at that point?

02-00:01:22

Leitmann: I was twelve.

02-00:01:23

Burnett: You were twelve years old. From the eyes of a twelve-year-old, what did you see and what did you encounter in those first few days?

02-00:01:34

Leitmann: Well, first of all, the airplanes flying overhead, and paratroopers coming down, which was very exciting, not to my parents, but to me. The whole military aspect—tanks rolling down the street, and listening to the radio. Of course, there was no TV. Getting the news, obviously from the Nazi side, reported. I remember—I really still hear this—the announcer said, just as [Adolf] Hitler was crossing the border in his limousine from Germany to Austria, he said, “We can already see the Führer's eye glinting in the distance.” You know, that kind of thing, which—

02-00:02:25

Burnett: Romantic.

02-00:02:26

Leitmann: Romanticized. You could already see that the whole idea that there was opposition was completely wrong. These people that were standing along these roads in the villages as he drove through – and then towns as well – were truly enthusiastic to see him. I wondered about that for a long time. Then I began to read—partly in preparation for this, but also before—was this really unopposed? It's clear that, from the very outset to the end of the First World War, the majority of the Austrians wanted to be part of Germany. Everything showed that. And understandably, I think, because they lost an empire, and suddenly they were Switzerland or Romania or something. They could go back to another seventy, eighty million Germans, and be, again, part of an empire. The idea that there was really whole-scale opposition, I think, is just wrong. Just as he won the election, Hitler won the election, fair and square in 1932, I think the same thing happened here. Now, there was a subsequent plebiscite three days after they took over there, and the vote, presumably, was 99.7 percent yes. Could have been, because I looked recently at the ballot, and

there was a big circle, and it says “yes” in it, and a small circle that said “no,” and the ballots had to be handed in.

02-00:04:21

Burnett:

So it wasn't a secret ballot.

02-00:04:23

Leitmann:

Of course not. Maybe that was part of it, too, but I think they would have got the majority at any rate. There's no question about it in my mind. That really gives the whole background—a different picture, different character. Some of the things that now surprise me, and my mother, really shouldn't have been surprises. There was always a history of, say, anti-Semitism. I think last time, we were mentioning why was the second district where most of the Jews lived called Leopoldstadt. Well, it turns out that that was—and I just looked that up a couple of weeks ago—that was in honor of Emperor Leopold I, the emperor of the Holy Roman Empire of the German Nation. It was the non-Jewish people in that district who were honoring him by his name, because he had, in fact, kicked the Jews out of that district. It wasn't in gratitude by the Jews, but by the non-Jews in that district. The whole color of history really was always dark. There were interludes where the Jews were appreciated, particularly the ones who helped the government—say, the bankers. That was true, of course, in Germany, in the different duchies and little places. They became ministers of finance and something like that. By the way, the Secretary of the Treasury, in the South, in the United States, in the Civil War, was a Jew. People don't know that. I think the first Jewish Cabinet—this happened to be in the Confederacy—was a Jew. That's a very special thing.

Anyway. I also, as I mentioned to you, I may want to take the opportunity to correct, or clarify, some recollections in previous interviews, I had either misrepresented or misremembered, and also maybe make a few additional remarks. I think, towards the end of the last interview, I mentioned my Uncle Paul, my favorite uncle, who had elected to return from the Soviet Union, where he was working, when he was given the choice, become a Soviet citizen or leave, and made it in time for the Anschluss. He, as I then mentioned or related, had come back two years after the end of the war—not in 1918, but in 1920—to Vienna, because he had been captured by the Russians in the first year of the war. The fort was called, by the way, Przemysl. We spelled it at the time, and I'll put it in my transcript the correct way. Then he spent, essentially, five years as a prisoner of war. I just finished reading a book called *Setting the East Ablaze*, which deals with that period of time, 1917 through maybe '20 or '21. The Soviet Union, the Bolsheviks, had won, but the Civil War was still going on. The role of Austrian prisoners of war in that conflict—for example, there was a big to-do in Uzbekistan, because the British were trying to block the possible invasion of India, because Lenin had announced openly that the way to get a world revolution is really start it in places like India. There, I think, my uncle probably experienced that. He never mentioned it to me. He wasn't very eager to talk

about those days. But I think he was a man very close to me, who already had experiences that are relevant to this whole movement.

Well, let me go back now, forward, to maybe the beginning of 1938. I recall, somewhat, what was discussed by my parents with their friends and relatives, because they had been listening, of course, to the radio and heard Hitler's speeches that the then-chancellor, [Kurt] Schuschnigg, who was the head of what was called the Christian Social Party—which, at that time, was the only party in Austria, because the Nazis had been banned because they assassinated the previous chancellor. The Social Democrats, who were the major opposing party, of course—small Communist party—had been banned in 1934. He thought he could oppose Hitler. Hitler had been agitating, for taking over Austria for a long time, and it became more and more pronounced in late 1937. So Schuschnigg was invited to Berchtesgaden, and Hitler scared the hell out of him, as he did with [Benito] Mussolini. First of all, he [Schuschnigg] made the Nazi Party a legal party again when he came back to get support. He would then have his own plebiscite, and that was going to be called for early March. Again, Hitler said, absolutely not. Schuschnigg resigned, and there was an interim president called Arthur Seyss-Inquart, who only lasted about a day or two. Then, I think it was the morning of the twelfth, I'm pretty sure, that the German troops crossed the border. The capture of Vienna was even earlier, because they sent paratroopers over immediately. That was very exciting for me, of course, to see them coming down.

02-00:11:24

Burnett:

So they came down during the day, in the daylight?

02-00:11:27

Leitmann:

Yeah, very early morning. Hours later, the first tanks arrived. The whole thing was very clear. Also, they had brought in, immediately, by trucks, uniforms for the SA [*Sturmabteilung*] and the SS [*Schutzstaffel*], and for the Hitler Youth. By the second day, these people were in full uniform. It was very well-prepared, and you have to give them credit for that, to be so organized. The very first thing that I noticed from the Jewish side was that, when we looked out of the window on the twelfth already, the afternoon of the twelfth, old Jewish men were scrubbing the streets. The Christian Social Party had painted slogans, "Say yes on all plebiscites." Those were all over the place. That was the first task that was given to Jews, was to clean the streets. Then the second one, and I guess I think that I mentioned this before, was that a troop of Hitler Youth—that must have been maybe on the second day—came marching by the house where I lived, in full uniform, with the drums and the whole thing, and they sang a song, which doesn't rhyme in English, but does in German. "When Jewish blood sprays, or spills, from our daggers, we shall be happy." That was the first song I heard. I wasn't really so terribly upset by that as, obviously, my parents were, because it was a very abstract thing to me. It was still the whole excitement that I felt.

Then, the next thing that hit me was that, suddenly, I was sort of alone. It was difficult to go out on the street. There were no friends. Nobody knew me. I was sort of jealous. I mentioned this before, in other interviews, I wonder whether I would have not become a Nazi myself, if they had been anti-somebody else. The whole idea, being a member of, obviously, a powerful and colorful organization, was very appealing. I really resented, at that time—of course, that changed very quickly—the fact that I was isolated. As I think back on it, and have for a long time, that was probably the worst part of the whole initial experience.

02-00:14:42

Burnett:

What you're describing in those first days was, as you say, very planned. I've read a number of narratives from different people who experienced that same—and we're talking the first five, six, seven days. It's the same stories, over and over again, of public humiliation, public expressions of sadism. There were things that were being done to the Jewish people, but the important feature of it was that it was public. That there were acts in the open that were designed to be watched by other people.

02-00:15:30

Leitmann:

Oh, absolutely.

02-00:15:31

Burnett:

It was a spectacle.

02-00:15:32

Leitmann:

Yeah. Painting "Jew" on stores, in letters this big. The newspapers that came in, like, for example, Himmler's newspaper, the whole tone—and it was the old story, which was, again, not new, because even in the twenties, when there was a reaction to losing the war and all that, the Jews were blamed to some extent, because they made the money on the war. Then the caricatures came up, and they, of course, existed already, long ago, already in some of the caricatures in Great Britain. Rowlandson, for example, had engravings showing Jews with big noses, and always had beards. They were always somehow associated with the myth that the Jews drank the blood of Christian babies—that's, of course, a story that goes way back.

02-00:16:38

Burnett:

The blood libel.

02-00:16:39

Leitmann:

Yeah. But on the other hand, also that they were just dirty old men, trying to screw the Aryan girls. That was a theme that ran for the whole time, right until the end of the war, through all the propaganda. Then there were these contradictions, as I think back about it. Before Hitler made his peace with Stalin temporarily, there was the whole cabal, if you want to call that, of Communists and Bolsheviks together with the Jewish bankers. They were sort of lumped into one group. From a logical point it's ridiculous, but people bought that, because now they had two enemies. They had the Reds and the

Jews. Then, of course, others, like the gypsies and homosexuals. That was a relatively small group, and not as important to people. Of course, being opposed to the Jews is part of the anti-Semitism of decades, centuries. They were still burning Jews in Vienna in the seventeenth century, on pyres. Then there were interludes, like the one that my family lived in. They were very lucky, because it just so happened that the Emperor Franz Josef was liberal in terms of that tradition. Then, of course, the emancipation in the middle 1800s had a lot to do with that. Although some of the names that were given to Jews who had their own names, and then wanted German names, it depended on the money they gave to the clerks. I ran into this directly, because I must have read about it, that when they handed out the names, the new names, depending on how much money they turned over—one name was Scheisseimer, which meant “shit bucket.” Then they had to, later on, change them slightly. That kind of thing was very common.

This kind of humiliation was very important. I think that’s really it. First, to make Jews disgusting from all points of view, and secondly, that they were humiliated. I think those are the two tools that were used for the public. You’re quite right. I think it was the first time that my generation and my parents’ generation encountered that. The Eastern Jews, under the czar, probably encountered that much earlier, but this was the same tradition of just making them, first, second-class citizens, and then, in fact, inhuman. They were not part of the human race. That was the final justification for eliminating them. On the other hand, it was also still possible, certainly for the Jews in Germany—say, from ’33 to ’38—to emigrate, leaving whatever goods they had behind, but at least get away with their lives. That didn’t change until ’40, ’41. It was strictly the idea that they’ve got to go and leave everything behind. They’ve got this whole Aryanization idea.

For that reason, I think, first of all, in the case of my family, given particularly my father’s family with his father having been a relatively high-ranking officer, his serving the fatherland and being wounded and all that, made it very difficult to believe that this is nothing more than a passing phase. So the excitement that maybe we will have to leave, temporarily even, because the Jews get to go here, and then they come back, that didn’t really sink in until about six months later. Even to the extent—we’ve talked about this before, that my family was completely assimilated. If there were any religious people, if there were—in my grandparents’ or earlier generation—still, there was never—and I really feel that I’m proud of that—there was never an attempt to deny being Jews in my family, never. It was for that reason that my father said, I want me to have a bar mitzvah. We’re not going to hide this from anybody. That’s our culture, our tradition. It doesn’t make us orthodox or believers in a larger—I know my father was an agnostic, certainly by his behavior. That, I’m very pleased with. So I was already under the instruction of a rabbi before the Nazis came in, and that continued, and I was able to have a bar mitzvah only about a month after the Nazis came in. They came in mid-March, and I had my bar mitzvah just before my birthday, so middle of May.

02-00:22:48

Burnett:

I think almost immediately after the Anschluss, many of the leading organizations of Jews in Vienna were shut down, I understand. Some of them were revived later, under the direction—so Adolf Eichmann comes in a couple of months later and really starts to take over the exodus, to manage the exodus. So he revives the IKG to get Jews out. That's what he wants. But initially, there was this decapitation of the top leadership, and the humiliation of the leaders, and so on. What I read was that the Jewish communities became grassroots. It was family-to-family communication about these types of things. How do you have a bar mitzvah a month after the Anschluss? It's an astonishing act.

02-00:23:49

Leitmann:

There was no problem about it at all. Not at all. We even had the special dinner, and I got a gold watch, and all that thing—which I still have. So that part of life went on, and it also made it, I think, easier to not have an opposition.

02-00:24:13

Burnett:

So one month after the Anschluss, you were bar mitzvahed?

02-00:24:17

Leitmann:

Right. There was a celebration, and we had friends in and all that sort of thing. That sort of gave it the flavor of life continuing, so there wasn't a major interruption. Also, it gave time to my father, who was sort of the family paterfamilias, to advise people what to do. Now, in his case, in his own case, and his own mother, and of course my mother and her mother and me, he felt that we can take our time and do this the proper way. So the first thing that happened to him was, of course, that his business was taken over by a commissar and Aryanized. Now, I have no idea whether there was a sale involved or whether this was just simply a process. I wasn't in on that kind of thing. One thing happened was that my mother returned home. She no longer needed to go to the office as help to my father, keeping books. She no longer did that. So I did see my mother, essentially, on a regular basis. That also was really necessary, because we could no longer employ Christians. That was part of the laws. Our maid, obviously, no longer could be employed by us. She wasn't happy about that, of course—the maid. So my mother started cooking again and all that kind of thing. That was a change.

The second change that occurred, very quickly, within three weeks or so, is that I was thrown out of school. I was in the gymnasium, and Jews had no place in the gymnasium, because that's higher education. So that was a big change. What it allowed me was to, at least before Bar Mitzvah, to study very hard for that. Then, very quickly, because there was certainly the idea that we need to prepare for possible emigration, to do something about that. One of the first things that I did was to start taking very seriously English lessons. My parents spoke fairly good English, so they thought that this was very

important. My two grandmothers never learned a word of English. For me, that was the number-one task.

02-00:26:53

Burnett: Do you think they were thinking emigration to the United States at that point?

02-00:26:58

Leitmann: Yeah, because we had relatives here. There was no notion at that time to go, for example, to Palestine. There were Jews who already were Zionist before the Anschluss. They, of course, immediately thought of, and did go, to Palestine. There were other Jews who had relatives in South America, for example, where there was, in some of the smaller countries, a relatively large Jewish population. So they had contacts there. There was also the notion that, if this is going to be temporary, let's not go too far. That idea of coming back, that didn't happen after the Second World War. I just checked yesterday. There are fewer Jews in Austria now than there were at the end of the war, when most of them had been eliminated. It's really interesting. Most of those are Jews who came from the Soviet Union, so there are essentially no second-generation Jews left in Austria. What is this? This is seventy years after the war. That is a big change, I think, which astounded me. I think the Jews were, in Austria, 6 or 7 percent of the population. In Vienna, of course, they were 10 percent.

02-00:28:34

Burnett: Two hundred thousand—

02-00:28:35

Leitmann: Yeah, right.

02-00:28:38

Burnett: —in Vienna.

02-00:28:40

Leitmann: Germany was a little bit different. Some of the Jews did go back. Again, it's a small number, but not as stark as in Austria. This brings back the whole idea that the really bad part of anti-Semitism, if you want to call it that—simply getting rid of the Jews—became much more serious once Austria was taken, maybe because Hitler was an Austrian. Then if you look at the composition of the really bad part of the— [Ernst] Kaltenbrunner and people like that—they were Austrians. That always hit me, and it did later on, when I had chances, to this day, discussing this with Austrian friends, why is this? They got away with a lot until the eighties, of course, but that's a later story.

So things went along. I became, after my bar mitzvah, very religious. I did all the rituals every day and everything, which I had never even heard of before until I was instructed. That was until November. My first exposure to really the stark part of this anti-Semitism—there were incidents. Once I got chased down the street, but it was minor. I came, in fact, on Kristallnacht from my English lesson that day, and passed the local synagogue in flames. I made it

home very quickly, but came into an apartment where dozens of people I didn't know, neighborhood people, people off the street, helping themselves to things.

02-00:30:44

Burnett: In your house?

02-00:30:46

Leitmann: In the apartment where I lived, yeah. That big apartment we had. My father lost his stamp collection. He was a very serious first-edition stamp collector. People helped themselves to the radio. It was a fancy radio. People just came in and they helped themselves. My mother, my two grandmothers, couldn't do a thing. The good thing is, my mother was able to call my father—he was helping the commissar in his business, so he was away—to tell him not to come home. So he didn't get involved in this break-in and stealing and stuff. In other cases, I know people were beaten up, for example. That didn't happen with us.

02-00:31:40

Burnett: Did your father still have his weapon at that time?

02-00:31:43

Leitmann: That was the next thing. No. He was a reserve officer, and he had his pistol. I think it was the third or fourth day, they came and got it. So they were that well-organized.

02-00:31:55

Burnett: They knew who had—

02-00:31:56

Leitmann: They know who had weapons, which was very unusual, because hunters in the country, of course, had weapons, but in town almost nobody. No, he had a pistol, and they just came and collected it. There was no confrontation. They said, "The law says you can't have one. You're a Jew. End of the story." I took another look at that much later on. I'll mention it, if I don't forget. So that was the first indication that they were serious about it. But again, it could have been a temporary thing. Laws change, that kind of thing. I flipped from being almost orthodox to an agnostic, to this day, when I found out, either the next day or two days later, that the rabbi had been inside the burning synagogue and also burned. They locked him in, they set the place on fire. So I said, well, I can't prove there is no God, but I also can't believe in one. That was the end of my story. It was something that absolutely flipped me. So I became a Zionist for a while. I joined a Zionist youth organization. We had a little plot of land somewhere. I don't remember where. We were learning how to grow things. All that was encouraged, because it was a way of getting the process of getting rid of the Jews easier. And, from a practical point of view, for example, they did provide funds to allow Jews to leave. For example, whatever money that company still made got put into an account. It was a blocked account. For example, if we wanted to have money to live on, we

would have to apply for it month to month, to get money out of the blocked account. And, even for preparation for emigration—so, for example, very early, late '38, or maybe it was early '39, my father, who was very organized, applied for funding to allow us to emigrate, which they happily gave us. So we were able to pack up some things and already put them in boxes. My father had arranged all that, and he had bought five tickets, open tickets, on the Italian line, that left from Trieste, in Northern Italy. That was very important for us, of course, after that.

He did similar things for the rest of the family. For example, all the rest of the immediate family—my Uncle Paul and his family, my Aunt Rose and her family—they all left by early '39. They all left. Because we had relatives in the United States, what was called an affidavit had to be furnished to apply for a visa to the United States. There didn't seem to be any problem about that. They couldn't take anything with them, not money or anything like that. My grandmother, my mother's mother, even packed her horsehair mattresses, and she slept on those until she moved to a nursing home. They were this thick, and as hard as a rock. Absolutely. She had her own apartment in Berkeley, and she had horsehair mattresses that came in 1938. She retired and moved to Berkeley in '69, and those were the same horsehair mattresses. Life is strange. Just really amazing.

02-00:35:53

Burnett:

So the Aryanization process for your father's business and any of the other businesses that were Jewish-owned or Jewish-managed, it was important to effect the transition to non-Jewish ownership in order to keep the business going, because if it was a Jewish-owned business, they would boycott it, and they would paint the windows and say, "Don't buy from this Jewish business."

02-00:36:15

Leitmann:

And also, it was a way to acquire those businesses, essentially, for nothing. Later on, when there was restitution and they figured out what did we really receive, it was, on the average, for some things, zero, and for some, ten cents on the dollar. We had, I mentioned, a little house in the country at the lake. The deputy mayor of Vienna took a liking to that, so he just took it over. That was easy. But again, in retrospect, it was just all things. We had paintings. My father was a big goer to auctions, which I inherited. He had crystal, bohemian crystal. Some of it is still here. Some of it, we were able to pack in those boxes and bring. My father had a very good coin collection, and a very good watch collection. He was able to talk to an SS officer, who said, "I'll get this out for you to Switzerland," but for a fee, for keeping part of it, whatever. Some of it was in Switzerland at the end of the war. A very small part, but some of it was. I think this was a clever move on his part, because he knew it'd be communicated to our relatives, and there was always the possibility we gave them the number of the account in Switzerland. They would go and check. [phone rings] Sorry.

02-00:37:57

Burnett:

It's all right. [break in audio] So there is this process of adjustment. At a certain point, Jews who had businesses, who had investments, they have to make a decision at some point: when do I cut the losses and go? It's agonizing. Some said, "I can't start over," and they stayed. Your father made provisions for the family to get out.

02-00:38:31

Leitmann:

He was very good about that, and obviously it saved us. There's no question about it. I think by early '39, all but my father had visas. We got the affidavit from the Kinspel family in California, and a branch of the family that I never heard of before by the name of Grynzspan in New York. So there was absolutely no problem. To this day, I'm puzzled why my father didn't—why he didn't get a visa, if his own mother, his wife, his kid—there was the idea that maybe because he was born in Poland—Poland had been part of Austria—that he was considered on the Polish quota. But then I said to myself, I'm pretty sure that his mother was born in Poland, and so why didn't that affect her? I'm really at a loss why this happened.

02-00:39:38

Burnett:

As a military man, there's the story of you make sure everyone else is safe first, and then you go.

02-00:39:48

Leitmann:

Yeah. He was persuaded strictly by his worry about jeopardizing our lives, to finally leave. But there's an interim period here which is, perhaps, from a very personal point of view, something I remember the most. After Kristallnacht—that was November '38—within a month or so, the family that had wanted the apartment house owned by my grandmother—they had a business downstairs, paint and chemical supplies business—they really wanted that apartment house, and they succeeded. We were forced to get into one of those forced sales. Once they knew that they owned the apartment house, they just ejected us. Here were the five of us—my parents, my grandmother, my mother's mother, and I. We moved in with my father's mother, who lived maybe fifteen, twenty minutes away, right on the street along the Danube Canal, in a very small apartment. She had a one-bedroom apartment, so it was a living room, a bedroom, and a kitchen. The five of us lived there for the next year and a half. But we made do. That wasn't so bad. I don't remember being really particularly upset by that.

Now we're in 1939. We had moved to the small apartment. I think it was probably more towards the middle of '39. Then the war started. Was it August '39? Poland was invaded. Then things got much worse, almost overnight. First of all, rationing was instituted. The Jewish rations were exactly a quarter, and some food items zero. For example, no meat, period, at least in my experience. I wasn't a vegetarian yet. I would have noticed. The winter of '39/ '40 really sticks in my mind. We were in that little apartment. Jews could not get any heating material. No coal, no wood. I remember, to

this day, being sent out with one of those big bags of stuff that vegetables come in—

02-00:43:13

Burnett:

A burlap sack or something.

02-00:43:14

Leitmann:

Burlap. Yeah, burlap sacks. To run behind the coal trucks, to pick up the little pieces of coal, and drag it home. It was a bitter cold winter. One of the worst ones experienced for a long time, apparently. That was really all we had. We used to be able to pick up pieces of wood and stuff. Simply the cold—and my grandmother's apartment was also in a very old house, and not renovated, so the toilet was in the hallway, and I remember sitting there freezing. Just as if it happened now, I can feel it. These little kinds of personal hardships, if you want to call it—it's a relative thing, of course—they stick in my mind, and come back, really, in a way. That was maybe another impetus to say, we've got to stop it. I must have told you the story about, one day, my father came home very proud, bringing a piece of meat. He had found somebody who would sell him, for a lot of money, a piece of smoked meat. My mother's mother was a great cook, and she said, "I will prepare that for us." When she started taking it apart, it turned out to be a cat. So we didn't eat it. My grandmother, Fanny, she was a real artist. She could make all kinds of things from cauliflower, just if it were meat. To this day, I love cauliflower. So there's this trivial, in a way, memory. By that time, early 1940s—it was before the Wannsee Conference in early '42, almost two years before that—

02-00:45:29

Burnett:

The Final Solution.

02-00:45:30

Leitmann:

The Final Solution had not been enunciated yet, but had begun, in fact. They were picking Jews off the street and deporting them to concentration camps. Sporadic kinds of things. It wasn't a total, suddenly all of you have got to go to Auschwitz, that kind of thing. We, by that time, knew that there were, of course, concentration camps. We already knew in the thirties. But there was no idea that there were the kinds of camps—nobody knew about ovens and gas. All that was in the future. But it was clear that it was becoming very dangerous to stay, and so my father decided he was going to leave. Once he was safe, then we could make use of the tickets, because, at that time, still, the official policy was to get rid. If you had a way of leaving, they didn't stop you. He knew the area between Austria and Yugoslavia extremely well. He had served there during the war. He spoke Serbian fluently. I don't know how he did it, but he was able to find a guide, and he took four or five other Jewish men with him. They were kept separately on the train, of course. They had a meeting place. I think it was a country inn in—if it was not the Alps, then the Dolomites, I guess. Croatia was the next country, and part of Yugoslavia, of course, at that time. When my mother retired and moved to California in '69, she was able to find, among her papers, and I wish I could find them—I

can't—a little slip that gave the address of that inn, and a password. So there were people, for enough money, who would help you. So he made it. We got a telegram saying the book arrived or something, from Niš [Serbia].

The additional reasons why my father made for Niš, which was in Serbia, of course, were, first of all, that during the First World War, after his second wounding, he was in the military hospital in Niš. And my mother had relatives in Niš, distant relatives, but she had relatives. That's why he made it, and he started working for the Red Cross there. So we still got an occasional card, using the Red Cross channel, until probably a year later. By the beginning of '41, we stopped hearing from him. I have his last communication to me. He sent me a postcard from Niš, and said, "Everything is wonderful here." It was, of course, at the time. "We'll see each other soon." It was written in English. And I have it. We didn't hear anything after that ever again, but finally found out what happened, more or less.

So we made it. It must have been maybe two weeks after we "knew" he was safe—we knew he was "safe," in quotation marks. And took the train, my mother, my two grandmothers, and I, to Trieste. We had passports that had the big J's in them. My mother and two grandmothers had the new middle names. All Jewish women had to be called Sarah, and mine said Israel in the middle. But we weren't stopped. They let us through. We had legal visas in our passports, so they said, okay, four more Jews we're rid of. We stayed maybe three or four days, maybe a little longer, in Trieste, waiting for the next liner to the United States. It was the SS *Saturnia*. Sitting on the Piazza Grande, in Trieste, at a coffeehouse, and having ice cream. My mother and my two grandmothers had coffee with whipped cream. It was almost a new life. We hadn't seen anything like that in a couple of years—longer. We weren't allowed to go to the patisseries in Vienna. Jews were simply not allowed. Couldn't sit on park benches. All that was forbidden. So it already was a good beginning. I tried to say that hell had stopped, personally, and everything after that was wonderful, no matter how difficult it was, for example, for my mother. But nonetheless, it was paradise.

02-00:51:09

Burnett:

A weight had been lifted, to some degree.

02-00:51:10

Leitmann:

Yeah, simply, life was good again. We had fifty dollars with us. That's all we were allowed to take. But we knew we had relatives to go to, so we'd get along, and it was correct. We boarded the *Saturnia*. It left via the Adriatic, of course. First town we stopped at was Dubrovnik, and I have an album—I'd started taking photographs. Then the next one was Patras. All that was excitement. We had a small cabin for the four of us, and I still remember that my two grandmothers had false teeth, and they had them at night in a glass, and put fruit around them to stay on the shelf. It stuck with me.

02-00:52:19

Burnett: The detail, the little detail.

02-00:52:20

Leitmann: Detail. Trivial detail, but nonetheless, it became part of life. I made friends with a girl aboard, and before that, girls were a mystery to me.

02-00:52:35

Burnett: And you're fourteen at this point?

02-00:52:36

Leitmann: I was fourteen, right.

02-00:52:40

Burnett: And possessions had been sent—I guess you can't send it anywhere. Is it in storage in Vienna?

02-00:52:48

Leitmann: No. My father had already made contact with a moving company called Spediteur in Vienna. Everything was in French. Half of Viennese is really French. The big container—it was probably a container the size of this table—had already been packed and put with us in preparation for eventual emigration. So that was sent ahead. Yeah, that was sent ahead. That awaited us in New York. My grandmother's horsehair mattresses were in there. Pots and pans. Crazy things we put in the container. A couple of paintings we put in there, this one up there by Schindler was in there, and it got here, because he wasn't a famous artist to the Nazis.

02-00:53:50

Burnett: Right, it was decadent art or something.

02-00:53:52

Leitmann: Right. Anyway, the war was, of course, already on for almost a year, maybe nine months, by that period. Yeah, nine months. We were stopped by a British submarine off Gibraltar. I remember that very well. It was exciting to see it come up, and then the landing boat came over. They were checking the passenger list, and they took some Germans off.

02-00:54:28

Burnett: Wow. Because they were looking for spies, or looking for—

02-00:54:30

Leitmann: I don't know what. All I know is that we had German passports. I guess they knew what the "J" was. I don't know. Anyway, we were clearly two old ladies and—next stop was Lisbon. We stopped in Lisbon—that was the last stop in Europe—and almost missed the boat. My grandmothers stayed aboard, and my mother and I said we'll do some sightseeing, and almost couldn't find our way back to the dock. Almost got stuck in Lisbon. That's another one, just little experiences. But we made it. I think it was a five or six-day trip, then, from Lisbon. It was a fairly large boat. It was about thirty thousand tons. The

Italian line had two of them, the *Vulcania* and the *Saturnia* and arrived in New York then. I don't remember the Statue of Liberty. We didn't have to go through Ellis Island. We arrived on the dock, I think in the Hudson, actually. There were docks there. Debarked—

02-00:55:55

Leitmann: Disembarked. I was absolutely crushed. I couldn't see any Indians.

02-00:56:02

Burnett: Your idea of the United States was the West.

02-00:56:05

Leitmann: Yeah, right. My favorite reading—Hitler's also, by the way—

02-00:56:11

Burnett: Tom Mix—

02-00:56:12

Leitmann: Was Karl May. He was the famous author, and I read many of his books. I remember the names—Winnetou, Old Shatterhand. These characters stick in my mind. It's crazy. So I was very surprised, but not for long.

02-00:56:32

Burnett: So you were not impressed with the skyscrapers? You were just disappoint—

02-00:56:36

Leitmann: It was the disappointment, really, of not seeing any—there was an elevated train going by, and I figured maybe Indians are up there or something. Then we stayed a couple of weeks with Aunt Rose, in fact. They had moved into an apartment in the Bronx. So I found my cousins again, my three cousins, and my Aunt Rose and Uncle Fritz. It was wonderful.

02-00:57:15

Burnett: Many Jews who tried to get to the United States were, at a certain point, turned away. But in your case, it was, A, early enough in the process, and B, you had relatives who provided some kind of a sponsorship for you.

02-00:57:31

Leitmann: I'm sure that these really egregious ones—there was a boatload of children that was turned away. They had these children's transports from Holland into Great Britain and so on. The whole boatload got sent back, and they all got killed.

02-00:57:52

Burnett: Some wrenching experiences. In that early period, in Vienna, Adolf Eichmann develops his expertise, as it were. [Reinhard] Heydrich is involved. After that, Eichmann goes to head up the Gestapo. There is a sense in which there was some experimentation in cruelty taking place in Vienna, and you and your family and others were subjected to that.

02-00:58:24

Leitmann:

Yeah, but it was preparation, because the real Final Solution wasn't enunciated until, I think, February '42, at the Wannsee Conference. Again, there was no public announcement about this. Only public announcements were after [Ernst] vom Rath had been killed, and caused, presumably, the Kristallnacht, there was a tax imposed on the Jews and that kind of thing, but it was minor in comparison to what was in store for us.

02-00:59:03

Burnett:

You land with some of your family safely in New York City. When I was thinking about your biography, I was thinking an adolescent boy—fourteen years old is when, if people are thinking about do they stay together or get divorced, they say it's a bad time to get divorced, because that's such a formative age. Here you are, family is not completely together, and the world is in crisis. I want to ask you about coping with that, and the role of your mother and your grandmother and your family. How do you get through those years?

02-01:00:02

Leitmann:

We were still thinking that my father was safe, of course, because the Germans marched into Yugoslavia in '41, so we still heard from him. That part of it was actually the major concern. My mother became a cleaning lady for about a year, and then the preparations for the war, and eventually the war in the United States, and she got a job in a company that made industrial diamonds, in fact owned by a refugee family. She was a very strong woman. She never made me feel that she was sacrificing anything for me, for example. First thing she did is she got me a bicycle. I think it cost five bucks. It was a Raleigh, a single-speed Raleigh. I biked all over town, all the way into Manhattan. We lived, eventually, in Queens. The whole thing was as if we had gone to paradise. I can't characterize it any other way. I immediately enrolled in high school, only three or four weeks left, and did very well. Got an A+ in Latin, after what they thought was three weeks of schooling. They didn't know that I'd had almost three years of Latin. Didn't disabuse them. And it was great. I can't characterize it any other way. It was a happy time. My Uncle Paul, we finally moved in with him and his family, for about a month. The high school I went to was almost across the street from there. We got an apartment in Jackson Heights, in an apartment house. I think it was on Seventy-Second Street—3715. See, the number popped into my mind just now. My mother got a very nice little apartment. My mother, her mother, my grandmother Fanny, and I lived in this little apartment. I slept on the couch in the living room. They had a bedroom. It was rent-controlled, and I know that because when my mother moved in '69 here, she paid the same rent that she paid in 1940 in New York. I think it was seventy-two bucks.

02-01:02:58

Burnett:

As you said, paradise.

02-01:03:23

Leitmann:

It really was. Then my mother got this good job. We didn't live high and grand, but we had everything we wanted. There's no question about it. So when the war started, for me, it wasn't really until I graduated from high school, which was '43, December of '43. I graduated mid-year. That was December '43. My mother still had her mother with us, so at least she wasn't left alone when I left for the army. That was a big consideration for me, because by that time, of course, I was eighteen.

02-01:03:58

Burnett:

You had English lessons, which is not the same as speaking a language comfortably, immersed in an environment. How quickly did you adapt to—

02-01:04:09

Leitmann:

Three weeks.

02-01:04:00

Burnett:

Three weeks?

02-01:04:10

Leitmann:

I'm not kidding you. I made a few mistakes. I remember, in English class, we read—maybe it was Walt Whitman. Anyway, that kind of poet. "Come, my children"—you know, the first line. Somehow, Arkansas was mentioned, and I said "Ar-kansas." The class laughed, and the teacher said, "No, no, no, he's right; we are wrong. Where does the 'ah' come from in A-S?" So I remember that. Obviously, I made mistakes—I'm regaining my accent, by the way, and forgetting English words, and suddenly German words pop into my head for some things. It's really crazy. I couldn't remember, the other day, the blanket for the beds.

02-01:05:03

Burnett:

Like the duvet?

02-01:05:04

Leitmann:

But the German word is "Decke," and that came into my head. The brain is a strange organ. Anyway, everything was fine. I went through high school. My friend, Werner Ganz, who came from a family in Aachen, became my best friend. We still, to this day, correspond with each other. Now it's by phone, because he's six months younger than I am. After gymnasium—I guess that had a lot to do with it—high school was a cinch.

02-01:05:47

Burnett:

Is it fair to say that your European education was more advanced, say, with respect to mathematics? Were you ahead in math?

02-01:05:55

Leitmann:

I think it was more—I wouldn't say organized, but it made what we had to do easier, by comparison. What was demanded of us was less than what I had been used to being asked to do. Maybe that made learning easier, because—and there was, of course, the war had started. I got a job right away on the

weekend, working for a drugstore in Flushing. It could be reached by elevated train from Jackson Heights, which was on the station. Very nice family, a big German lady and her little Jewish husband. She told me later on, the real love of her life was killed in the First World War. A very kind woman. She would stuff my pockets with toilet articles to take home every weekend, that kind of thing. I was delivering the Sunday paper on December 7. It's just amazing. So people found out from me what had happened. I had a job, as I say. That was really my first job. Obviously, since I had worked there already in December '41, I must have had that job—probably during the summer, started it during the summer before. I had that job, essentially, until I graduated from high school. It was a weekend job, of course.

02-01:07:53

Burnett:

Sorry, I'm going to interrupt. During those years, your family had contacts with some refugee families. There was obviously some kind of social network. Did you encounter anti-Semitism in New York at that time, or was it just a different—

02-01:08:17

Leitmann:

There was some of it obvious, because it was legal. For example, you could refuse Jews, blacks, other people—legally, you could refuse to rent to them. I still remember that, when we were looking for an apartment, my mother, A, was looking for one that was inexpensive, but B, that was not restricted, because the word in the ad was “restricted.” That's what that meant. It was a key word that really told you, don't even try. That certainly was an aspect of anti-Semitism. But other than that, I can't think of—for example, in school, never encountered that. We, by that time, of course, knew better what was going on in Europe, because the newspapers reported it and so on. I found out later on that there was a certain amount of social anti-Semitism, in the United States. Even President [Franklin Delano] Roosevelt suffered from that. He loved Jewish jokes. A number of his cabinet ministers were Jews, but there was still that kind of social anti-Semitism.

02-01:09:39

Burnett:

And quotas in the colleges.

02-01:09:40

Leitmann:

Quotas, for example, right. That kind of thing, in medical school, particularly. There were just too many Jewish doctors, everybody knows that. I never felt it personally. No, not once.

02-01:09:58

Burnett:

In the news reels and news reports, as the United States—December 7 is—there was not a lot of enthusiasm for going to war initially, but Pearl Harbor changes that. When the United States gets involved in the war, and you're still in school, how was the fight framed in the press? How did people talk about it, when the United States was now going to go to war against Europe?

02-01:10:33

Leitmann:

There were, of course, very strong factions. There was the anti-war, or isolationist, group, which was very strong, and we knew about that, of course. The Jewish newspapers in New York talked about that. There was a newspaper called—it was a German refugee newspaper, *Aufbau*, or something like that, which we got, just to see what was going on in that part of the world. So we knew about isolationism, we knew about Father [Charles] Coughlin, for example, that kind of thing, but didn't experience it personally. We knew that it existed—weren't even surprised, frankly—but never suffered personally from it. My mother didn't ever not get a job because she was Jewish, that kind of thing. We weren't not allowed to rent. It wasn't a highfalutin neighborhood, but you know. I thought about it, because you asked me that. I can't think of a personal incident that would make me believe that. There was the knowledge that this wasn't an anti-Semitism-free country, but then we knew that where is there one? That wasn't surprising.

02-01:12:13

Burnett:

It sounds like your mother protected you, in the sense that she wanted you to have a normal childhood—

02-01:12:21

Leitmann:

Oh, absolutely.

02-01:12:22

Burnett:

—and adolescence.

02-01:12:23

Leitmann:

Yeah. It was really adolescence by that time. I had discovered girls in high school. The world was great. My mother's jobs were fairly well-paid. Later on, after the war, they didn't need industrial diamonds anymore. She worked for a costume jewelry company. She became a designer and all that. She was the forelady there. She was a very strong and very pragmatic person. By that time, we had very little hope that my father had survived, because right after the war, in the late '40s, she wrote to the Red Cross, and they informed her that, if my father was in Niš, the chances of survival would have been very small, because it was well-known that people got wiped out there. It was nothing specific. That came later. So she became officially a widow at, I think, the age of thirty-eight, and stayed a widow for fifty-four years. Very interesting.

02-01:13:49

Burnett:

You graduate high school in December of '43. Already, the Allies are in North Africa, and they're making their way towards—they land in Sicily when? In '43.

02-01:14:08

Leitmann:

Forty-three, I think, yeah.

02-01:14:09

Burnett:

So they're making their way up that way. What was next for you? Did you get a job after high school? Are you subject to the draft at that point?

02-01:14:23

Leitmann:

Yes, yes. Oh, yes. Again, an irony. When we first arrived, we had to register as enemy aliens, and I still have my enemy alien card. They never did anything bad to us, but that's what we were, enemy aliens, because we had German passports. The bureaucracy always sort of has interesting little quirks to it.

02-01:14:51

Burnett:

Depending on whether you're needed for a specific—

02-01:14:55

Leitmann:

Well, that, also. Even the czar suddenly loved the Jews. Remember, there was a big announcement that he made. It was called "To My Dear Jews."

02-01:15:07

Burnett:

This was when? This was in the Romanoff—

02-01:15:08

Leitmann:

It's the First World War, of course. The war started, and suddenly—"To My Dear Jews." It's all relative. Just personally, I had a very good time, because my favorite uncle was alive. We spent a lot of time with him and his family, my cousin Elfriede—called Elfie. He had a very good job. He was the technical director of a big brewery in New Jersey. So they bought a house. In those days, probably cost \$2,500. We spent lots of time together. I have all kinds of pictures. I was always performing little tricks. I turned wheels. That was my big thing. When I graduated—the graduation was in December—I knew that I was going to go in the army. I wanted to do that. But I did want to also explore whether I could get a profession in the process. There was a program in the US Army. You could apply for that. I think it was either the V5 or V8 program [Army Specialized Training Program] The Navy had V-12 to get trained as an engineer. I applied for that, and what came out of that is that, when I was inducted, they put me in an engineer combat battalion. But that was fine. I loved army food. It was crazy. It's really crazy. I caught pneumonia the first week. Keeled over at reveille. It was in New Jersey, before we got shipped to basic training. Got good treatment. All that was not a problem.

02-01:17:11

Burnett:

What's the base in New Jersey that you were at? I'm trying to remember the—

02-01:17:14

Leitmann:

I don't really remember the one in New Jersey. We were there only a couple of weeks. It was a transition camp, but then we got sent to Colorado, Camp Carson—what's now called Fort Carson. All the camps got to be forts after the war, don't know why. It was a combat engineer battalion that went into

basic training there. Again, I really had a good time in the army. As I say, I loved the food. I became a corporal, I think, while I was in basic training. Friday afternoon, before people could go on pass, they had to spend an hour with me. I became the information NCO [non-commissioned officer], discuss the week's news. That's where I found out that, presumably, the Red Army had prostitutes for their soldiers. I remember all my fellow GIs were terribly interested in that. So I tried to find out. I wrote a letter to the Soviet embassy. That must have been between maybe February, March, and June '44. So just before the invasion. They answered. I got a letter back, with the return address, Embassy of the USSR [Union of Soviet Socialist Republics]. I got a one-line answer: "The information you have is incorrect."

02-01:19:09

Burnett:

So you're doing your basic training, and you're advanced training, February, March. This is already the build-up to D-Day, basically.

02-01:19:17

Leitmann:

No, no, D-Day actually occurred before we went into advanced training.

02-01:19:24

Burnett:

June of '44 would have been—

02-01:19:26

Leitmann:

Right. Advanced training was at Fort Leonard Wood in Missouri, and I became a citizen there. Because in those days, you could not be sent overseas without being a citizen. That's obviously no longer true.

02-01:19:44

Burnett:

Wow. So you did advanced training, and just briefly, could you talk about the role of the combat engineer in combat? What you were told you were going to do, and where they sit in military operations.

02-01:20:04

Leitmann:

Well, they're a little bit worse off than the infantry, because the ordinary engineers were essentially construction companies. Combat engineers were supposed to go ahead of the infantry, to clear mines, to repair bridges, to ferry infantry across rivers, to build pontoon bridges, that sort of thing. I was put into the reconnaissance section of that battalion, which goes ahead of the rest of the battalion.

02-01:20:40

Burnett:

I was going to say.

02-01:20:41

Leitmann:

But it was actually safer, because it was usually just a Jeep driver and me. The idea was for us not to be caught, because we were supposed to find out what was going on. When we heard gunfire, we hid, because it would destroy us—and I didn't have to work as hard.

02-01:20:59

Burnett: Okay. Wait, why is that safer than—

02-01:21:02

Leitmann: Well, because you could hide. When you start hearing shooting, your job was not to participate. What the hell? The two of you aren't going to be able to do anything, and so your job was to find out what was going on and report it. We didn't have wireless radios in those days. The idea was, you hear shooting, you go hide.

02-01:21:29

Burnett: Also, the thing about radios, too, are you—because you could have had a wireless radio, I suppose.

02-01:21:37

Leitmann: We certainly didn't have them.

02-01:21:38

Burnett: Okay. I was wondering if they would also give away your position if you were using—

02-01:21:42

Leitmann: Possibly so. We were still stringing wire.

02-01:21:46

Burnett: Geez. Nonetheless, vulnerable to sniper fire and that kind of—

02-01:21:53

Leitmann: Oh, sure. In fact, I ran into this once. Some sniper was shooting at us from a church tower. But that's piddling.

02-01:22:05

Burnett: It's a part of the job.

02-01:22:08

Leitmann: In fact, I tried to do, the other day, enumerate all the situations I got into that I could have easily got killed in and didn't. It's surprising. For example, we already had people killed in basic training, because we used live ammunition. One part of it was to learn infiltration, and we had an obstacle course using live ammunition. You had to crawl and get under the barbed wire and stuff, and keep your ass down. The machine guns were automatic machine guns that were spraying maybe this high above us, two, three feet. This was in advanced basic training. Somebody very close to me rolled over into a ditch, and there was a snake in it. He jumped up, and he was just cut in half, because those machine guns were automatic. They couldn't be turned off. It was a night course, and every fifth round was a tracer.

02-01:23:17

Burnett: So they had tracers?

02-01:23:18
Leitmann: Tracers, right, so you could see. We had one person killed in what was called village fighting, he was shot in the back.

02-01:23:30
Burnett: What was village fighting?

02-01:23:31
Leitmann: That's the idea to infiltrate—how to take—

02-01:23:34
Burnett: Oh, house-to-house.

02-01:23:35
Leitmann: House-to-house. It was called village fighting, and we used live ammunition.

02-01:23:40
Burnett: And that was training for village fighting?

02-01:23:41
Leitmann: Yes, yes. The first two casualties I came across were during training. That poor guy shot in the back—well, first of all, the M1 [M-1 Garand rifle] was vicious. Made a big hole when it exited. We didn't have an ambulance standing by. He was hauled back to the first aid station in a truck. And, interesting sort of little side lights, at Camp Carson, there was a German prisoner-of-war part to that camp, and it was the *Afrika Korps*. That's another thing I remember. We had to go out into the mountains to build roads and that kind of thing. Here we were, being moved in trucks, of course, out of the camp, drive up into the mountains, and marching in the other direction, singing Nazi songs, in their shorts, because it was the *Afrika Korps*, they were going to the circus.

02-01:24:56
Burnett: They had their desert gear, their desert uniforms.

02-01:24:59
Leitmann: Right, absolutely. Every once in a while, we found one of them hanging in the barracks. He was, I guess, considered to be a traitor for being cooperative with the Americans. That happened two or three times. They were singing Nazi songs.

02-01:25:17
Burnett: And they were doing work, or they were working in fields, as part of their—

02-01:25:22
Leitmann: Well, not very much. Not very much.

02-01:25:24
Burnett: At that stage.

02-01:25:27

Leitmann:

Yeah, they were still very gung-ho. It's really amazing. I can even recite some of the songs, still. "*Wir gehen gegen England*." That kind of stuff sticks in the mind.

02-01:25:41

Burnett:

What does that translate to?

02-01:25:43

Leitmann:

"We're going towards England." They were planning the invasion, of course. At that time, they were still planning the invasion of England. They were sure they were going to win the war. No question about it. It wasn't until October that we were actually being shipped over to Europe. So it was well after D-Day. We still landed on the beach, because we got about another six weeks of training in England. So this must have been early December. Again, a very cold winter. Two things, really, that stick in my mind. We were moving towards the British coast to cross the channel, and every once in a while the troop train would stop at a station, and those nice English ladies would, in the middle of the night, two o'clock in the morning, stand there and hand out tea. But of course, to the Americans, it looked like coffee to them. They spat it out right in front of them. This sticks in my mind. We were transported across the channel on a Polish ship called the SS *Sobieski*. We had to go on those rope ladders down the boat into landing craft. They had fed us a typical British breakfast. It was kippers.

02-01:27:20

Burnett:

Oh, God.

02-01:27:22

Leitmann:

With the boat going up like this [motions indicating heaving of the seas]. [laughter] So we landed. It was already dark on the beach, so we bivouacked just a little bit up the beach. It was very cold. We had two-man tents. We stayed there three or four days. But when we woke up in the morning, the first morning, that was the first time we all thought we would not survive the war, because we had bivouacked in an area full of unexploded munitions.

02-01:27:55

Burnett:

Wow. Geez.

02-01:27:56

Leitmann:

Yeah. That's the first time that occurred to us. I remember that.

02-01:28:03

Burnett:

I was going to ask you, during your training, and the sequence of training and the more advanced training, did you feel trained? Did you feel ready for what you were going to face?

02-01:28:16

Leitmann:

Well, we thought so. That feeling disappeared very—well, I'll give you the first instance. Paris had been taken, so we were going into France, beyond

Paris, on a troop train. There were no toilet facilities, nothing. We had the regular cattle car, so it's eight horses or forty men kind of thing. So it was very important, when the train stopped, to go and do your business, because—in the middle of France. I got out. My pants were down. The train started to move. They pulled me up, finally, but it starts very slowly. So I almost got left behind in France. The winter was beginning. Our uniforms were completely insufficient. We had combat boots. They latched up like this, with the soft part on the outside, so once they got wet, they never got dry. It was a crazy idea. The absorbent part was outside, the smooth part inside. We didn't have winter uniforms. The prisoners of war who went in the other direction still had winter uniforms. We had no winter uniforms. We arrived in Alsace-Lorraine, in a town called Épinal. We got put, for two or three weeks, in the old Maginot Line.

02-01:30:01

Burnett:

A bunch of caves, basically, right?

02-01:30:03

Leitmann:

Bunch of caves. The only light we had was sand in cans, food cans, and poured the gas in, and then we had light, because there was, of course, no light. When we emerged, we were black. We hadn't washed in two. We finally got out, after a couple of weeks. The first thing they did for us, they did have a unit that gave you a shower. Gave you five minutes to take a shower, and you got new uniforms. The uniforms, they just threw at you. They would fit or not. Look at you and say, "Yeah," you know. I really needed a shave and a haircut. I found a barbershop that was open, and of course, that had been part of Germany by that time. The young people, of course, were all trained in German. I got this nice haircut, and the barber was going to shave me with a straight razor, and he was assisted by his daughter, who must have been in her twenties. Of course, I was a GI. As far as they were concerned, I didn't speak German. As he had his straight razor across my throat, she said to him, in German, "Why don't you cut his throat?" I scared the hell out of her. I opened my eyes and I said in German, "That was not a very friendly thing to say." I thought she was going to faint. She thought I was going to shoot her. These little episodes, you know, those I remember.

02-01:31:54

Burnett:

There's obviously a tremendous amount of planning around the invasion, but you can't plan everything, and there's supply issues.

02-01:32:01

Leitmann:

Unexpected things happen.

02-01:32:04

Burnett:

And that was a cold winter. It was one of the coldest in recent years.

02-01:32:09

Leitmann:

Thirty-nine, forty was a terrible winter, and then the '44/'45 winter was another very bad winter.

02-01:32:18

Burnett: So you shipped to the UK in July or August of '44, and then you're in France in December of '44. I'm trying to think. The Battle of the Bulge—

02-01:32:32

Leitmann: That started. As a matter of fact, we thought that's where we were going.

02-01:32:39

Burnett: The fighting is in northeastern France, and the front is across into Belgium. The Bulge is this counterattack that the Germans—

02-01:32:49

Leitmann: The last really big battle, yeah.

02-01:32:51

Burnett: They drive through the middle, and they create this bulge in the line that goes in—

02-01:32:55

Leitmann: The weather was bad, so the Air Force couldn't be used. So we couldn't drop bombs on the Germans, and the Germans actually retook about forty miles of territory. They got pretty close back to the coast by that time, because there the coast is much closer in Belgium. It wasn't until the weather cleared that we could bomb the hell out of them, and of course that was the end of that. In fact, we did not go to the Battle of the Bulge. We went to a neighboring battle. There's a veteran where I live now, who was a veteran in Battle of the Bulge, and we almost met, because we were only about fifty miles apart. Anyway, the French were coming up from the Mediterranean. They had landed in France from the Mediterranean. It was the First French Army. Rhine and Danube, their patch. In fact, it's on my uniform. We were attached to the First French Army, because they had no tank dozers. Combat engineers had tank dozers, and so we got attached to them.

02-01:34:07

Burnett: And those are used for clearing—

02-01:34:09

Leitmann: Clearing mines, mostly, and obstacles. Again, there's certain little things that stick in my mind. We had a crazy executive officer in that battalion, Major Spence, whose idea was, the way you find mines is to drive into the field, and if there's a mine there—and that happened to us. Fortunately, I was in the back of the Jeep, but the Jeep driver got killed, and he got very badly—Major Spence was in the hospital for months after that. He was a nut.

02-01:34:44

Burnett: Did he feel like he needed to do that to inspire the men, or he was—

02-01:34:47

Leitmann: I don't think so. I think he was just a nut. In fact, the main battle there was planned at that time, say, two or three weeks later, was to retake the capital of

Alsace-Lorraine, Colmar. The Germans fought like crazy there—it was the last patch of land, occupied land—because then they’d be driven back across the Rhine. It was something that they really wanted to keep, just symbolically, if nothing else. Maybe I’ll go back a little bit to England, because there’s a connection between that. We were cleaning up one of the British great houses. The family had buttoned it up. All the valuables were in separate rooms. We were cleaning it up for part of Eisenhower’s headquarters. Somebody, opening a closet, had found—they had put wallpaper across the closet—had found a place where it was open, and he found that there was an entrance into the next room with all the valuables. So people stole stuff like crazy. Next morning, they shipped it home already, and we felt that, as punishment for doing that, we were going to be sent to the Battle of the Bulge. I remember that very clearly, because, two days later, we were on the boat.

I also remember that, in Colmar, where I’d been dropped off behind the German lines—there were no definite lines, but still, unoccupied territory—and we knew that our battalion was supposed to come in through a particular part of town, because we had places where tanks and tank dozers could come through. The Germans had, very cleverly, found out that on the big roads, where tanks and trucks could come through, they built massive brick walls across the road, and just left enough room on each side for a person to go through. My task was to find out if that had happened on that route, because that would have been a disaster to get the whole battalion stuck—

02-01:37:25

Burnett:

And vulnerable.

02-01:37:25

Leitmann:

—then have to back out.

02-01:37:27

Burnett:

And vulnerable, yeah.

02-01:37:29

Leitmann:

I was there maybe not quite three days, in the suburb—yeah, in a suburb—in a big mansion. Here, I was all by myself. It was perfectly safe. Nobody was going to come in. The Germans knew that there were no Americans there yet. I could hear the patrols outside at night, marching by. Suddenly, a shell hit that mansion and began to burn. I said, well, I’m getting out of here, but being a GI, I’ve got to have a souvenir. So I went into the library, and I found a folder of woodcuts. Just a little thing, maybe fifteen woodcuts. I looked at it, and it was by a German artist—at least the guy who had signed it—Otto Abetz. It was called *Eine Sizilienfahrt*. It was a trip of Boy Scouts to Sicily. Maybe a dozen woodcuts in there. That became an important name for me, because I found out later on, after the war, reading a magazine, that he had been put on trial. He wasn’t the major war criminal. He had been the German ambassador to Vichy France. Now, when he was an artist, that was in the late twenties, and he was a Francophile. That’s why they made him the

ambassador to Vichy France. Then he became, also, some other big wheel. Otto Abetz. I'll leave that for a later story. He came up, even before that, again. Otto Abetz came to me three times. Very strange.

02-01:39:31

Burnett: So you were in advance of the forces, trying to determine the route.

02-01:39:36

Leitmann: They sent the Jeep and driver back, and just left me behind.

02-01:39:41

Burnett: How did you feel about that?

02-01:39:44

Leitmann: I wasn't scared. It was very exciting. How old was I? It was 1945.

02-01:40:00

Burnett: Early '45, yeah. At this point, the Germans are in retreat, but they're—

02-01:40:07

Leitmann: So I was nineteen. Right?

02-01:40:08

Burnett: Yeah.

02-01:40:09

Leitmann: Born in '25. Yeah, nineteen.

02-01:40:14

Burnett: Just a question about lines, because you think of the First World War as these really rigid, solid lines, but there are these open—

02-01:40:22

Leitmann: There was no trench warfare.

02-01:40:23

Burnett: Yeah. There were these open spaces and highly mobile forces.

02-01:40:28

Leitmann: Yeah, there'd be your troops here, and an hour later, there'd be other guys there, driving through. Everybody was moving. There were periods where they were stationary. For example, at that time, once we reached the German border—it was right after Colmar—we went maybe twenty miles north, and that was the border between France, Belgium, and Germany, in a town called Saargemünd, which in French is Sarreguemines. There, we were almost two weeks in preparation for the attack, through what was called the Siegfried Line, sort of the counterpart to the Maginot Line, and as engineers, that was important. We built a bridge there, because it was still, for the Germans, possible to bomb us, so that bridge had been blown up by a German plane, one of the last few that was able to do that. I also had a girlfriend there. That's a funny story, because the Germans were exactly a block away. Somehow, I

made friends with—her name was Jeanette. She convinced her parents to stay in the basement at night, because it was dangerous to be up there. So that was very good. I crawled on my stomach across the street, because the Germans were only a block away, to get there. Sticks in my mind. I have a picture of her. I still have a picture of her.

02-01:42:02

Burnett:

So, in a sense, amidst all of this death and destruction—

02-01:42:07

Leitmann:

There's life.

02-01:42:07

Burnett:

—and danger, there's life, and a vitality. You felt alive.

02-01:42:11

Leitmann:

Yes, yes. Absolutely. Absolutely. It was scary, in a certain sense, but for a nineteen-year-old, it's very exciting. I can see how people become enthusiastic. Now, in trench warfare, I can see that you couldn't become enthusiastic, but when it was this kind of thing that things were moving, most of the time, you just sat there, because it—

02-01:42:39

Burnett:

Nothing was happening.

02-01:42:40

Leitmann:

Nothing was happening, sometimes for days. It was really very different. Yeah, go ahead.

02-01:42:53

Burnett:

I read in *Building Bridges to Victory* that Colmar was—there was some village fighting in there. Was that—

02-01:43:02

Leitmann:

Outside, there was a town called Kaysersberg, on the way to Colmar. No, there were certainly skirmishes, that kind of thing, but there was nothing really stationary. The Germans really wanted to hold that town, and they did something very clever, which we should have thought about. Town was badly damaged. The place to stay were the public buildings. The school—the bivouac. It was a town, so you didn't have to sleep in a tent. I think maybe the second or third night after my unit came through, we spent the night in—I think it was a school. So it was another thing. I said, how come I didn't get killed? I woke up in the morning. Didn't hear a thing. The Germans had zeroed in all the big public buildings, because they thought that's where the Americans are going to bivouac. The roof was gone! I slept through the whole thing. I was in the attic. I had a sleeping bag, and I had all that over my head. I pushed it down. I was in the open. That happened twice. There, and then later on, when we crossed the Main River. It just somehow goes with the whole territory. I don't remember any time when I, and I think the other guys, felt

really that that was going to be their end. Everybody believed that they're going to make it.

02-01:44:33

Burnett: Is there an adaptation to danger? Do you feel that you kind of adapt, you adjust—

02-01:44:38

Leitmann: Yeah, I think so, particularly if you make it. I think the worst part of that winter, for me, was that I got frostbite. I have it today. Today, my feet are as cold as can be, because my dorsal arteries were clogged. Seventy-plus years later, I can still feel the war in my feet.

02-01:45:03

Burnett: You've got the damage.

02-01:45:03

Leitmann: It's really true. So, no, I don't think. There were things that bothered me, simply from a human point of view, the kind of inhumanity that creeps in. When we finally got through the Siegfried Line—personally, I never fired a shot—I had to hide a few times—we were ferrying infantry across the Main River in pontoon boats, and then landed, because the bridge hadn't been established, in a town called Worms. The trucks could come across by that time, because we had put a pontoon bridge in, and so trucks—tanks couldn't, but the trucks would come through. Again, we got to a place, a clearing, and pulled into the clearing to rest that night. One thing is, you sleep well, because you're so damn exhausted. I think that's one thing. I woke up in the morning. It must have been daylight, but it looked, still, very dark. In the middle of the night, trucks had pulled in, and one of them was on top of me, and never hit me or anything. I woke up under a truck. Maybe I have a guardian angel. Who the hell knows? That kind of thing happened a number of times, even after that.

02-01:46:41

Burnett: In reading that book and other tales, often the enemy is abstract. You're trying to achieve objectives, you're trying to take ground, and you're just given these orders.

02-01:46:54

Leitmann: Yeah, it's very rare that you see them in person. Of course, it happened, but—

02-01:47:00

Burnett: Yeah. Sometimes, there was a story of Germans who surrendered to a platoon, and they were just very ingratiating, and they immediately asked for cigarettes. The American soldier raised his rifle right to them. He just wanted to shoot them right on the spot, because of something that had just happened, where a bunch of his buddies had been killed.

02-01:47:29

Leitmann:

I think that that kind of gut reaction happened, of course, but it wasn't the normal kind of operation. The Germans, very often, as policy, did that kind of thing. For example, during the Battle of the Bulge, they didn't take prisoners. Somebody surrendered, they just shot him. Not as a personal thing. Those were the orders, because they couldn't afford to take prisoners and send them back.

02-01:47:55

Burnett:

They're in a retreat at that point.

02-01:47:56

Leitmann:

Yeah. Logistics didn't allow that. There are two aspects to that, of course. I volunteered for a number of years, and I still, this year, did it, with a middle school.

02-01:48:18

Burnett:

Prospect Sierra?

02-01:48:18

Leitmann:

Prospect Sierra. A very good school. They have a yearly project on the Second World War, and they're supposed to interview eyewitnesses, not just during the war, but, for example, people who were in Japanese internment camps, that kind of thing. Very important thing. There are not too many of us left. They always try to make a case—I said, the war is not parades. It's just hell, no matter how you look at it. It may not be a personal hell, but just the whole idea. I remember, for example, I was on a little reconnaissance with my driver again, and we came to a stream—this was still out in Saareguemines, doing, still, reconnaissance before the Siegfried Line—and came to a little rubber raft with bloody water in it. We had found out that there were two Americans who had been shot there, and bled to death in that boat. That happened, but it wasn't any prolonged thing, like—the First World War must have been incredible. You go over the top and get mowed down right away kind of thing.

02-01:49:43

Burnett:

And the shelling, and the—

02-01:49:45

Leitmann:

The shelling, and living in the mud for weeks, getting all kinds of diseases and stuff. There was very little of that. There was a little of that, maybe, in the Pacific, when they landed on islands, but there wasn't too much of that. It usually got very bad when there was an offensive like the Bulge, when they came in with a lot of tanks and stuff. Obviously, many people got killed that way. But there wasn't that kind of thing of just sitting there, waiting to die.

02-01:50:20

Burnett:

It was fast-moving for you, definitely. For you and the people you were with, things—

02-01:50:25

Leitmann:

Yeah, and the war was essentially won, which made a lot of psychological difference. We never had any doubt about the fact that the war was over. It didn't mean you weren't going to get killed, but the war was over.

02-01:50:40

Burnett:

Well, perhaps we should pause. We're now, actually, in the story, we're entering German territory at this point. Then we'll pick up next time.

02-01:50:51

Leitmann:

Okay. Did we do, what, two hours already?

02-01:50:53

Burnett:

That's right.

02-01:50:54

Leitmann:

Wow. This went faster than the first one.

Interview 3: May 17, 2018

03-00:00:16

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History series. This is our third session. It's May 17, 2018. We are here in the Berkeley Hills. Last we left off, your company was about to enter Germany. I was wondering if we could pick up from there. I imagine that's a huge barrier, psychologically, for the Germans.

03-00:00:55

Leitmann:

I think that's what accounts, at least in part, for the fierce resistance they put up, knowing that they were losing the war, but trying to not lose it in such a way that it was really a defeat. They knew that things were over. It was always the hope, by those who hoped to win, that there would be some secret weapon, which was a big thing Hitler counted on. In that respect, I think it's maybe okay to intersperse that, because it came up in some of the interrogations we did after the war that, from a point of view of physics, there's no question about it that they knew the physics involved. But the interesting thing was that Hitler forbade his physicists to work on this, because it was "Jewish science," with Einstein. We lucked out. It's just amazing. So he put everything into these "weapons of retaliation," they called them, the V-1s and the V-2. That really was never a major factor in the war, because that one thousand pounds of explosives, particularly in the V-2, which is a much more fearsome weapon—if it had a payload, but it never had a payload. Actually, the V-1s, which were pulse jets, were really much worse, on the Londoners in particular. I remember when we were stationed for the few weeks in Great Britain, before going to France, we used to get, of course, passes on the weekends, and I spent a weekend in London and went to the Royal Haymarket Theater. I don't remember what the play was, but it was interrupted by a V-1, because you could always hear that they were about to hit you when the *put, put, put, put* stopped, because it was a pulse jet, so it would *put, put, put, put*, then it stopped, and you knew it was going to hit pretty close. This one landed just maybe two, three hundred feet in front of the theater. Again, it didn't have a huge payload, but being such a small weapon by comparison to the V-2, it could be produced by the hundreds. It was really, from a morale point of view, a much worse weapon than the V-2, which failed so many times.

03-00:03:40

Leitmann:

One of the first post-war books in basic mechanics was written by two Dutch professors, [J.M.J.] Kooy and [J.W.H.] Uytendbogaert [sometimes spelled Uytendbogaart]. I still remember the book [*Ballistics of the Future*, 1946]. I have it downstairs somewhere. They also had a nice section on applications, and a whole appendix, in fact, on the development of the V-2, with photographs, everything. At the beginning, when they were testing it, they were firing most of them from the Netherlands. I don't know how many crews they took out, because they had the bunker with the controls two, three

hundred feet from the launching pad, and those things used to stand up there and go up and fall over with a full load, and just took everybody out.

03-00:04:46

Burnett: It was fortunate for the Allies that the anti-Semitism of Adolf Hitler—

03-00:04:51

Leitmann: Well, that was one part. The other part—that came out of our interrogation, that other part came from somebody else's—is that, after the war, when we had Operation Paperclip to pick up German scientists and engineers, and we were always putting them in one room, in the hotel usually, or a castle, and have the adjoining rooms, we could listen in on them. In August of '45, when the two Hiroshima and Nagasaki bombs dropped, from their conversations you could gather they thought that we had built a huge reactor, and their amazement was the size of plane that would have taken to carry that. The whole idea of the implosion and all that was completely absent, because they didn't involve engineers. It was the engineers who thought of the way of doing that. So we lucked out in many ways. And the Germans lucked out that they lost the war when they did, because, since we had two bombs, we might well have dropped one on Germany and one on Japan. All these coincidences or quirks in history are really amazing, because the alternative would have been so different, catastrophic, or other ways. So it's interesting. Anyway, go ahead. You maybe have a question.

03-00:06:36

Burnett: I guess one question that comes, because it is certainly true that in the absence of a super weapon, the Allied bombing sorties relied on incendiary bombs and blockbuster—

03-00:06:54

Leitmann: Yeah, the British and we undertook this jointly.

03-00:06:57

Burnett: The various kinds of and the advancements in high-explosive devices that took out a good chunk of cities and towns in Germany. There was certainly some damage being incurred in Germany at the time, and they did not escape unscathed.

03-00:07:17

Leitmann: No. If you look at Berlin after the war, of course.

03-00:07:20

Burnett: It's astonishing, isn't it?

03-00:07:22

Leitmann: Again, as an aside—I was going to mention it later, but I might forget if I don't think of it now—in Würzburg—and we'll talk about that episode later—but I remember that's where I witnessed, not the raids themselves, but the operation. I could look up, over a two or three-hour period, and see these huge bombers of ours fly towards Berlin, and they were still flying towards Berlin

when the first waves were coming back. That's how many. There were two thousand planes at a time sometimes. And yet, given the payloads, which were horrific, of course, were so piddly—compared to a nuclear weapon.

03-00:08:15

Burnett:

Right, but it adds up when you have two thousand sorties.

03-00:08:18

Leitmann:

Oh, yes. To see these huge planes fly in formations of two hundred planes in one direction, while the ones from before were coming back. That was a one-and-a-half, two-hour flight.

03-00:08:32

Burnett:

Just an airborne train.

03-00:08:34

Leitmann:

Yeah, train. Again, as an aside, towards the end of the war, we instituted a policy of warning the citizens of towns to be eliminated of that fact. That in the foreseeable future, we urge you to leave your town, because you'll be flattened. By that time, of course, the Germans knew what these air raids did. It had an effect, usually, on the poor people. In Würzburg, for example—a town maybe the size of Berkeley, 120,000, 130,000 people—the rich people lived on the west side of the Main River, and they will come up in my stories later, and about 80,000 to 90,000 of the population stayed, and that part of the city was flattened. The only thing that stood up was a church tower somewhere, actually higher than your arm. The concomitant of that, which is, I guess, never mentioned, but sort of came to me, was the absolutely unique smell of these towns when you came in, because there were thousands of bodies under that rubble. Maybe one didn't know exactly what it was, but it had a very unique aroma.

03-00:10:00

Burnett:

Of death.

03-00:10:00

Leitmann:

Part of it was the burning stuff. Of course, that had its own smell. Those are sort of the things one doesn't necessarily think about in terms of war, because it isn't the actual fighting. It's sort of the aftermath of combat, which lasts a long time.

03-00:10:27

Burnett:

It's part of a strategy of total war, right? The idea goes back to the American Civil War. If you take out the capacity of the enemy to fight, not just the military, but the train stations and the civilian population is exposed.

03-00:10:43

Leitmann:

Sure. Of course, it was to the nth degree in the Second World War, and then, of course, in Vietnam, I guess, even worse in a sense.

03-00:10:53

Burnett:

Yeah. Four times the number of bombs were dropped on North Vietnam than in all theaters during World War II.

03-00:11:01

Leitmann:

That's horrendous to think about. And people not prepared, really, for that. Anyway, those are the things. We were sitting around, I think in Sarreguemines—Germans called it Saargemünd—I don't know how long, but it was at least a week, maybe two. Just doesn't stick in my mind that closely. Then the decision was made to actually break through the West wall, called the Siegfried Line. There was a Siegfried Line in the First World War already. It must have been early March, towards mid-March, I guess. See how close that was to the end of the war, when you think about—two months.

03-00:11:49

Burnett:

It's astonishing.

03-00:11:50

Leitmann:

Two months. Still how many people had to bite the dust at that time. I mentioned this before. I was fortunate to be in the reconnaissance unit. I guess it was called—I printed that sheet out—operations and something.

03-00:12:25

Burnett:

Oh. Plans and Operations.

03-00:12:27

Leitmann:

I belonged to the Plans and Operations section of my battalion. My picture is at the very bottom there.

03-0:12:35

Burnett:

Can I ask you what this is? It looks like a scrapbook.

03-00:12:39

Leitmann:

Yeah. One of the people, actually right at the end of the war, put out a book, not for publication, but just for the family.

03-00:12:51

Burnett:

I don't know if I can focus on that. [zooming camera]

03-00:12:54

Leitmann:

I think his name was Hoppe, the other one, is still around. His father was in that unit, and he and some other guy got together and collected material, and about 1946 or so, they put out this little booklet for the veterans of that unit. There's one of these for everything. Water supply, this, that. This was Plans and Operations. I don't know how I got into operations. Yeah, well, operations. Plans, I was certainly not involved in the plans. I guess gathering information belongs to that.

03-00:13:40

Burnett:

Sure. How far out in front is reconnaissance when you talk about—

03-00:13:50

Leitmann:

I think that depends entirely on the situation. In some cases, it may be miles, and in other cases, it may be a few hundred feet. The concomitant of that was, A, I didn't do any work. I didn't carry panels around, building bridges. Many of these people lost their fingers in that operation, because they had to put the pins in. These things are flexible. These Bailey bridges are still used, by the way. Bailey is the guy who invented them. In Hawaii, you will find many Bailey bridges on the highways, where the engineers, after the war, just used those to build the local bridges. They're sort of panels, maybe thirty, forty feet long, and about maybe four, five feet high. There were bolts put through at the end. So they're all flexible, which took some of the stress off. They could be built as singles, doubles, triples, and single, double, and triple high. For railroads, we built one in Würzburg that was a double triple, or a triple double or something like that, and got into the newspapers even, I noticed, at the time. In my book, you'll see some pictures of it.

03-00:15:17

Burnett:

Was that a transformative technology from the point of view of rapid advancement of mechanized forces?

03-00:15:28

Leitmann:

I think in terms of mass use, yes, but the bridge was invented, I think, before the war. I don't think it was available in the First World War, of course. So it was transformative, certainly, in terms of the rapidity with which it could be put up, with sufficient loss of fingers. It's not funny. I think our first casualty after the bridge that we built in Sarreguemines was a member of one of these, and there was an attack. Usually the Germans tried to, of course, fire during building on those things, either with artillery or close by, just ordinary weapons.

03-00:16:14

Burnett:

Slow them down.

03-00:16:15

Leitmann:

Yeah, because that was always a transformative thing, to be able to get heavy weapons across. They, with some notable exceptions, such as Remagen, really did blow up most of the bridges. There, they missed. The officers who missed all got executed.

03-00:16:38

Burnett:

Can I ask you, how fast could you build a bridge across a medium-sized river?

03-00:16:43

Leitmann:

Probably in a day. If there wasn't any resistance. You had to go and hide, but if it's unopposed—yeah. It's very fast, because these things slip together. It depends on what the terrain is on both sides. You might have had to build some structure to support the ends of the bridge. That might take much longer. Usually, the sequence of crossing a river was first by assault boats, which were operated by the engineers, and then building pontoon bridges, at least for

infantry to walk across. Again, built by engineers. Then the bridges obviously came last, because they take much more equipment. Trucks would have to come along and bring the equipment. That presupposes that the roads were good enough. It really depends on the amount of damage that was encountered during the preparation. Building the bridge is probably the quickest, the fastest, operation. Again, if it got to be a double triple or something, well, that would take much longer, of course.

03-00:18:03

Burnett:

Did you feel, looking back, that you learned some things that you applied later in life from those experiences?

03-00:18:15

Leitmann:

Certainly not from an engineering point of view that I can think of. They are, from a technical point of view, very simple. The basic idea to build them in panels, that maybe six or eight persons could carry with sticks across. Once they lined up the holes, just to put pins through, about this size, that was certainly something which gives you the—and there are contests, even at the universities now, to build a model of a bridge, a small bridge, as fast as possible. That certainly is in that history. But that's been around, really, quite a long time. We're talking about maybe eighty or ninety years.

03-00:19:15

Burnett:

Maybe there's a more elemental thing that you learn, is something about stress, and alternating with boredom, perhaps.

03-00:19:25

Leitmann:

You could see that from our schedule, boredom or discomfort. For example, that winter was terrible. Sitting in the truck, where you couldn't move around. I heard, maybe, myself, two people crying from the pain.

03-00:19:46

Burnett:

Oh my God.

03-00:19:48

Leitmann:

I'm sitting here now, I know I had frostbite. I know it now.

03-00:19:52

Burnett:

Can you feel it now?

03-00:19:52

Leitmann:

I can feel it now, because my dorsal arteries were clogged up and they never came back, of course. I'm a veteran with a very small pension, [Laughter] good for lunch. We moved on, and I'm not really sure, without looking actually at the plan, what our first stops were. Certainly the first major stop was on the Rhine, in the vicinity of Worms. I think it was north, actually, at Worms, between Worms and Mannheim, in that area. There was another engineer battalion—it was the 280th and the 290th—who were very heavily involved in establishing the bridgehead. I think we really came on, maybe, the next day or the next hours, to start using the fact that the other side was not

safe, but at least there was a way to stand up and hope nobody's going to shoot you. But at least it wasn't direct combat. That was, I think, our first major stop, as I remember it.

We got to the other side, and again, the exact time of day and all that very hazy in my mind, but it must have been late afternoon that we got across. I think by that time, we were able to already take trucks across. We must have been, because we stopped maybe a couple of hours after the bridgehead to pull in for the night. We pulled into a little clearing in the woods there. On the way there, there were still lots and lots of Germans, dead, lying around. They were beginning to bloat up. It's a sad commentary on inhumanity that the GIs were taking potshots at those bloated-up bodies, just to hear them pop. It makes people not very nice. We pulled in there, and by that time, in fact, since I was using mostly a Jeep with a driver for these missions, I had picked up one of these—what do they call them? Gurneys, I guess.

03-00:22:25

Burnett:

Yeah, a cot.

03-00:22:27

Leitmann:

Yeah. So I slept on that. In the Jeep, I could stow that. So I put that thing down, and I had a sleeping bag that I could pull over my head, and woke up—never even moved during the night, as far as I was concerned, I was so pooped. It seemed dark, and I couldn't figure out why it was so dark. I could see light coming in from the side. When I looked up, I was lying under a truck. A truck had pulled up right over me and not hit me. I've been trying to list all the many times, or few times, that I just barely made it. It's pure—what do you call it? Luck or whatever. If I were religious, I'd say it's a guardian angel. I think I recounted this before.

03-00:23:22

Burnett:

The answer is you're here because you survived.

03-00:23:27

Leitmann:

Yeah. No, of course.

03-00:23:28

Burnett:

In the end, a Darwinian sense.

03-00:23:30

Leitmann:

The product of that, and how to get to surviving, is very haphazard. It happened a few more times after that. I think first time really in Colmar, when we used one of the public buildings that the Germans had zeroed in. The roof was missing when I woke up in the morning. I didn't hear a thing. Here, again, it's really fortuitous a truck isn't that wide, that he missed me. From there, again, it was a number of days, with lots of stops in between. By that time, we usually tried not to sleep in tents, but to find—in these small villages, most of them were not really badly destroyed. There was no resistance there, so there were always peasants' houses or something that we took over. I think

the only thing that I stole there was a bunch of stamps. They were lying around. Somebody's stamp collection. I didn't take the stamp collection. I took a couple of sheets, just as a souvenir, just like those woodcuts.

03-00:24:50

Burnett: Right, you mentioned the woodcuts earlier.

03-00:24:51

Leitmann: That was in Colmar.

03-00:24:54

Burnett: I imagine, when you mention these empty towns, you're also talking about massive numbers of refugees. So the displaced, the soldiers are running, but the people are running.

03-00:25:06

Leitmann: Yeah, going in the other direction. In some of the very small villages, there were essentially no men left, and it led to a lot of quick love affairs. It's really interesting. People say, "I'm going to die."

03-00:25:24

Burnett: It does something to you.

03-00:25:26

Leitmann: You don't think about morality. It's a completely different view of the world. It occurred to me at the same time, because I mentioned it to somebody yesterday, the essentially—not hatred—disdain combat troops have for the troops in the rear and for civilians in general, because they always found out that the civilians [on the home front] were complaining they didn't have enough meat, or they had to eat chicken, or stuff like that. Suddenly, you say, what the hell is the matter with these people? I wish some of them were here. It's an unthinking thing, because it meant your own family, obviously, back home, but nonetheless, there was sort of an enmity built up. It's a separate society. That's really what it is. The others have it so good, and what are you doing here kind of thing. There was less of that, I think, by the time I got into the war, when it was pretty clear we were winning the war, that it may take a lot of time. I mentioned that I think the first night on the beach there, when we slept in little two-person pup tents, and then woke up in the morning in a field of unexploded ammunitions, suddenly we thought, we're not going to make it. But that changed after a while. Then it became primarily something one gets used to. It sort of becomes daily life, with just sporadic things that were dangerous. The German Air Force no longer existed, for all practical purposes. It could have been much worse.

Again, as an aside, right after the war, we were driving along the autobahn, and the autobahn was essentially built as an airfield for the Germany Army. That was clear from the beginning that those were the runways. Parked, for stretches, on both sides of the autobahn, were jet airplanes. Not the full airplane, but clearly what—so the Germans were that far ahead. The

Messerschmidts, they had jet planes by then. There's a picture of me somewhere in there—

03-00:28:01

Burnett: There is. I've seen it.

03-00:28:02

Leitmann: —where I stand on top of a wreck of one of them. It's amazing how iffy the whole operation was.

03-00:28:10

Burnett: A sense of contingency. We look back and think, oh, it was inevitable, once the United States got into the war.

03-00:28:15

Leitmann: No, it was very iffy. Very iffy, always.

03-00:28:18

Burnett: Even at that late stage, did you feel it?

03-00:28:21

Leitmann: It wasn't clear that the war was won, certainly to us. We knew we would win it eventually by that time.

03-00:28:30

Burnett: But at what cost? That was the debate about dropping the bomb on Hiroshima and Nagasaki, right?

03-00:28:35

Leitmann: Yeah, particularly where there are landings involved and that kind of thing. Again, you have to put yourself into the shoes of the decision-makers, who are faced with these alternatives. It's easy to be super moral. The whole operation is immoral. That's the thing. The only question is who is less immoral than the other side. Atrocities occur in every war, of course, but this one was no—in a way, because of the swiftness of D-Day to the end of the war, which was just a little over a year, much of that could be avoided. There were executions of American prisoners during the Battle of the Bulge, but I think it was a rarity.

Anyway, I'm not sure. The next stop was probably on the Main River. Major stop, where we stopped for a number of days. The other stops were only like a day, and then go right on the next morning. We came from the west towards the Main River. I guess we didn't quite expect what we would find. It turned out the east part of the river was really not fully secured. We lucked out, because the rich people in Würzburg, first of all, heeded the warning to get out, because they had the means and the money and everything, and they lived in very nice villas. So that's the first thing we did. That's where we bivouacked, in the villas. I think we were in a three or four-story building. Again, when you say a trivial kind of immorality—we always looked for the best china to eat on, and then throw it out the window instead of washing it, just for the hell of it.

03-00:31:06

Burnett:

I guess you're in a surreal situation, because a few weeks earlier, you were in a cattle car, unable to go to the bathroom, and freezing.

03-00:31:14

Leitmann:

Right. By that time, of course, it was springtime, so the weather was not quite as bad. Then we found out, I think the next day already, from some of the troops who had arrived before us, that there was a barge moored in the Main River, full of champagne. The Germans had brought up as much champagne as possible from their conquest in France. That, and other things, they transported with them. I think we, first of all, drank a lot of it, and having drunk a lot of it, we also took a bath in it, which is not a nice thing to do, because we had to take a shower for a long time. That I remember very clearly. Then it must have been maybe the second or third day, the munitions dump right across the river from us blew up. There were still snipers around. Whether they fired on it or whatever, I don't know, but it really made a big bang. Again, it didn't particularly wake me up. Very much like Colmar, I had been sleeping in my sleeping bag. When I pulled the head cover back, I was out in the open again. The roof had blown off. I guess it just blows out.

03-00:32:40

Burnett:

You must have been so exhausted to be able to sleep through that.

03-00:32:44

Leitmann:

I think that that's the secret. The people downstairs didn't do so well. I remember that we had very—nobody was killed, but badly wounded people from flying glass. The communications people had set up down on the ground floor, and the guy who was with the operations equipment sat near a window. So he very nearly died because damage glass can do is really terrible. But up on the roof, it was just the roof that went, but it flew out. So another little guardian angel thing, I guess.

03-00:33:33

Burnett:

You mentioned in another interview that not everyone could take the stress. Especially since the Gulf War, we talk a lot now about post-traumatic stress disorder. Were there signs of combat stress in your company? Did you experience it yourself?

03-00:33:54

Leitmann:

There was a young lieutenant—I probably didn't mention him. I don't remember his name now. Moving ahead, he saw a map case. Germans carried their maps in leather cases over their shoulder. He figured it would be interesting to find out, and he pulled on it, and the body fell on him, and the body was not in good shape. He had to be sent home. He went crazy.

03-00:34:23

Burnett:

So it was a cadaver that was up in the—

03-00:34:24

Leitmann:

The cadaver fell on him. The cadaver had been there probably a number of days. He was pulling on that map case that the guy had around him. He couldn't take that. And I can understand that. You get that stress partly from being in danger yourself, but partly from incidents like that, which are just hard to take.

03-00:34:55

Burnett:

There also wasn't necessarily—

03-00:34:58

Leitmann:

A buddy. This wasn't a buddy. No, I think it was just the shock.

03-00:35:06

Burnett:

Was there support for people who were going through that, or was there less of a sense—

03-00:35:11

Leitmann:

No, we didn't have any psychologists or psychiatrists. Medical officers were usually butchers. I'll come to one of those later, Butcher Adolina. I'll tell you who took the place, were the chaplains. There weren't a lot of chaplains around, but the chaplains usually took that as part of their assignment, to take care of people who were over-stressed until they could get them out, if they were really that badly damaged. Yeah, I think that was really the only support, that I remember, that could be found for people who were either slowly or quickly going nuts.

03-00:36:06

Burnett:

You're moving in fits and starts, but pretty quickly, up northeast towards an area south of Frankfurt, right? To help us orient ourselves.

03-00:36:17

Leitmann:

Right. Aschaffenburg, I think, was as close as we got to Frankfurt. It was a suburb.

03-00:36:25

Burnett:

You've showed me this fascinating Google Maps indicator of where the 286th—somebody went through and positioned the 286th from the time they arrive on the continent—

03-00:36:37

Leitmann:

I think there are written records of this. The officers, at least the executive officer, had to really keep track of where we were, when, and kind of write a diary of the movements and the operations. I think that's probably where that came from. Of course, he has a staff, so there are a couple of sergeants who might have figured that would be a good thing to do. There's the Google Map. There's the other map from—in fact, the same little book that these people are working on has a map, too. I haven't printed it out, but I can do that. It's similar to that Google Map.

03-00:37:30

Burnett:

You're obviously chasing the German Army back. Are you encountering people who are surrendering? Are there mass surrenders? Is it kind of piece by piece? How does it work?

03-00:37:45

Leitmann:

I think it's mostly piece by piece. There were, of course, sometimes whole units that surrendered. It was not appreciated by the German government, and many of these units that surrendered *en masse*, their officers, generals, were condemned to death in absentia by Hitler. For example, the people who surrendered at Stalingrad, they were all condemned to death. By that time, they were prisoners of war, so they were protected from that end, which is sort of a funny way of looking at it. There is an incident which, in fact, fits into the Würzburg scenario. I think it was the third or fourth day, we had sent out a couple of guys to see what was going on in Würzburg, and then beyond that. It was completely flat, as I said. There was certainly no mass resistance left in there, but there were still German troops there. The next day—I think it was after the explosion—we were trying to find out just how much more of that stuff is hanging around. Two guys got sent out, and they didn't come back. So then, about a day or two later, my driver and I were sent out to see if we could locate them. So we went across, and we were only fired at, I think, once. I think there must have been a sniper in one of those church towers that stood standing once in a while. We got away very quickly, because our whole mission was not to engage in a firefight, but to get information, which was again something which made my job, in a way, safer—you want to call it that.

03-00:39:50

Burnett:

Yeah, you said you could maneuver, you could leave an area.

03-00:39:53

Leitmann:

I could maneuver, and in fact, their mission was not to get engaged in combat. What are two guys going to do in combat but get information and get it back? For example, we didn't have any shortwave radios. They may have existed, but we didn't.

03-00:40:12

Burnett:

It's old-fashioned recon. You are going out and getting it directly, and going back physically.

03-00:40:17

Leitmann:

Absolutely. Human intelligence. I don't know how far out it was. We'd been driving for the better part of the day and didn't encounter anything. We got into the suburb, and I think it was Aschaffenburg, which is a fairly large suburb or town just outside of Frankfurt. There was an alley with trees and lamp posts, and hanging from—I don't know how many, because—what I perceived to be mostly kids. I mean, hanged there. As soon as we got to houses, I knocked on a door. They knew the Americans were coming. In fact, American tanks had already gone through that part of the place. That's exactly what happened, is that these kids—ten, twelve years old—had been given

bazookas. Each a bazooka. It's called a Panzerfaust. And said, "As soon as you see an American tank, shoot at him." Well, they ran away as soon as they heard the tanks coming. They didn't wait for American tanks. Then, to their great sorrow, American tanks came through. Two hours later, an SS company came through. This intermingling of the front line was not unusual, because things moved so fast. They asked, "What happened?" The people said, "Well, these kids ran away." So they hanged them. And they hanged other people who surrendered. I found out later on that was not unusual, either executing or hanging. That was not a singular event. It's something that I still wake up to, because it was so crass.

03-00:42:34

Burnett:

It was very shocking when I was a kid, and I think a lot of young kids were very fascinated by World War II, of my generation and earlier generations, too. There's a childlike, a boy-like, fascination with it. I remember seeing a photograph of—it's the tail end of the war, and it's a German soldier, and he couldn't have been more than fourteen.

03-00:43:01

Leitmann:

Volkssturm, it was called. "People's storm."

03-00:43:05

Burnett:

He had a Panzerfaust, which is like a rocket-propelled grenade.

03-00:43:09

Leitmann:

Yeah, right, exactly. This was normal procedure, of course. It's a little bit like, in the Middle East, or in Africa, these armies of kids. Eight, nine years old. In Asia. Well, look at the Children's Crusades, those two crusades. Those kids were of that age. It's nothing new in humankind, and it shows that things don't change, certainly not rapidly enough to suit me.

03-00:43:56

Burnett:

There's a larger context of desperation in the enemy, that they're running out of resources, they're running out of food, and strategy is out the window, and they're on the run, basically, at a full—

03-00:44:11

Leitmann:

First of all, there are not too many men left of military age, so clearly the next generation are the ones who get—and there's also maybe the subliminal hope that maybe the enemy will not really fight those kids, which I think is a vain hope in most cases. That sort of fits what drives you crazy. By that time, I expected almost anything. I was nineteen.

03-00:44:48

Burnett:

You're a teenager yourself at this time.

03-00:44:51

Leitmann:

I didn't consider myself one.

03-00:44:53

Burnett: I'm sure.

03-00:44:56

Leitmann: I was probably one of the younger people. In a sense, it was a grand adventure. Thinking back on it, I liked army food. Even K-rations.

03-00:45:13

Burnett: Are you the first person to say that with a straight face? [Laughs]

03-00:45:18

Leitmann: I'm telling you. I was brought up with pretty damn good food, but it was different. Maybe that's what made me a vegetarian, is I like all these simple things. I think the thing that, in a sense, was most—not disturbing, but most painful—was real pain, like the frostbite. I sit here and I know exactly what's going on now. It's a lasting effect. Of course, that happened to most of us at the time. It wasn't my singular experience. But I think it was a different experience from what I think almost the remainder of certainly my buddies—they didn't speak the language. So I could really find out much more quickly what people really thought, sometimes by listening only, not letting them know that I spoke—

03-00:46:31

Burnett: You did tell us the story of the barber situation.

03-00:46:34

Leitmann: Oh yeah, for example. That's more humor-of-the-gallows kind of humor. Sure. I think it gave me a very different—that and the experience of my own family. Got to remember that between that period and the time I left was only four years. From the time I came to the States and went back was four years.

03-00:47:03

Burnett: It's difficult to comprehend. So much of this is difficult to comprehend, and I guess one of the things I wonder is, what does that level of experience—how does that change one? Feel free to speak for a generation at this point, because there are so many people. There were, depending on how you count them, between ten and thirteen million men at arms in the United States alone. The losses, twenty million dead in Russia, seven million in Germany, four hundred thousand US dead. Tremendous losses, but tremendous mobilization with people having a kind of common experience. That's often called the Greatest Generation, for—

03-00:47:55

Leitmann: Whatever that means.

03-00:47:56

Burnett: Whatever that means, exactly. But it's a common experience. I won't ask you to answer that now, but perhaps keep that in mind when we're thinking about your further growth and development after the war as a student, and then in your professional life. How this affected you, and if you can comment—

03-00:48:21

Leitmann:

I'm not sure that I have an answer. I haven't given a lot of thought to it as self-examination. It's certainly true that motivations, the way, perhaps, I look at the situation in the world, maybe that's influenced by it. In a way, it made me perhaps more tolerant. For example, I never lost my temper in later encounters with true war criminals. People ask, "why didn't you go ahead and hit him?" I was never even moved to do that.

03-00:49:14

Burnett:

Was there a sense of—I don't know, I don't want to ask a leading question—but almost a sense of a common understanding about things? Or did you feel it was not worth the investment of emotion? Was it that conscious?

03-00:49:31

Leitmann:

I don't think it was that conscious. I never analyzed myself, saying, why don't I lose my temper? I don't remember ever doing that.

03-00:49:39

Burnett:

It probably kept you alive.

03-00:49:41

Leitmann:

It seemed to me an obvious thing, particularly where I had a job that would have been impossible. And I certainly didn't want to be like them. Now, that's certainly a motivation that I thought about. But that's sort of obvious, because if you're like them, you should have joined them. If they let you. [Laughter]

03-00:50:06

Burnett:

There's obviously the advances in interrogation techniques that come later in police interrogations. The state of the art now is to gain the trust of the adversary or the suspect.

03-00:50:18

Leitmann:

Either that or waterboard them. Two ways of doing it.

03-00:50:21

Burnett:

That is, in fact, a regression—

03-00:50:23

Leitmann:

That's a regression. Yes, of course.

03-00:50:25

Burnett:

—to earlier techniques.

03-00:50:26

Leitmann:

To the Middle Ages.

03-00:50:29

Burnett:

But best practice in interrogations now is to win the trust.

03-00:50:32

Leitmann:

It seems almost obvious. There is a prime example. There's this book that I mentioned to you by [Richard] Sonnenfeldt, called *Witness to Nuremberg* or something like that. He was a man who was maybe four or five years older than I am, and was moved to write things down for his kids and grandchildren, and that eventually got published. He was also an engineer, or became an engineer, and in fact he became a vice president of General Electric. When they were preparing for the Nuremberg Trials, they were looking for people who could speak both languages, and he happened to be one of those. He was working in a motor pool as a sergeant or something, and they pulled him out. He was a translator for General Telford Taylor, who was the US prosecutor. I'm getting ahead of myself, but you asked me that particular question. He had a terrible time with [Hermann] Göring. Telford Taylor wanted to particularly get to Göring. They let him keep his uniform, but of course without the epaulettes and stuff, and they refused to call him "Herr Reichsmarschall." But Sonnenfeldt recognized immediately that if he could become a buddy of Göring's, he would be much more useful, and that's what he did. He called him by his rank title, that kind of thing, and let him know that he recognized how important he was. Like so many people we know even today, he was very concerned with being admired and respected and given credit for, and that kind of thing. And he was very successful.

03-00:52:43

Burnett:

It was a bit of a master stroke. The journalists covering the trial, they could see what a showboat Göring was. It was pretty clear.

03-00:52:54

Leitmann:

That was a very clever move. When you read the book, you realize that he recognized that fairly early. He was maybe twenty-five at the time, so maybe five years older than I am.

03-00:53:08

Burnett:

Let's turn back. You're still in the 286th, attached to another division. You had gone northeast, up to the area kind of below Frankfurt, and then there's a turn. You're basically chasing the retreating Germans.

03-00:53:27

Leitmann:

You've got to recognize that the individual GI has no idea what's going on. I didn't know I was turning left or right. All I knew is we were moving from one place to another. The complete lack—it's probably the only way to go—of information about what the hell is going on, except at the fairly high level. I'm not even sure that our colonel knew exactly what was going on. It had to be even higher than that. I certainly don't recall ever being aware of any kind of strategy or any particular way we were going. I'm not even sure I knew what direction I was going into. I knew I was moving every day or every other day, but that was strictly local information.

03-00:54:18

Burnett: This is into April now, and you're moving southeast. According to this map, you end up basically right at the Austrian border, right at that—

03-00:54:30

Leitmann: Eventually, at the end, but there were stops in between. For example, the surrender of Bad Tölz, where I was without even, essentially, a uniform. I was essentially in just pants and a shirt when this officer—and he wasn't a high-ranking officer. He was just part of the surrender party that surrendered to me, and I was just reading him the instructions in German. That seemed very reasonable to me.

03-00:55:06

Burnett: What did, his surrender?

03-00:55:09

Leitmann: Well, the whole point that I was involved in it; I was the German speaker. That's one of the reasons they put me in the reconnaissance unit. So that was my job.

03-00:55:17

Burnett: You're the interface.

03-00:55:19

Leitmann: I didn't consider myself special. I didn't realize how lucky I was. At that time, I didn't analyze the situation, saying, you're luckier than 99 percent of this battalion, because you don't have to work like a dog. Particularly the notion that what I did in a sense was safer, that didn't occur to me until afterwards. I certainly didn't see it as an immediate plus. I found it an interesting assignment. "Interesting" has many definitions.

03-00:55:54

Burnett: Yes. It clearly had its challenges. I've talked to other veterans who had German, and they were picked right away for a very specific task because they could speak German. And you were [chosen], in a sense, in your division, but are you then plucked for this kind of role and positioned into other places? How does that work? How do you transition?

03-00:56:36

Leitmann: Well, it did, in terms of after the war. I tried, even before the end of the war, to let my officers know that I would like to transfer to what I thought was a unit that would make use of my talents. The colonel always said, "No, I need every man. Forget it." Colonel Johnson. So it wasn't until after the war, maybe July or so, that I was able to actually apply officially for a transfer to the CIC [Counterintelligence Corps].

03-00:57:17

Burnett: So you sought it out yourself.

03-00:57:18

Leitmann:

Yes. They didn't come and find me. Nobody let anybody know that there's this useful person around. But I knew that, particularly during the occupation, there must have been use for somebody who could speak the language, and presumably had some knowledge of the system. I certainly was never approached and said, "Would you like to transfer?"

03-00:57:49

Burnett:

Do you remember V-E Day? Do you remember where you were?

03-00:57:54

Leitmann:

Not specifically. I don't think there was this jubilation that I saw reported on by the newspapers and stuff like that. I don't think we even had a party or anything. Because it sort of happened in dribbles, little dribbles of danger disappearing, in a way. The only other experience along those lines was when we were already in Austria, near Innsbruck. We got as far as almost Innsbruck. Got into a little village, and suddenly were surrounded by German paratroopers, armed. They had these Schmeisser submachine [Maschinengewehr] machine guns. They had the short helmets, so you knew they were paratroopers. They weren't shooting at us. I identified myself. Of course, now they knew that I spoke German. I asked them who they were. They said, well, they're in the air force. They are Field Marshal Göring's personal—whatever they're called.

03-00:59:16

Burnett:

The guard.

03-00:59:16

Leitmann:

Guards, yeah. It must have been about forty or fifty of them. I said, "That's terrific. I'd be happy to take his surrender." They said, "Too late. He's having dinner with General [George] Patton tonight." At that level, they all get together. It's very interesting, those professional—I ran into this much later. Even at the height of the Cold War, our officers and theirs very often had parties. They're the ones who would talk to each other. They didn't talk to the civilians.

03-00:59:53

Burnett:

There is a kind of—

03-00:59:56

Leitmann:

It's a brotherhood of arms.

03-00:59:57

Burnett:

It is. It is. And it serves a kind of function. You can see that it's necessary to have communication between the enemy parties at a certain level, to be able to communicate.

03-01:00:09

Leitmann:

At the officers' club kind of thing.

03-01:00:13

Burnett:

To be able to discuss terms. I'm sure there's a whole highly evolved protocol around how one behaves. That's partly what's in play.

03-01:00:21

Leitmann:

Yeah, and I think that was sometimes followed, sometimes not. You see it more in movies, I think. I'm now thinking of—was it *Bridge on the River Kwai*? *Stalag 17*? I don't know. There was a movie where they were saluting each other, the Germans and the American officers. Sort of gentlemanly kind of interaction. I didn't see anything particularly. I was not an officer, so maybe that happened. I think by that time, there had been enough known about—not details of the terrible things that were happening, or had happened. So that sort of precluded too close [a fraternity]. But once you get into the general class—or even higher, a general, two or three stars—I think there was that. [Dwight] Eisenhower, I think, was an exception to that. He would not buddy-buddy with his counterparts. But Patton certainly would. Patton was very admired by the Germans. Their kind of general. That's a later incident, that reminds me of that.

03-01:01:44

Burnett:

We're talking about some of the behavior and some of the things you saw. Where does Landsberg fit into this?

03-01:01:52

Leitmann:

Landsberg, we got very close—in fact, we got into Munich. Of course, combat was over, but it must have been maybe two or three days after Munich actually fell that we were there. It may have been even after Munich, when we were moving on towards the Austrian border. There was a small town, almost a suburb of Munich, called Landsberg Kaufering.

03-01:02:31

Leitmann:

I think we got to the concentration camp, which was only a mile or so outside of the village, by pure chance, because I think the first units that you could call the liberators were the 101st Airborne Division. We certainly got there in time to see the smoldering bodies they were trying to burn and the skeletons. So it was certainly hours. That probably hit me more than it hit the rest of the guys, because here my father was still missing. I still had hopes to see him among the DPs. I was looking at people—

03-01:03:27

Burnett:

Displaced persons.

03-01:03:27

Leitmann:

—streaming back, which was, of course, stupid. Our colonel was so outraged with what we found that he decided to march every man, woman, and child through the camp. I figured I'd listen in on what they had to say. The gist of it was, "These Americans are, as we were told, barbarians. Look what they're making us look at." That was the attitude. They knew damn well there was something going on, because you could see the smoke from the crematory.

Word gets out. You can't hide that kind of thing. I don't know how surprised I was. But it certainly sticks in my mind.

03-01:04:29

Burnett:

What did you and others know about that kind of thing? Was that just being uncovered then? People knew that people were being disappeared and being transported. All of that was being reported.

03-01:04:41

Leitmann:

I think that it was certainly known. Certainly known by our government, for example. The individual GI? Not until he got into personal contact with it, or ran into somebody who was in the unit that went through a concentration camp. I don't think that that was particularly known.

03-01:05:09

Burnett:

Much has been written about those encounters. Unforgettable, of course. Is there anything that stays with you that you recall? I imagine it's just kind of unforgettable, the whole thing.

03-01:05:27

Leitmann:

It's something which is, in a sense, post-traumatic stress, of course. When I have a nightmare and I wake up, some of it certainly pertains to that. Just like those kids dangling from the lamp post. In a way, it didn't surprise me. I think that's really the interesting part. I don't think so. I don't know whether it had any effect on the troops in general, that they decided not to take any prisoners or something like that. I don't know personally of any such—I'm sure it happened, of course. The whole thing of the culpability of individuals you meet is almost impossible to know. If you're at all rational, if you ran into SS, then you could say, "okay." Maybe I didn't expect too much, because I'd mentioned it. Of all these people who were so-called friends in Vienna, nobody even surreptitiously would say to anyone of my family, "We're sorry we can't do anything. But we're very sorry." So maybe I just got inured to it.

03-01:07:19

Burnett:

Everyone is very familiar with the large camps, the Auschwitz—

03-01:07:22

Leitmann:

These were satellite camps. There were a lot of them.

03-01:07:26

Burnett:

Five thousand, apparently. Or more? Were you saying there were more than five thousand?

03-01:07:31

Leitmann:

In the report that I read some time ago, they counted the number of places where people were held, from five in the police station kind of thing, but up to the huge camps, eighty thousand. Eighty thousand places, separate places, for—

03-01:07:47

Burnett:

Detention.

03-01:07:51

Leitmann:

I forget now the guy that wrote the book, Goldberg or something like that. He counted from what he could put together, to run all these places, at least two or three million people were involved directly. That was sort of denied, and is still being denied, that, no, no, it took very few people to run those camps. And, on the other hand, they had lots of volunteers from the Ukraine to do it. The officers, of course, were Germans. Very often, camp guards were from the satellite countries, who volunteered for that. It was a good life. I know at Sobibor, for example, it was Romanians. I'm particularly interested in Sobibor, once I found out that a Leitmann was the number two in command of the breakout. If you read that account by the Russian lieutenant after the war, he said, "My number one friend and planner was Samuel"—called him Shlomo—"Leitmann." Who knows? It's not a very common name. But I had no inkling of that at the time, not the slightest.

03-01:09:27

Burnett:

It was so close to the end of the war, or right at the end of the war. It's this revelation of horror. This is one more additional—

03-01:09:36

Leitmann:

Even that took a while. I don't recall that they made much of it at the Nuremberg War Crimes Trials, per se. It was a clever way of running that whole operation. As I recall—of course, that's a posteriori knowledge—is that they took care of a number of problems, for whatever reason, once they set up the whole idea of war crimes trials. So that must have been maybe one or two years before the end of the war, they [the Nazis] started planning for this. To take care, first of all, of the excuse that the idea of concentration camps, for example, and atrocities, that always happened by the very people who are prosecuting us now. For example, they said in the Boer War there were concentration camps. That was an excuse. I guess it's been done before, because there is, in fact, a philosophy of law that says that prior information about your prosecutors is not admissible. It has nothing to do with anything you did. So that was immediately put into practice. As soon as, for example, one of the attorneys—they got very good attorneys at the time—would even raise that question, they were pointed immediately to—this has a Latin name. This was ruled out when the trials were set up.

The other one, which is maybe even more important from a practical point of view, is if, as suspected, millions of people are involved at some level, you can't have trials for millions of people. So what you have to do is you get the top people, and then you try the organizations. Which organizations are criminal organizations? Like the mafia. Well, the SS, the SA, the *Sicherheitsdienst*, all those. Then you say, well, you have to be above a certain rank before you can become personally culpable. So that was a very, very important idea, because they could have done nothing. What kind of trials can

you have for millions of people? That doesn't make sense. So that, I think, was a very important step in the whole legal process.

03-01:12:19

Burnett:

Was there also, kind of in the background, a sense that, when it came to settling the terms and determining the future of Germany, the question of justice was framed in terms of the previous post-war failure of World War I, that is, this retributive justice that led to resentment, that then produced another war? That's the narrative that won out in the end. I think, built into that, is a certain amount of looking the other way, because if you pursued culpability as you said, there are people responsible for the water supply of a major city. They were at a very high level. They might have known about these kinds of things. But in the end, the city needs clean water. There was a practical, pragmatic approach to rebuilding Germany that necessitated a certain amount of—

03-01:13:29

Leitmann:

Presumably, the use of people who were clearly guilty of bad things—just the police. You've got to have police. There weren't too many men around. You have to use people who are sort of policemen. That may have involved people in even the SS. That's practical. That's the operational level. At a high level, the Cold War had broken out already by the end of the war, and it began to bloom only a year or so later, but it was there. I think what saved the Germans and the other Axis powers was really that we had a new enemy. You want to make allies of those who might be useful. I think that was very, very important, and I could see that already, even in '45, when I was involved in this, we were already considering the Russians to be, if not enemies, certainly people that you had to watch out for and that were not really our friends kind of thing. But that was a return to pre-war days. It was "Uncle Joe" [Stalin] during the war, but then—

03-01:15:03

Burnett:

It was an alliance of convenience, for sure.

03-01:15:06

Leitmann:

On both parts. It was the same between Hitler and Stalin, obviously. That was maybe the crassest event that I can think of. If you look at what they said and believed about each other, hours before they signed things, it's incredible. It was strictly with a plan to make use of that for your own purposes, for the Russians to take over parts of Poland and whatever. Hitler just passed into the general plan, of course, of his world domination. Making friends and enemies is an interesting thing.

Just the other day—it's a complete departure—I saw a program called *Spying on the Royals* on channel nine. You have to give the BBC credit, what they bring out about the royal family. Apparently, when Edward was beginning to think of marrying Mrs. Simpson, his own father, George VI, established a spy operation against him, involving MI5, and eventually even the FBI. They

tapped the telephones. There are two professors in the history department of some university in Great Britain who made that their major topic of research for a long time, and they were finding documents, because more and more of that is being released. The most amazing document they found, after Edward resigned—and we know how buddy-buddy they were with the Nazis. One of the things that drove him the most was that his wife was outraged that she was made a duchess, but not a royal highness. Everybody else has the HRH, and specifically she was excluded from that. She would never be a member of the royal family, or have hopes of becoming queen. This was a major topic of discussion between them and the Nazis, and in particular even Hitler. They signed an agreement. Hitler signed an agreement, because he was trying to make Edward king to replace the actual king, and make use of the Fascist movement in Great Britain through an anti-government party, and then he would become the king. They would guarantee that she would be called HRH, and then become the queen. You say to yourself, no, this is a made-up story. First of all, he was never allowed back. Then the supreme insult, to make him the governor of the Bahamas. It was really interesting. I think individual motivations, whether they are grand plans you make for your life, or whatever, lead to very strange things.

03-01:18:44

Burnett:

There are all kinds of collaborations, from the very top to the very bottom, that people were sympathetic to the Nazi ideology, or they were pragmatically cooperating with them for their own purposes. In some circumstances, they were making choices between eating and—there's that level. Did you encounter that? In France, there were the *épurations*; there was this retribution after the war. Is there something similar that's happening in the places that you were at?

03-01:19:27

Leitmann:

There was something called the de-Nazification, which was presumably a way of, not retribution, but at least having a legal penalty for having been at a certain level in the system. In fact, one of the major functions later on—we're not at the CIC yet—was, in fact, de-Nazification, interrogation of people who were applying for jobs, people who wanted to become the police chief in this village, that kind of thing. The only thing is that I have never found, at that time—this isn't true now, and isn't true with the current generation—that there ever was a Nazi. "The people across the street—boy, they were terrible Nazis." It was that kind of thing. I ran into this all the time.

03-01:20:20

Burnett:

My father was stationed in Baden-Baden in Germany in the early 1960s, and they had a saying, with a very thick, almost Hollywood German accent: "There are no Nazis left in Germany. They all died in the war."

03-01:20:38

Leitmann:

In the war, yeah. "Obviously, nobody now, my buddies or anybody, could be." I understand, from just a practical point of view, if you can make use of

that or persuade people you weren't culpable of anything, or whatever you did you had to do, up to the point where people would say, "An order is an order." There's an old saying. "So it's not my fault; I was ordered to do it." And it's true, people were ordered, except they didn't resist. Even if you don't talk about resistance, there was a way of not cooperating, at a price, not being promoted or getting the job you want. But it usually didn't lead to really crass things, like being imprisoned or killed or anything like that. If there were actual resistance or plotting, that was another thing, but simply saying, "No, I don't want to be a sergeant, I'll just stay a corporal." That really wasn't a terrible thing, other than, of course, you didn't get promoted. Very few people could escape that. There were the exceptions. Very brave people. I'm always reminded of that Huguenot village in Southern France. Six hundred people in the village. None of them had gone to beyond second or third grade. They saved two thousand Jews. After the war, they were told, "Are you crazy? What are you thinking of?" They didn't understand the question. They said these were people who needed help.

03-01:22:27

Burnett: It was simple for them.

03-01:22:28

Leitmann: Yeah. No philosophy involved. Nothing.

03-01:22:31

Burnett: No justifications.

03-01:22:32

Leitmann: You get some, then, on both sides. On the other hand, "I'm not culpable; I'm brought up to obey orders." And you still get this. It's still a debate even now, military law, for a long time. What can you do if you get an illegal order from an officer? How many people would *actually* resist, even now? I think there would still be the brave people. As soon as it becomes dangerous to you or your family, everything else disappears, except for the exceptional people. One never knows whether one would be one of those exceptional people.

03-01:23:18

Burnett: Right, until you're tested.

03-01:23:22

Leitmann: I've thought about that. We can say, "No, no, never." I don't know about that. I don't know about that at all. It's not a trivial question, but I think it's one that has to be taken into account when you draw up laws, regulations, and so on. How much can you demand of an individual, a normal individual, in making rules? It's not a trivial question, because you can't ask too much of people. We have the hearings about the head of the CIA. You have the same thing.

03-01:24:11

Burnett:

I think that's one of the things that emerges in the postwar order, is that there is such a thing as what's called international justice, but it's really supra-national justice. Over and above, you're going to be held to account, and there is such a thing as a war crime. I think, for the longest time, there have been rules of engagement that go back, and those change over time.

03-01:24:36

Leitmann:

Yeah, but not in the sort of governmental level. That's a move in the right direction, except it's very hard to put into practice.

03-01:24:46

Burnett:

I think you've alluded to this before. Many people came out of that war with their own strategies for coping. Some, it became sort of black-and-white. That's an enemy, and that's how I deal with it. Your strategy was—I think you cultivated some empathy. You really looked at how morally ambiguous a number of situations were, and you cultivated a lot of understanding for other people. Is that a fair assessment?

03-01:25:24

Leitmann:

In the case of the women concentration camp guards I dealt with, there was no empathy at any level. I just said, first of all, I'm not going to be like them. Okay? And number two, it would make my job impossible. It would be counterproductive. So as long as I can keep my temper, I'm doing a good job. Because beating them up, what would that do? In fact, it would increase their resistance, and I wouldn't be able to do anything. Not that I was hopeful that they would ever admit anything. I'm generally—it bothers some people—too controlled. I'm self-controlled. It's just the way I am.

03-01:26:17

Burnett:

Let's talk about your shift to the next phase. The war has concluded. Tell me how you then end up with the Counterintelligence Corps. You applied. At what point are you brought in, and what is that like when you first encountered that?

03-01:26:42

Leitmann:

I think that I actually was transferred, physically, or legally. My unit was disbanded. I would have been assigned to another engineer unit. I should have been sent home, because legally, we were supposed to already be going home. I don't know what would have happened. Here, the occupation had started. I don't really fix in my mind the day-to-day operation. Orders came down, and I was transferred. I kept my rank. I was a corporal by that time. A lot of important people were corporals. Hitler. Wasn't Napoleon a corporal at one time? I was twenty by then. May '45. I just turned twenty. There was this whole idea, I'm going to be wearing an officer's uniform. Now, that seemed to me, at the time, very important. One of the first things I did is pretty funny. I found a tailor who could make me an Eisenhower jacket. That picture you have of me in an Eisenhower jacket—all that was done. I had to get the US's [insignia] and all that stuff. Seemed very important to me at the time, just sort

of as an operational thing. It's really funny. That seems to me, it was something that I really worked at.

03-01:28:33

Burnett:

You're still a corporal, but when you're transferred to the Counterintelligence Corps, something happens to rank in terms of the operations that you're doing. Can you talk a little bit about that?

03-01:28:46

Leitmann:

Within the Army records, you're still a corporal, but as far as the outside world is concerned, you're a special agent. You have credentials, you have a badge, and you have all the powers that are stated on a piece of paper, "...shall not be hindered in any way from anything." We could take on any rank. First of all, wear civilian clothes if we wanted to, or take on any rank, up to colonel, without special permission, if the job required it at a given time, as it did at one time, at least. That was very attractive. We were in teams, usually six to ten people assigned to a particular region. I just found out, fairly recently, that one of my close colleagues was Henry Kissinger. He was a special agent in the CIC. Did you know that?

03-01:29:51

Burnett:

Yes.

03-01:29:51

Leitmann:

In Hanover. He got into that through training right at the beginning. He was put into military intelligence, and then assigned to that particular branch of military intelligence. As far as I'm concerned, he never passed one of the things that was stressed to me, is that you mustn't have an accent. He hasn't lost it yet. [Laughs] So I don't know. He was always an operator, I think. That was clearly attractive, because we usually lived in very nice places. My unit was in Wiesbaden, the capital of the state of Hesse. We were bivouacked in the villa of a coal magnate. I think the team was maybe about eight or ten of us. We had a former German countess as the lady who took care of the household. She was the head of the household.

03-01:31:01

Burnett:

So she had staff to do—

03-01:31:02

Leitmann:

As staff. We had a kitchen and all that kind of thing. I became very friendly, and eventually got to be one of my best friends, a man about twelve to fifteen years older than I am, Tom Hermann, who was, almost from the beginning—well, strange—it will take two minutes. He was originally from Luxembourg, and very bright guy. He had a brother who lived in Switzerland. Another story. He was a perennial bachelor. Very funny guy. He was a guy I could talk culture about. He knew literature. He spoke eight languages. Eight. He didn't count the ones that he couldn't take shorthand in. He was actually, when I met him, already a lieutenant colonel, his regular rank, because he had done this for a long time. He fled from Luxembourg to France to Spain, and then got to

South America and he became the private secretary of Ambassador Biddle, who was then the American ambassador in Brazil. Then, very shortly after he got that job, he volunteered for the Belgian Army in exile. His boat that was going to take him back to Europe, because they were stationed at that time in Great Britain, it was torpedoed, and he landed in New Jersey, on the coast. He said, "Well, the best thing I can do is volunteer for the American Army," so he did. They made him some lowly thing. I don't know what, but some clerical job. It took them quite a while to realize that they had real treasure there: he knew Europe, traveled all over Europe, spoke all these languages. He got put, right away, into the CIC. When I met him, he and I got to be best friends.

It was the kind of unit where I think he and I were really the only fluent German speakers. Then we had a dog, a German Shepherd. His name was Wolfie. He was another member—he was a German member—of the contingent. And Tom—I bring this up with respect to the countess, who was handling the household—he ate starting with the dessert, and ended with soup or nuts or something like that. She arranged this for him—all that, that's perfectly fine. It was, again, a very different life rather suddenly, but suddenly I was—I don't know what you would call it, but a member of the master race, I guess. [Laughs] It was very interesting. For a twenty-year-old, it's like a movie script. We each had a Jeep and a private car. I had an Adler, a red Adler sports car. It was the last German car that was produced, still, in February of '45. It belonged to some Nazi bigwig. It had a siren in it. I used to speed on the autobahn with it, going a hundred miles an hour, and then the military police would stop me, and I flashed my badge. [Laughs] It was that kind of stupidity. It was surreal, in a way. But there were very interesting aspects to it.

I must have mentioned this before in other interviews. I was put in charge of a small prisoner-of-war camp for a while near Wiesbaden, maybe two hundred prisoners, something like that, a transit kind of place. One day, they brought in a Russian. That wasn't known to me at the time. I had to go and talk to each one of them. He didn't speak any German to speak of, just really fundamental German, so we could talk to each other in German. It was clear he had been a lieutenant in the Red Air Force, badly wounded. He was a little older than I was. He was about twenty-five or twenty-six, and became a drug addict, because his pain was so bad that they treated him with opiates. He became an addict. He was stationed in Berlin. Berlin was operated by the four powers jointly [United States, France, Great Britain, and the Soviet Union]. For example, the Jeeps: each had a member of each one of the Four Powers in it. They had a joint officers club. Then, what later became the CIA—I guess it was sort of a sub-branch of the OSS [Office of Strategic Services]—immediately latched onto him, and fed him opiates as long as he gave them information. Then, when they had dried him out, they didn't want to kill him themselves, but they thought the best thing is to stick him in a German prisoner-of-war camp and let everybody know he's a Russian. And that's what they were doing. The only thing I could do for him, I brought him cigarettes. That was at least something. To this day, I don't know what happened to him,

because I was only in charge for maybe a month or so. But I'm sure they killed him. That was one way to assassinate your enemies, have the enemy do it.

03-01:37:37

Burnett:

It's just bleak for him. He is someone who is wounded in battle, and as a result of his treatments, he's made into a vulnerable security liability.

03-01:37:49

Leitmann:

It's the horrific way things are operated, unfortunately, almost every day, everywhere. This is just a particular instance. We did the de-Nazification interviews. Right at the end of the war, there was a suspicion that the Nazis had started an underground called the Werewolves. We spent quite a bit of time on that for the first few months. I don't think we ever found any. We once raided an outfit that was presumably Werewolves. I don't remember what happened to them, except that we sort of used movie techniques to make the raid, coming through the skylights.

03-01:38:38

Burnett:

Were you participating in that, or in the planning?

03-01:38:40

Leitmann:

Yeah. Not the plan—well—

03-01:38:43

Burnett:

You didn't go in through the skylight yourself?

03-01:38:45

Leitmann:

I think I did. Yeah, I think I did. I was one of the younger people on that team. In fact, I was the youngest, because by law, I had to be twenty-four, and I was an exception. I got special permission. I still have some papers signed by command of General Eisenhower.

03-01:39:03

Burnett:

So yeah, you were physically one of the most capable. Did you get a crash course in rappelling techniques, or they just said—

03-01:39:12

Leitmann:

No, I saw it in the movies.

03-01:39:16

Burnett:

A rope is thrown down, and you go down?

03-01:39:18

Leitmann:

Yeah, that kind of thing. We saw a movie. I remember that definitely. We weren't trained for anything, as a matter of fact. Maybe they were in general, but that particular team never—I certainly didn't get any instruction. It was all practical. It was osmosis.

03-01:39:41

Burnett:

You improvised and you absorbed what was around you.

03-01:39:44

Leitmann:

There were more senior people who had been on the team for quite a while, so they told me what to do. You know, what do we do next? I certainly wasn't any kind of higher-up in the team. We were all special agents. I don't know, really, what the ranks of the other people were. That never came up.

03-01:40:04

Burnett:

If you got your techniques from films, then presumably the higher-ups were not advising you, because they presumably would have had knowledge of how to storm a place or raid a place. Or not, I don't know. But if you guys had to say, "what are we going to do?" "What do they do in the movies?" "They come in through the skylight." And you did it, and it was okay.

03-01:40:30

Leitmann:

Yeah. Later on, when I was a reserve officer, I did have a three-month course in military intelligence school. It was called Camp Holabird, in Baltimore. Camp Holabird. It was the army intelligence school. I think it still is. But that wasn't until the summer of '49.

03-01:40:59

Burnett:

Weren't there werewolves—there was a kind of guerrilla movement during the war. Was that supposed to be a revival of that?

03-01:41:09

Leitmann:

I don't recall any connection with that. You mean against the Nazis?

03-01:41:15

Burnett:

No. I think it might be mentioned in that book—we can verify it—that it was a kind of paramilitary—

03-01:41:32

Leitmann:

For after the war?

03-01:41:34

Burnett:

I think it was actually during the war. Anyway, I'll school up on that.

03-01:41:39

Leitmann:

It doesn't strike a bell with me, but it's quite possible. We can verify that.

03-01:41:45

Burnett:

So you did some—

03-01:41:49

Leitmann:

We even got commended for it. I have the piece of paper, the commendation, for successful demolishing the werewolves. It's in the thing that you have. [Operation Nursery]

03-01:42:01

Burnett:

So there was some kind of group that was nascent. It wasn't fully formed.

03-01:42:08

Leitmann:

I think it came out of the suspicion that in the redoubt up in the Alps, Berchtesgaden, that area, there's a group of Nazis hiding, trying to come back. I think that was a suspicion right from the beginning of the end of the war. There may have been a group. It's not surprising. In a sense, the neo-Nazis of today are really a continuation of that process. We were just speaking about Nuremberg. One of the people I interrogated was the widow and the teenage daughter of Heinrich Himmler, who had committed suicide when the British caught him. Again, I tried to figure out this very mysterious thing. What do these people really think they can get away with? I asked, "What did your husband—what was his job?" She said, "He was the police chief of Germany." Which is true. Then I said, "But you know, we know now that there were these concentration camps." Didn't have a clue. I said, "Did you know what he did?" "Oh, no, no. We never talked business. But he was a very bad man," she said. I said, "Why?" "Oh, he had many affairs." That brings me to today. That daughter, Gudrun, who was then maybe twelve or thirteen years old—exactly seven years difference. I was twenty and she was thirteen. That's right. She is the godmother of the neo-Nazi movement to this day. She founded an organization to help former SS officers. She's sort of the patron when the neo-Nazis meet. She's now eighty-eight, something like that, or eighty-seven. It's not an armed resistance, which is what the Werewolves are supposed to have been, but certainly from the operational point of view and the future, that kernel is still there. We're talking seventy years, more than seventy years. [Gudrun died at age 88 on my birthday this year, a nice birthday present for me].

03-01:44:47

Burnett:

It comes up and goes down. It sort of ebbs and flows, doesn't it?

03-01:44:51

Leitmann:

It depends. Also, I think it either is one of the underlying movements that are involved in the general move in that direction—if you look at the number of dictators that have arisen in the last few years, it's really astounding. I guess what it tells me is, it ain't ever over, which is very disturbing, of course, but you've got to be realistic. It didn't all die when the war ended. It takes different forms, whether it's Pol Pot up there as a Communist, or whether it's—

03-01:45:46

Burnett:

Viktor Orban in Hungary.

03-01:45:48

Leitmann:

Yeah, or [Recep Tayyip] Erdogan in Turkey. It's just never-ending. It makes one, I think, aware of you've got to do what you can do, at any given time, to stem that. It has, really, to do with—I mentioned this before—the current generation of Germans, and even the one before that, a lot of good people who feel terrible about the Nazi period, young people. We had a party in this house, and I remember sitting in the dining room, noticing that the wife of one

of my German post-docs was crying, sitting in the corner and crying. I walked up to her and I said, "Why are you crying?" She said, "I just spoke with your friend Werner Ganz," who was a refugee from Aachen, "and what happened to his family and all that." I said, "You're just sad about it?" "No," she said, "I feel guilty." Then I met with her and her husband the following week, and I said, "I need to talk to you about this." I said, "A long time ago, I decided there's a complete difference between guilt and responsibility." They're not the same thing. It's clear, if you're at all rational, that people who weren't alive then, or kids, how could they be guilty of anything? It doesn't absolve them of responsibility as members of the nation, as members of humanity. The question then is, what can they do in terms of being responsible? The main thing is that you try the best you can to prevent that kind of thing from ever happening. That's number one. Then, giving some kind of compensation to people, that's really a secondary thing. I always give the example of slavery. I say, hey, I'm an American. I wasn't a slaveholder. But I still feel responsible in the sense that we've got to prevent that kind of thing ever happening, of course, and maybe also some—and you do it because you know that people have been put in positions that make them inferior now, in terms of particularly economics, but also education, that you try to make this up to them. You can do things for a long time, and it may mean very different things to particular ways of what happened in the past, or what you can do in the present, but nonetheless that's really a task that you have to take on. They understood that. They understood that.

Also, later on—we're not in that period yet—it made it possible for me to go back and to interact with people. To this day—there are not too many left—I have trouble until I know a lot about the person of my own generation, Germans of my generation. I want to know a little bit more about them. Were they or their parents—and I know people who are very high Nazis, that I knew of that generation. It is something that does give sort of the operational part of your life a direction, even without being very specific about this, but simply as an underlying sort of philosophy, to say, how do you deal with problems of the world? Obviously, as an individual, there's a very limited role, but there are ways. Whether it's to make yourself feel good or whatever, I don't know. I haven't really given that much thought. That's one of the things we talked about. How did the experience of then, whether it's under the Nazis or in the war or in the trials, how did it influence me? It obviously did. It must have.

03-01:50:36

Burnett:

And you developed strategies for making meaning out of what happened? Because so much of it was senseless. So much of it had such an alienating, negative drive to it, and you coped with it by developing a positive orientation, like you did with this couple. "Let's build something positive out of it."

03-01:50:56

Leitmann: Because I think it's important, just as a human interaction. I felt sorry for them that they felt so guilty. You have to live in humanity. You can become a hermit, of course. That's the alternative. No, I'm serious. That's the alternative.

03-01:51:18

Burnett: Well, we're glad that you did not become a hermit.

03-01:51:21

Leitmann: Well, that's yet to come. [Laughter] Just getting to the present and to then, I am constantly amazed that this happened over seventy years ago. I wake up and I say, gee. I can remember things now that I've completely forgotten.

03-01:51:46

Burnett: It comes back.

03-01:51:46

Leitmann: This even comes to language. I'm beginning to forget English words. Something pops up, and the German word pops up, and then I have to think very hard, now what's the English word for that? I'm serious. That's very disturbing.

03-01:52:05

Burnett: It's a bit of a mystery.

03-01:52:07

Leitmann: The brain, you know. I have a theory that as things disappear from your brain—there's only so much storage—suddenly this old stuff fills up the free storage space.

03-01:52:23

Burnett: Yeah, the stuff that's on the surface goes, and your core memory is intact in that sense, maybe. Let's pause for now, and we'll continue next time.

Interview 4: June 21, 2018

04-00:00:17

Burnett: This is Paul Burnett interviewing George Leitmann for the University History series, and this is our fourth session. It's June 21, 2018, and we're up here in the Berkeley Hills. Welcome back, Professor Leitmann. We last left off—we were talking about some interviewing that you were doing.

04-00:00:42

Leitmann: Interrogating, really.

04-00:00:43

Burnett: We talked a little bit around that. I'm wondering if you could backtrack a little bit and talk about how that came to be. How were you designated for that role?

04-00:00:55

Leitmann: I think it's a little bit, right now, up in the air, because we talked about the interrogation of the Heinrich Himmler widow and teenage daughter. Since we have talked about my essentially interrogating people in terms of de-Nazification programming, that might have left the impression that it was part of that milieu, but actually it's not, and so what I think I ought to go back to is really my assignment—used to be called detached duty—as a CIC [Counterintelligence Corps] agent to interrogate at the Nuremberg war crimes trial. That happened just at the beginning of 1946, early January. I actually moved to Nuremberg. The whole assignment lasted about two months, at which time they gave me an option to stay on, and I chose to return to my regular assignment, which was much easier to take. I did that for a couple of months before being demobilized. This actually came as a complete surprise to me, that they decided to give me this special assignment. It's something I had never thought of, or even imagined. In fact, my predecessor in my job left. He was demobilized, and so they needed a quick replacement, I suppose. We had mentioned before, it was difficult for them to get people who spoke both languages well, and so maybe that's how I got it, because certainly, age wise, it was a surprise, because I was twenty years old.

04-00:03:07

Burnett: In *America's Secret Army*, which is a history of the Counterintelligence Corps—and there are other references as well—one of the things that's so striking about it to me is the fact that there was such a shortage of German speakers in the Counterintelligence Corps, which you would think would be a basic skill for that kind of work. So you had all these college graduates, who were just fresh out of school, going there. They didn't speak a word of German, and it had serious consequences for the counterintelligence effort.

04-00:03:42

Leitmann: Of course, somebody mentioned recently—I don't know the circumstance—that the requirement, or the wish, to have somebody who spoke essentially a flawless language—two languages—that was never realized in terms of our

secretary of state, Henry Kissinger, who has, to this day, not accomplished that. I have a feeling, actually, it's a put-on.

04-00:04:14

Burnett: It's part of his persona?

04-00:04:15

Leitmann: Yeah, because his older brother doesn't have a hint of an accent. So it's sort of interesting. Anyway, I ended up in Nuremberg, which was still in pretty bad shape as a city, very badly hit. The whole situation, in terms of the population there, was grim. The trial took place in the Palace of Justice, which they had patched up sufficiently to do that. I actually got almost no instruction of what to do. This was mentioned in this book you just mentioned. There was this sort of learn-by-osmosis kind of culture in the CIC. That certainly happened to me. They gave me an idea of what I was going to do, of course, but in terms of instructing me how to do it or anything like that, there was none of that.

I thought I might go back. We had talked about the interrogation of the Himmler widow and their daughter. There were maybe two other ones that I dealt with—most of the people I interrogated were really minor people. One of the major ones was General [Heinz Wilhelm] Guderian, who was the father of the German armored forces. It was his idea, sort of a father of the blitzkrieg, if you want to call it—

04-00:05:47

Burnett: And the panzer division.

04-00:05:48

Leitmann: Panzer division, yeah. He had been commander in the Battle of Stalingrad. That's probably the worst war story I can remember reading about. He surrendered. He was condemned by Hitler in absentia to death. He was brought back by the Russians as a witness. He was not accused of anything. They brought him in simply to be a resource. I met him one morning. I was told, of course, who he was. I had my uniform on, but the CIC agents normally had no rank insignia, just the US's on both sides, an officer's uniform. He ignored me. I greeted him nicely and told him who I was, and he just ignored me. He wouldn't even answer me. I recognized very early on, of course, why it happened, because it was clear just looking at me, a twenty-year-old—although I had a little mustache at the time—that I just wasn't equal enough to him to deign to engage in any kind of intercourse. So the next morning, when I came back—I excused him very quickly—I came back as a lieutenant colonel in the US Air Force, I figured those pilots were very young. He immediately reacted. We had a conversation. I don't recall in detail what I was supposed to ask him, but certainly there was some indication of—the Battle of Stalingrad, for example, was one of the topics, and his role in that. He was perfectly normal after that. It was an interrogation. It didn't last too long, maybe two or three sessions.

04-00:08:03

Burnett:

So there wasn't an agenda in terms of do you need to establish this or that kind of—

04-00:08:08

Leitmann:

No. They gave me general instructions, sort of a topic that they might be interested in, but certainly no program for the questioning. It's very strange. I think, again, this was sort of indicated in this book on the CIC, that there was a lack of underlying philosophy, a tremendous range of various geographic areas that the CIC operated in very different ways over the years, with a lot of abuses. According to this book, the agents were primarily interested in having a good life. Apparently, Henry Kissinger was one of those that had established his own empire. He was a sergeant, by the way, in his military rank.

So that was Guderian. The other people that I interrogated were, I think, either five or six female Germans, female concentration camp guards from Bergen-Belsen, which was one of the worst extermination camps. Again, I alluded to this before, I always controlled myself, spoke in almost this kind of voice. The first question I asked them was, "What were you doing, and what were these camps?" I still remember the German, one of them, sort of the spokeswoman, said that these camps were really sanatoria for sick Jews. I know the German phrase they used, *Krankenhaus*—no. Not "sanatoria." They used a different word. Anyway, that's what it was. It just slipped away from me. I asked them what they were, and they said they were nurses, you know, *Krankenschwester*. Again, I had a very short interrogation session with them, because I knew I wasn't going to get anywhere. Those were outstanding things that stick in my mind, because as I said, most of the other people are sort of minor people.

The other, I think, major impression left with me was that I had a pass to attend, when I was not busy, the trial itself. For me, given my history, to sit fifteen feet away from Hermann Goering and those people, again, it didn't excite me in any way, but just the realization that I was witnessing the kind of history that I couldn't have imagined. To this day, I can't believe that I actually sat there. From here to past the piano [about twelve feet]. I was in maybe the second row. Look at these people that—well, anyway, to this day, seventy-plus years later, it still astounds me that I was able to do that.

04-00:11:44

Burnett:

You know when you visit a famous place that you've seen in books before, and it has this kind of aura of an other-worldliness, and then when you're actually there, it becomes real in a profound way? Was it something similar to encounter these people? They're obviously diminished human beings. They don't have their epaulettes. They've removed their insignia and their medals.

04-00:12:05

Leitmann:

Right. And Goering had, by that time, lost two hundred pounds, or thereabouts, so that certainly made a difference. I remember that he sat there, still very much in charge. It was clear that the other of the twelve or thirteen, I

guess they were, for the most part felt that they were taking orders from the *Reichsmarschall*, to that day. So he was certainly a kind of overwhelming personality, even diminished as he was. I did allude, I guess in the previous session, that Sonnenfeldt, who was the translator for General Telford Taylor, who was the main prosecutor, actually, the US prosecutor, he recognized that you had to deal with people who were so self-taken in the proper way if you were going to do anything useful. He was terrifically successful in doing that. At the trial, that was really my major experience. Two months went by very fast. I got sort of a little thank you from the US Army. I inherited the secretary of my predecessor, who was meant to be more than a secretary, as I found out. She was actually a Nazi. Well, she was the head of the Sudeten German Nazi girls' troop, the BDM.

04-00:13:53

Burnett:

It's probably safe to say.

04-00:13:54

Leitmann:

Yeah. Very quickly broke up with her when she called me "my little Jew."

04-00:14:03

Burnett:

Goodness.

04-00:14:04

Leitmann:

Yeah. I still have a picture with—saying, "Don't forget me" inscription on it.

04-00:14:14

Burnett:

Well, I wanted to ask you, because there is—I remember this from a long time ago, and then I read about it again recently in preparation for this—one of the big motives behind the CIC was to handle the occupation. What are you going to do with these people who have been fighting tooth-and-nail a bloody conflict, and are angry, and their homes have been bombed, and how are you going to pacify this population? The absolute astonishment they had at the ways in which—

04-00:14:48

Leitmann:

How easy it was.

04-00:14:49

Burnett:

—they rolled over and just said, "Oh, you're now in charge? Okay." I want to ask you, from your perspective, with hindsight, is there a sense in which—well, A, how true is that? And B, is there a sense in which it falls into a stereotype about the German people as rule-followers, and that's how they got suckered into National Socialism?

04-00:15:17

Leitmann:

I really don't think so. First of all, by that time, they were used to taking orders. So maybe for that particular generation, that was probably a very natural thing to do. Some of them were just pragmatists, or maybe most of them were pragmatists. Again, I think we alluded to that. There were really no Nazis, except the people across the street.

04-00:15:49

Burnett:

Abdication of responsibility.

04-00:15:50

Leitmann:

Yeah, but on the other hand, perfectly willing to point to other people. “Not I, but they” kind of thing, which is a natural thing. The Austrians did that until the middle eighties, as a whole country. So it’s not surprising. I’m not sure that this wouldn’t be uniform in other countries. It’s particularly bothersome because the Germans at least, although probably it was a bad experience, had ten-plus years of a democratic government. But the situation was, of course, terrible economically and everything, so maybe that wasn’t the best advertising for democracy. In addition to, of course, the treaties that they were subjected to—I don’t want to use the word “subjected,” but—it made a difference. I think the Marshall Plan was a superb idea, although it was obviously, maybe largely, motivated by the Cold War by that time, but nonetheless, I think, we can see that it makes a difference now, because as I mentioned to you earlier, the Germans are tremendously miffed at the United States today, because they feel they are being subjected again to dealing with somebody who’s in that tradition that they suffered from for so many years. So maybe there’s a positive aspect to this in the end, that they’re not going to take it. They’re not going to roll over.

04-00:17:36

Burnett:

That’s very interesting. What you were saying earlier, or suggesting at least, is that the experience of National Socialism and the dominance of the Nazi party, of Nazism from, let’s say, ‘33 until the end of the war, nurtured a generation of people who had been cowed, people who had been threatened and bullied into submission, so it’s less a national character than the local circumstances of ten years of political conditioning. One wonders about—that that happens elsewhere, and in other times, as well. People can be fed a steady diet of lies or propaganda, or whatever you want to call it.

04-00:18:28

Leitmann:

Also, I think they recognize it. Having experienced it, they [the Germans today], I think, are maybe better judges of what is happening currently, simply because they’ve gone through it, and they saw the individual steps. Maybe that’s a major aspect. On the other hand, they are still—and I sort of marvel at that, or I wonder at it—there’s this—I can’t say admiration, but this sort of love for nobility, and that still exists.

04-00:19:12

Burnett:

In Germany?

04-00:19:12

Leitmann:

In Germany, yeah. My closest German friend, really my unofficially adopted sister, who was the Hessian minister of science and art, Evelies Mayer—I’m in touch with her practically every other day by email—she was a Social Democrat, so she was a Social Democrat minister for five years, and she was a professor before that, at the University of Darmstadt. She’s still very much

taken with nobility. She's introduced me to a number of dukes and duchesses. She's a very good friend of an admirable noble family in Hesse, who have a castle on the Rhine, where they live, where they established, after they returned to the castle, an art colony. There are sculptors and painters who live in apartments in that castle as well. What happened was, after Kristallnacht, the mother, the old duchess whom I met and had long conversations with, said, "We're not staying here." In 1938, they left and went to South America, and didn't come back until after the war. The heir of that family became the chairman of the Communist party of Hesse, a strange mixture of things. It's a curious thing. But they never really got over this idea. We had a counterpart to nobility through our industrialists, for example, the people who sponsored the arts and that kind of thing. Those are admired people here, too, Carnegie and the Rockefellers. They're sort of the nobility. So maybe this is a universal thing, or at least not as rare as I thought it would be.

04-00:21:33

Burnett:

It's also fairly politically diverse polity, I suppose, that there's Marxist and post-Marxists and greens, and now Fascists, too, in everything but name. But they're aware of their own history, and they're sounding the alarm in 2018. I wanted to ask, before we move on and move you out of Europe, about this post-war environment. It's a huge part of our literary world and our films, where we're very preoccupied with these moments in history, where civil society has broken down or is barely being held together. You're in this environment, and you're in the Counterintelligence Corps, and there is danger all around, I imagine. There's concern about saboteurs. There's concern about spies. Were you getting advice about how to conduct yourself in this world, or did everyone pretty much understand what was happening?

04-00:22:52

Leitmann:

I certainly don't remember getting instruction. Now, I was the youngest agent in that group of eight or ten people on the team. We had teams in different cities. I was the youngest, and the most recent. I had an older colleague who became one of my very best—I think I mentioned him before, Thomas Hermann. We certainly talked a lot about this. He was very involved in German history and culture. I was there a relatively short time. But, by that time we had got into the Cold War, and so maybe our concerns, by that time, were more with that aspect of our job than with de-Nazification.

04-00:23:45

Burnett:

It was already on? By 1946, you felt it was on?

04-00:23:49

Leitmann:

Absolutely, absolutely. Because, for example, almost from the end of the war, even the summer—I wasn't in the CIC in the early summer, '45—we were being visited, often, in fact, by groups of Russians who were looking at factories, either taking them back to make up for—

04-00:24:19

Burnett:

They dismantled factories, moved them into the Soviet Union.

04-00:24:22

Leitmann:

Absolutely. And also, of course, to sort of begin to establish what you might call a foothold in the country. It was the Russian zone, except for Berlin, which was quadripartite. There were four. All the area around Berlin, and then east, of course, was the German Democratic Republic, so-called. So I think they were very much involved in spreading that. But I do recall, again, the lack of professionalism in the CIC.

04-00:25:10

Burnett:

So it's fitting we were talking about the breakdown of civilization. We just had a power outage.

04-00:25:18

Leitmann:

Well, seems fitting. My exposure to the specific post-war dilemma, whatever you want to call it, in Germany, was reasonably short. I remember one particular delegation was looking at factories and other things like that, industry in Germany, in the state of Hesse, because my unit was in the capital that state, Wiesbaden. We put them in the best local inn. It was called the Golden Stag or something like that. They were in one room, or maybe two rooms. There were maybe six of them, something like—not a huge delegation. Then we took the rooms on either side, and we had bugged their room. So we were listening in and recording what they were saying, and we still didn't have tape recorders, or even wire recorders, the Germans didn't have tape recorders, but they had wire recorders already at that time. We recorded on discs, seventy-eights. They were, of course, speaking Russian to each other. Now we had this stack of records, but there wasn't a soul around that we knew who could speak a word of Russian. Nothing. I don't know what happened to them, but certainly the whole time I was there after that, they just sat around.

04-00:27:01

Burnett:

No one picked them up and spirited them off to Washington or something like that?

04-00:27:05

Leitmann:

Not as far as I know. These units, these teams, really operated very much on their own. For example, the description of the venality, if you want to call it, of some of these teams, our team was nothing like that. We didn't try to live really high. We had a wonderful life, given the war period. We lived in a villa, and we had a German noblewoman who was sort of the head of the household. So we had a very good life. Each one of us had both a private car and a Jeep, that kind of thing. But beyond that, we were certainly not making money.

04-00:27:54

Burnett:

Did you hear stories about people who were?

04-00:27:56

Leitmann:

The people who were, were the ordinary GIs, because they sold cigarettes in Berlin. I know of a number of cases—and it wasn't until the late summer that they instituted control over the money that you could send home. We got little booklets, and every time you sent it, they'd tick it off. Given your pay, you knew what you made, but you wouldn't be able to send—I know, for example, a couple of guys in my own unit—I was still in the engineers at that time—who loaded up a little trailer and dragged it behind the Jeep to Berlin, loaded with cigarettes. They cost something like eighty cents, a whole big carton. They made fifty, sixty thousand dollars on that trip, and sent home the money. That kind of thing, I think, was much more my experience than anything like that, the team I was on. But I'm sure it happened. It was extremely varied, but the individual teams were quite autonomous. I never saw anybody from higher up come to visit us, and we didn't really have a head agent. We were all special agents, pretty much equal, but some had a lot more experience, like Tom Hermann, of course, who was already in the CIC during the actual combat phase of the war.

04-00:29:30

Burnett:

You and Hermann were the only two who spoke German fluently, is that right?

04-00:29:35

Leitmann:

I think there were one or two others who spoke it well, but we were the only ones who spoke it fluently, because we were Europeans. The other four or five agents were not European, as far as I know. So that obviously had a lot to do with it. It was not a highly organized operation. I'm sure that varied. Some were, probably; some were not. I didn't go through Army intelligence school, but when I became a reserve officer, and just prior to the Korean War, they recalled reserve officers from each of the military districts in the United States and sent them to Fort Holabird—it was then called Camp Holabird—in Baltimore, which was the military intelligence school for the Army. We had a three-month course, which is another story. I still have my notes right down there. Notebooks.

04-00:30:43

Burnett:

About what you learned—

04-00:30:46

Leitmann:

Yeah, my notebooks. For example, we even had a course on constitutional law. What is legal, what is not legal. Obviously, wiretapping without a judge's orders. The next hour, we learned about wiretapping, and how to open letters and reseal them. [laughter] I have my class notes.

04-00:31:14

Burnett:

“So, here is what is expected of regular society, just so you know what officially the law is. Now here we're going to teach you how to break it.”

04-00:31:23

Leitmann: I have scrapbooks, for example, of that period, where we did, sort of as a final examination, a three or four-day course of arresting people and that kind of thing. Following them, for example. I learned how to put stuff in the gas tank so that they'd smoke and we could follow people around. In Baltimore, we did that to people's cars, and then followed them—

04-00:31:51

Burnett: Just to experiment?

04-00:31:51

Leitmann: Just, you know—

04-00:31:53

Burnett: To test it out?

04-00:31:53

Leitmann: Yeah. You saw the guy go off in the parking lot. We'd drop a couple of those things in his gas tank. Then he came back, and we learned how to follow people. I can show you the pictures of people standing up against the wall. We were, in a sense, sort of a group of people onto themselves.

04-00:32:22

Burnett: All that stuff has yet to completely gel. The Office of Strategic Services. The CIA is founded in 1947. So right around that time, they're starting to figure out what this kind of activity is.

04-00:32:38

Leitmann: Yeah, and also who can do what, where. For example, the CIC was not supposed to be doing anything in the United States. It was supposed to be an overseas operation. The FBI was in charge here, and then, of course, later on. I think this is still the law regarding, for example, the CIA. I'm sure that that isn't followed every day, because the idea is, now, if you're possibly in touch with somebody overseas, then you can be—

04-00:33:11

Burnett: Homeland Security has sort of put all those things together.

04-00:33:13

Leitmann: Enlarged, yeah. But there was a serious effort, maybe it was under Truman, to really delineate who was in charge, where, and what can be done by these—there are so many of these secret groups, and we've got just as many now, maybe more, and what they could do. There's always, of course, the whole idea—where does my jurisdiction stop? There's always an attempt to enlarge the jurisdiction, because it presumably, A, makes you more powerful, and B, maybe gives you the idea that you can do more. That's the whole idea.

04-00:34:01

Burnett: That was a tremendous burden at that time. You've just finished fighting fascism, and now you're fighting totalitarianism. Both systems are these

closed, autocratic systems that dominate people through fear and surveillance and force. The United States is trying to put together some kind of system to combat those systems without becoming those systems.

04-00:34:27

Leitmann: Yeah, and that's very difficult to do, actually, because you sort of learn from each other, and we see that today.

04-00:34:38

Burnett: Can you explain that?

04-00:34:40

Leitmann: I think the idea is that you're doing this for a good cause. It's the whole idea of maybe interrogation techniques, like waterboarding, that kind of thing—I didn't run into any of that, ever—is really justified by saying, "We're doing it to preserve democracy." Then the question is how far can you go, and it's not an easy question. For example, getting orders that are clearly illegal. What can you do about it? Can you simply say, "I won't take orders like that"? I think most people would go along with them. That was really the excuse in Germany: orders are orders. "I didn't want it. I know it was bad." Even to the extent that people would say, "I could not disobey the orders, because I took an oath of allegiance to the leader of the country." That's almost a religious in the Bible kind of thing. And so, "I couldn't disobey, because I would have broken the law," whether this was the real law or religious law, whatever you want to call it. That's a kind of personal decision, which I'm sure is very difficult, aside from, of course, that it's dangerous to disobey orders. But I think, even from the point of view of just being a good citizen, how far can I go in disobeying legal orders—and then the question what is legal is, of course, another question—that's unresolved.

04-00:36:36

Burnett: And the disturbing evidence that comes out in a number of psychology studies over the years, the Milgram experiment and so on, that show that, without any risk to the person, the person is just simply told, "Do that." If someone in an official-looking capacity says to do something, and the person does it, even though they believe that it's harming someone, and the person in authority says, "No, no, no, continue," without any justification or explanation, "No, finish the study," and the person does—

04-00:37:04

Leitmann: Yeah, there is that whole idea, and particularly in the United States, we're a state of law—this is always the idea. For some reason, they think other countries are not. That was the Napoleonic law, and we have our own, Anglo-Saxon, so maybe it's a better one. It is a very, very difficult thing to be different, first of all, to stand out, to obviously subject yourself and your family to retribution and danger. There are so many aspects that make people obey what is clearly a bad thing, and I think, in Germany, it rose to a level that obviously is as bad as it can get. But I understand it. I understand it. The

people who are heroes are very few, very few. People are self or family-oriented, and that comes first. Then morality, I think, is really sort of an add-on.

04-00:38:37

Burnett:

That morality needs to be expressed with other people. It's very difficult to do by yourself. You need to find others with who—

04-00:38:44

Leitmann:

Yeah, and then it gets to be a very dangerous thing, because then you're suddenly an underground group or whatever.

04-00:38:51

Burnett:

Or it could be a counter to some kind of organized force that is acting badly.

04-00:38:57

Leitmann:

Yeah. So this is why I think it's very important, if you do want to object to what is happening, to use all possible legal means. That becomes a little fuzzy sometimes. You'd be surprised how relatively few people do even that. It may be a monetary contribution. People say, "I can't afford it. I don't have the time." There are always excuses. Unfortunately, sometimes, it's too late when you realize you had better do it. Then it really becomes too dangerous to do it.

04-00:39:35

Burnett:

Yes, exactly.

04-00:39:36

Leitmann:

That's, I think, what happened to a large extent in Germany. That, coupled with, of course, the bad economic situation. It's easy to sort of poo-poo that and feel superior, but I think that these things can happen anywhere.

04-00:39:59

Burnett:

Right. It's a human problem, not—

04-00:40:01

Leitmann:

They may be less likely in some societies. Fortunately, we can still speak up. Every time I watch CNN and marvel at how they belabor our president, I say, it's great that they aren't yet going to jail. Let me put it this way.

04-00:40:23

Burnett:

Maybe that would be too, too obvious. They should check their cereal for polonium, perhaps. Anyway, I wonder if we have time for one more story about that period, and that is a Tom Hermann story, I believe, or at least he leads you to this work. Can you tell me about—is it a French pronunciation?—the Case Poupette?

04-00:40:45

Leitmann:

The Case Poupette. Poupette was the younger sister of Madeline de Mumm [of the Mumm champagne company]. Now, I've been trying to think back. This must have been maybe in March of '46, so towards the end of my service

in the CIC. Tom Herrman and I were sent, for two or three weeks, to Paris, to work with what is historically called the Deuxieme Bureau, but actually it was disestablished quite a long time ago, but that term is still used. It's been around since Napoleonic times. It's sort of the Secret Service of France. To help them in ferreting out collaborators. The case in particular that stayed with me was something that was called the Case Poupette, because it was brooded about that Madame de Mumm, who was, in those days, still probably only in her early thirties, had been or was a collaborator of sorts.

The German ambassador to France, which was occupied France, of course, where the high commissioner, but Vichy France in particular, was in fact Otto Abetz, who came into our story earlier. Remember that I had "borrowed"—in Colmar, when the building I was in was burning, I got a hold of a folder of art. It was, as I said, woodcuts, signed Otto Abetz. He was an artist, clearly. He was a great Francophile, actually. That's probably why they put him in as ambassador, because he would be very good. So it was brooded about, and the Deuxieme Bureau thought that they better look into it. Now, I've looked very hard since then. I could never find anything that established this. But at least that was the case, and Poupette, as I say, was the youngest sister of Madeline. So we then became quite friendly with them, and the Deuxieme Bureau was located, physically, in the police headquarters of Paris, which is a whole block near the Notre-Dame. The Gestapo and their headquarters were in that same building during the occupation, and the Deuxieme Bureau very proudly, showed me that the Germans had wiretapping equipment for every telephone in Paris. That they kept it. They were still using it under [Charles] de Gaulle. That was interesting.

04-00:44:05

Burnett:

So one of the consequences of the occupation was that they had a kind of surveillance state in place. What was the atmosphere like in Paris, compared to the other—obviously it was spared bombing that the Germans had suffered, but it had suffered five years of occupation. I wonder, if you took the temperature of the city, what was it like? What was going on? Was it exultant? Was it exhausted?

04-00:44:45

Leitmann:

I wouldn't say either. I think that the French I knew, my closest friend, and colleague, eventually, once I went into academia, he and I talked about this a lot, but that was already the early sixties by then. They were really very pleased that it was possible, had been possible, to preserve at least the treasures, art, and architecture, of what they obviously considered the greatest city in the world. In a way, it is, of course. There was that kind of self-satisfaction that they could continue, actually, as the cultural capital of the world. They were very pleased with that. And it's true. That's, of course, what happened. Both sides agreed to not destroy that city, which is amazing in a way, because that didn't happen in other places. The Germans made sure that, starting with Warsaw and going on with Amsterdam and Rotterdam and all

the places, did not try to, in any way, make any concessions. But they did in Paris, so there was sort of a mystique, I guess, about the city. I think that carried on into the way the French felt about themselves. They were very proud, of course, of the so-called Resistance, which was over-estimated, from all I can determine. The percentage of the population that were Resistance—until after the invasion, of course. Then there were a lot more. To my surprise, I looked into this, because who were the early Resistance, in Paris in particular, starting almost at the beginning? It happened to be a group of Communists, and they happened to be a bunch of Jewish tailors, very poor people. They were sort of the first Resistance, as a group. There were, of course, individuals in the Resistance all along. That was sort of surprising. I looked into that. I had to be careful to, I wouldn't say denigrate, but at least to make the claims of resistance greater than they were indicated. It wasn't resistance of a guerrilla type. There was never any of that. There was sabotage—

04-00:47:35

Burnett:

More subtle forms.

04-00:47:36

Leitmann:

Yeah, more subtle. There was a certain amount of sabotage and that kind of thing. If I look at the collaborator part of the population that the Resistance won, I'm not sure that they were about the same. Or maybe the collaborators, there were more. Women became prostitutes for economic reasons, and their customers happened to then be Germans. I don't know if that made them collaborators, yet they were treated that way.

04-00:48:06

Burnett:

They certainly were.

04-00:48:06

Leitmann:

Had their heads shaved, and paraded around.

04-00:48:10

Burnett:

I think there were eight thousand murders after the war. There were these debts being settled, or accounts being settled. The *épurations* I guess they call it.

04-00:48:20

Leitmann:

And again, it's something that can be overdone once you're in charge again. It's retribution, earned retribution, but also making yourself feel good that you now can take it out on these people. But there were people, obviously—for example, the French police, the Gendarmerie, they were almost to a man collaborators. They rounded up the Jews and brought them to the Germans to be hauled off. That's commemorated these days as a terrible thing, but it was not at all unusual. I think, just as an aside, the sort of nth degree of that was in Croatia. In Croatia, where they had a Fascist government, of course, by that time, where they paid the Germans to take Jews. The Gestapo said, "We've got enough of them. Just leave us alone." They said, "Well, we'll pay you so

much to take them.” You get that, to different degrees, of course, almost everywhere. It’s just human beings. No damn good.

04-00:50:01

Burnett:

It’s complicated for you as a kind of reckoning with that in the context of what you experienced in Vienna years earlier. You’re seeing the full consequences of that line of action and that ideology, and its full expression. You saw it in the camps, and you saw it in the behavior of these people.

04-00:50:25

Leitmann:

It’s not a good advertisement for humanity. Let’s put it this way.

04-00:50:31

Burnett:

No. That’s true. Let’s step out of time for just a second to acknowledge the recognition, speaking of France, for your service with the First French Army. Can you talk about how that played out? You weren’t recognized immediately, right? Or were you given a citation at the beginning?

04-00:50:58

Leitmann:

There was, I think a day or two after the liberation of Colmar—again, the French were allowed to go in, as they did in Paris, before American troops. There was a ceremony, and they conferred on units, and sometimes individuals in the units, medals. There were no medals around, but I remember the little ribbon that goes onto the medal bar. I was one of the people who got that. It was called the Croix de Guerre avec Palms. It was sort of brooded about, that it was really a unit kind of thing. French simply like ceremonies. I was, for a long time, convinced that it was General de Gaulle that did it, but it was really the commanding general of the French First Army, who was tall, and the French generals have hats about this high. His name was [Jean] de Lattre de Tassigny. Although if you read some of the commendations I got, or people will say that General de Gaulle gave me this, it’s not true. I have to make a little correction.

Then nothing happened. I never got the medal, actually, or anything. It wasn’t until, I think, my eightieth birthday, that happened to be the sixtieth anniversary of the Battle of Colmar. Some of my friends, including, for example, a woman by the name of Gertrude Humily, who was the dean at the first French *grand école* for women only in engineering—there is, to this day, like the [*École*] *Polytechnique*, there is a counterpart for women students, still today. We became very close, almost about the same time that I became close to Evelies Mayer, the Hessian minister, to Gertrude Humily. It turned out, as a matter of fact, that she had come from Vienna and become French, not because she was a Jew or anything like that. Simply, that’s the way it worked out. She thought that it would be nice if that coincidence of my eightieth birthday and the sixtieth anniversary of the liberation of Colmar could be combined and that I could be given the medal.

Now, these schools are really under the Ministry of Defense, like the *Polytechnique*. She knew a general, Jean Roche, as I found out later. She mentioned this to him, and they had better records than our Army did. So they looked back, and they said, “Yeah, we gave out these things there in Colmar, and if George got it, he certainly should have the medal.” That’s how I was invited, Nancy and I, to Paris. It must have been June 2005. Our son flew in from Jakarta, where he was with the World Bank. We stayed at the main officers’ club of the French Army, which is a whole block in Paris. It’s called the *Cercle National des Armées*. We got two rooms there. We had to pay for that, by the way. Josef, our son, got there before we did, so he took the bigger room. [laughter] He said he’d be willing to change, but we told him to stay.

It was a very nice ceremony. It took place in the officers’ mess, dining room. Two generals, General Roche and another general, and their wives, and Josef and Nancy and I were there. We had a little champagne lunch first. The French are very wonderful about that. Afterwards, they were going to confer this medal. General Roche was going to confer this medal. Nancy had given me lessons in how to say a few words of thanks in French. My French was rudimentary. But General Roche gave his twenty-minute speech in English, and so then I answered and thanked him in English as well. I have pictures of all that. That was very nice. So I have the medal, and I have the program that they printed for this. That was from the University of Berkeley. That’s actually what it says on the thing, or Berkeley University. I have that in my scrapbooks. So that was nice.

The next thing that happened was, of course, that they found that I’d been working closely, even scientifically by then, particularly with Austin Blacquière and people at the University of Paris. So that was an extra plus, and I did that for many years, say from ‘61 onward, or ‘62. General Roche, I’m sure, although I have no official word of this, must have been—he was a commander in the Legion of Honor—he must have put that on and put it through. Then, four years ago now, a little over four years ago, I was informed that they were going to make me a chevalier in the *Légion d’Honneur*, which was a complete surprise. I never in my life expected that. They said I don’t have to come to Paris, given my age, because by that, I think was eighty-eight, but they would do it here. Then the dean at Berkeley, Shankar Sastry, he loves this kind of thing, so that was all put on in the Faculty Club in the great room. That was terrific. They were giving the *Légion d’Honneur* to quite a few American soldiers. That got to be policy, that if they could establish they were in combat units, particularly if they were associated with the French Army. But usually in groups, like four or five from that unit would get the *Légion d’Honneur*. Here, it was just going to be me. Furthermore, we had a contingent of twelve or fifteen PhD students from the *Polytechnique* here. So they air-mailed their parade uniforms over for the occasion, and they formed the French honor guard on one side, and the ROTC unit from Berkeley on the other side. Then they had their hats, the fancy Napoleon hats. That was really a very nice, typically—they just do ceremonies better than anybody else.

It was a long ceremony. It lasted a half an hour. George Breslauer, who was the executive vice chancellor and provost at the time, gave a speech on behalf of the administration here. In fact, I just had lunch with him and his wife this Sunday, because he's now the director of the Magnes Collection on campus. Shankar was the host, so he spoke. Then the colonel who commands the Golden Bear Battalion gave a long talk about the brotherhood of arms. Then the French consul general said, "On behalf of the president of France"—first of all, he said, "You understand that I have this accent, because by French law, we must use this accent on official occasions." [laughter] He was very funny. But then he made a point of saying, "This is the very highest honor that France can bestow on anybody," which is true. It certainly, to this day, is. Nancy was very involved, because she did her graduate studies, and was an undergraduate major, in French, very involved with collecting books for French libraries that were being restored. She got an award and called it the Fleur-de-Lis award. All that was mentioned. Of course, they brought flowers to Nancy and all that. That was very nice. When I really think about it, all these orders and medals and stuff, I don't take them as seriously as I should, because I know that there are always people, A, who deserve them more, and B, it's the old story, we can't give you a million dollars, so we'll give you a medal kind of thing. So there is that, but I'm not poo-pooing it. I'm very grateful, of course, and I enjoyed it every time. There's no question about it.

04-01:01:54

Burnett:

I think the key piece, though, is that they are grateful, that the nation is grateful, and that's something to be proud of, I think, all machinations aside. I think any veteran usually feels like the folks who deserve it are in the ground, or they're terribly wounded, and there's a certain amount of truth to that, of course.

04-01:02:19

Leitmann:

It's absolutely true. You sort of stand in for somebody when that happens, and I certainly look at it that way. Otherwise, you'd be fooling yourself. The older I get, the more I believe that, and it has to do with academic honors, and honorary doctorates, members of academies. It looks a lot better on paper, and posthumously it looks very good.

04-01:02:51

Burnett:

Excellent, yes. Absolutely. [laughter]

04-01:02:53

Leitmann:

Because when I go to memorial services, these things are always trotted out. I told Nancy, "When I die, be sure that they have a purple pillow on my casket, and put all the medals on that." [laughter]

04-01:03:10

Burnett:

I think there is always a certain amount of humility that goes into the package of accomplished folks. I have a hard time interviewing people who don't take proper credit for things, and I understand why. Here's the thing: you are in the

flow of history, so if not you, there might have been somebody else in that position. We accept that. But the fact is, you were there at that time, and you acted the way you did, and you acquitted yourself well, and people want to recognize that.

04-01:03:49

Leitmann:

Okay. Well, I appreciate that. I'm not being nasty or not appreciative. All I'm saying is I just don't take these—sort of the epitome of this kind of medal thing, of course, were always the Russians, the Soviets. I remember going as a security detail when General—

04-01:04:21

Burnett:

[Georgy] Zhukov?

04-01:04:22

Leitmann:

Was it Zhukov or [Ivan] Konev? I forget. I saw him with General [Alexander] Patch, and the Russian general actually had an extension, a hard extension, to his uniform, with the actual medals. Our generals have all these ribbons. That's okay. He actually had something that stuck out beyond the shoulders to here, with the medals hanging on it.

04-01:04:46

Burnett:

To accommodate the large number of medals he had. He had like a cantilever structure?

04-01:04:54

Leitmann:

Yeah, absolutely, absolutely. But people are truly—they're honored, and the Russians particularly—I knew some of their veterans—they take this very seriously. It's the Great Patriotic War. They lost, of course, twenty million people or more. So I understand that. It's just that it's too bad that it's particularly military accomplishments that are honored in this way, and it would be very nice if we could stop doing that. I'm sure it's not going to happen. It's not going to happen.

04-01:05:38

Burnett:

Let's return, then, to your return to Nuremberg. You come back from Paris to Nuremberg. Are you finished at that point, when do you—

04-01:05:51

Leitmann:

That's right, I came from Nuremberg to the Paris job. Then it was the normal kind of procedure for—I think I was demobbed in early May of '46. Actually, I was invited at the time to stay in the CIC. I think one of the things that presumably would have attracted me is they wanted me to be an escort the people they wanted to bring back to the States for further interrogation. I just decided, if I'm going to go to school—the GI Bill, of course, had come in, and so that was a big incentive—that I had better do this. For example, Tom Herrman stayed on. He was a lieutenant colonel, of course. One of the things they then offered me was to go and join the historical division in the Pentagon. They were writing the history of the war for various branches. I

didn't want to do that either; I wanted to go to school. But I told him that was open, and he did that. He got that job for two or three years. I can tell stories about him for hours. I'm not going to do that.

04-01:07:21

Burnett: We can think about that, maybe.

04-01:07:25

Leitmann: I decided, I'll just quit and go to school.

04-01:07:34

Burnett: When you're demobbed, you went from Nuremberg to Vienna, is that right?

04-01:07:39

Leitmann: No, no. My first return to Vienna was when I was still at Nuremberg. It was actually the end of January. I took one of the first flights into Vienna, because the Russians did not let the Americans into their zone until about that time. Then it became a multi-zone operation, as it was in Germany.

04-01:08:05

Burnett: When was that, roughly?

04-01:08:06

Leitmann: That was in January of '46. I flew into Vienna on a DC-3/C-47 plane, operated by actually—there was a civilian operation flying for the Army. It was an interesting flight, because the only other passenger was a nurse, and we flew into the military airport in Vienna, and the pilot was drunk.

04-01:08:41

Burnett: Really drunk?

04-01:08:42

Leitmann: Well, every time he saw Russian troops, he would go way down and wiggle his wings at them. We had steps, of course, from the plane. He actually fell down and fell on the ground. These little things stick in my mind. So what did I do? I had a hotel room, of course, and I stopped on the way from the airport to my hotel room at the Central Cemetery, which was actually on that route, and tried to visit my grandfather's grave. He was the one who had been lieutenant colonel. The gatekeeper had the books, because it's a cemetery that was multi-religious. They had a Catholic section, a Protestant section, a Jewish section, and maybe a Muslim one. I think at that time, probably not. When I told him I wanted the Jewish section, he showed me on the map where it was, but he didn't warn me. When I got there, every gravestone had been smashed, just flat. But I found the place that's closest, because he told me it was by numbers, and I sat there quite a long time, just contemplating this whole thing. Then I took the taxi to the center of Vienna, where I had a hotel. What year was that? That was '46, right? That was '46.

04-01:10:27

Burnett: Yeah, January '46. The winter.

04-01:10:31

Leitmann:

Yeah. Then I went to the apartment house where I was born. That was an interesting experience, because as I had mentioned before, there was nobody who really indicated any concern about us when the Nazis were—

04-01:10:49

Burnett:

Your neighbors didn't help, or they helped themselves to your possessions.

04-01:10:52

Leitmann:

But the concierge was a woman from Bohemia. She even still had an accent. When I was a kid, her name was Stasny. See, I remember her name. I had left there five and a half years before that. I left in May '40 and I came back in January '46. But she spoke German, A, with an accent, and B, she spoke the local dialect, and I know exactly what she said. [phonetic – yaysoos-na-der-schurl-is-da] which is Viennese dialect for saying, "Jesus Christ, little George is here." We recognized each other. She was actually the head of the Communist Party section for that part of Vienna at the time. The Russians had put her in, because she was really from Czechoslovakia. She was an immediate kind of connection. It was really very touching. The street had been bombed, so the building where I was born was not in ruins, but damaged, and had been fixed up enough.

That was my first stop in the city. Then I went on to my uncle's place in the Prater, the amusement park. We had mentioned that in an earlier section. His place was gone, of course. The whole area had been bombed, so I don't know what—there were no trees. All these beautiful trees had been taken down, cut down, for firewood. The giant Ferris wheel was not operating at the time. It was kaput. Then I visited my paternal grandmother's apartment, where we stayed after they had thrown us out of our apartment in the place where I was born. I have pictures of all that. I didn't stay very long. I think I stayed three days, something like that. I had to fly back to Nuremberg.

04-01:13:27

Burnett:

Did you accomplish or find what you wanted to see when you were there?

04-01:13:34

Leitmann:

I think I just wanted to reconnect. I was curious, of course. I don't have very definite recollections of what motivated me, other than wanting to go back and see what had happened. It was sort of a long weekend I took, like Friday to Monday. It was a free flight. That was my first return, and I was able to describe that on my eighty-fifth birthday, in fact, when the then-president of Austria had invited me to the imperial castle, where his offices were, the president prior to the current one. His name was Fischer. He wanted to know what I did on that day. "When did you come back the first time?" So I gave him exactly the same story I gave you. You know what he said to me? He said, "Now we have rebuilt that part of the cemetery to its former glory." He said, "I know it very well, because my father-in-law is buried there." He's married to a Jewish woman. So that was interesting. Heinz Fischer. We spent

an hour together, which was very unusual. But that was much later. That was in 2010, I guess.

04-01:15:12

Burnett: We'll come to that. I guess what's obviously clear before you even went there was that your future was not there.

04-01:15:22

Leitmann: Oh, yeah, absolutely. Absolutely.

04-01:15:24

Burnett: This place is gone for you.

04-01:15:26

Leitmann: No, no, absolutely. I could take a dual citizenship today, for example. I wouldn't think of it. I might have, when the Social Democrats were in the government. I might have done that, but not with the current government in Austria, which is actually neo-Nazis. I have very, very close friends there to this day. I communicated with one of them this morning by email, because he had sent me this article in *Der Spiegel* about how they felt about—

04-01:16:13

Burnett: About Trump?

04-01:16:14

Leitmann: Yeah. History goes on. You're the historian. I can see that this is a fascinating subject. It's my major reading these days.

04-01:16:25

Burnett: Yeah, I bet. It's a way of thinking about what is next for you. So there is a process of demobilization. What is next? What happens? You return to the United States?

04-01:16:41

Leitmann: Right. My mother had an apartment. My grandmother, her mother was still alive, so she stayed with her mother. That's the one thing that made my going into the Army a little bit less traumatic. She had her own mother with her the whole time, and I think that made it—she was a very strong woman, very competent and self-sufficient. So that's what happened. And I had the GI Bill.

04-01:17:14

Burnett: Was that a nice reunion with your mother and grandmother?

04-01:17:16

Leitmann: Oh, yeah, it was wonderful, of course. And my grandmother, who was a little bit mad at me, because when I got home I shaved off my mustache, and she was used to men with mustaches. She wasn't happy that I did that. She really tried to dissuade me from it. It wasn't a big mustache. It was just a little one. Little funny things. I became very important to her, because by then, she had medical problems. I used to take her to the hospital. That was one of my first

tasks. It was an interesting experience, of course, because I left almost within a year of coming. I left in—

04-01:18:12

Burnett: May of '40?

04-01:18:13

Leitmann: No, no, it was four years. Three and a half years.

04-01:18:21

Burnett: It's a long time for an adolescent. You had your adolescence in the United States, and you grew and became a young adult, and then you're in the Army.

04-01:18:34

Leitmann: Immediately, right. Of course, no father, so that sort of maybe took a part of the family. I didn't mind being in the Army at all. I told you, I even liked the food. But anyway. I immediately applied—I wanted to live at home, and so it was a question of where. I had, by that time, decided that I would stay in the basic sciences. It wasn't engineering, but I had my engineering exposure. So I applied to Columbia University. They had a very good program, particularly for returning veterans. They did things like giving you elective credit for anything that's physical education. Anything like that, you got credit right off the bat. Then they had a very nice incentive program. If you had got only A's and B's—real B's, not a B-minus—for every six units of A, you got a free elective unit. There was only one semester in my whole period at Columbia as an undergraduate that I didn't make that. I'll return to that in a minute. So I started as a physics major.

04-01:20:08

Burnett: I don't mean to interrupt, but I wanted to ask you about—you said, "I wanted to go to school." Did you have in mind going to school, and then you would figure things out, or did you have an end goal: I want to be a scientist? What was your thinking?

04-01:20:25

Leitmann: I'm not really sure that I thought this through completely. I knew that, A, I wanted to go to school. B, it certainly was something that I had been prepared for, because I went to gymnasium, and my parents certainly expected me to go to school. Certainly my mother wanted me to go to school. I lived at home, so it was inexpensive. Even in those days, at a private school like Columbia, tuition was negligible, but I didn't have to pay it.

04-01:20:57

Burnett: Did you have scholarship?

04-01:20:59

Leitmann: No, I had the GI Bill.

04-01:21:00

Burnett: Oh, the GI Bill. I'm sorry. Of course you did.

04-01:21:02

Leitmann:

It was terrific. And I had a good GI Bill, because combat soldiers got extra credit. They determined what level of education they would pay you for, and it was through the master's degree. So I knew that I was going to go to graduate school, provided I made it academically. Again, just as high school was a wonderful time—I told you that I just adored going to high school—it was the same at Columbia as an undergraduate. I took a lot of evening courses. Columbia, I think to this day, has a wonderful program that, even as an undergrad, you could attend any course, provided you did what you needed for your major, as long as the instructor let you in. So, for example, even as an undergraduate, I took two courses in social anthropology from B.J. Stern. B.J. Stern was a very famous anthropologist. He wrote a textbook called *When Peoples Meet*, which was sort of a standard textbook, and he was a Marxist. In fact, he was the editor of *Science and Society*, a Marxist journal. All this will come in later, so I want to bring it up now. It was wonderful.

In order not to interfere with the courses that I had to take, I would take courses in the evening, and used to go to Horn and Hardart. Was sort of an automat store, like a delicatessen. There was one right near the Columbia University campus, where I had a standard meal, evening meal. It was toast with cream cheese and chocolate milk. I still remember that. [laughter] It was my evening meal when I went back to campus. I went to campus, and back, of course, on the subway, and it got quite late sometimes. That was just great. I still am so thankful, because, to this day, I can't believe how lucky and pleased I was with the whole operation. I did well, as I've said. For example, I had a number of Nobel laureates as professors, including Charlie Townes. When I became a graduate student, he was my quantum mechanics professor.

04-01:24:01

Burnett:

Columbia was an enormously prestigious institution in the United States.

04-01:24:06

Leitmann:

And it should be.

04-01:24:07

Burnett:

And it was involved in war work as well. There was a lot of research on ballistics, I believe, and the proximity fuze [MIT was the center of that research].

04-01:24:20

Leitmann:

Certainly atomic energy. For example, Enrico Fermi was one of the main—

04-01:24:24

Burnett:

The rad lab?

04-01:24:25

Leitmann:

Yeah. In fact, it turned out that when I did my master's, I used part of that lab for my project. That's yet another story, which I'll get to when I get to my graduate work. It was terrific, really terrific. As I said, I had only one semester

when I didn't make this extra credit thing. I went to summer school twice. The other semesters, I had only A's and B's. I had a couple of semesters where I garnered eighteen or nineteen units.

04-01:25:10

Burnett: How many courses is that, eighteen units?

04-01:25:14

Leitmann: Probably the equivalent of four courses. Four or five courses.

04-01:25:20

Burnett: So your approach was, you're a young student, and you were just drinking up the knowledge. You're not thinking, I'm doing this to get a PhD, and I'm going to have this career. Or I'm going to go into business—

04-01:25:34

Leitmann: No, I wasn't even thinking about a PhD at the time.

04-01:25:37

Burnett: You were thinking, I want to learn. That was your goal.

04-01:25:40

Leitmann: Yeah, I wanted to learn, and of course I wanted to make a living afterwards. Obviously, it was partly that. I wasn't going to be just a scientist.

04-01:25:52

Burnett: Right, but you didn't know what that living was at that time?

04-01:25:54

Leitmann: No, absolutely not. These were the 1940s, '46 through '50. The people I remember were really people that sort of stick out. For example, in calculus one and two, advanced calculus, I took as an undergraduate, of course, from a professor by the name of Ervand Kogbetliantz. He was originally from Armenia, but was educated in Paris. So he had this mixture of an Armenian and French accent, and people found it very difficult to understand him. There were always two sections of calculus, and one guy had fifty students, and Ervand Kogbetliantz had five. That was a challenge to me. So I took his course. Second semester in advanced calculus, he gave me a C. I went to see him and I said, "Professor Kogbetliantz, you know the policy here?" He knew that this extra credit was available. He was an amazing guy. I wanted permission to retake the course during the summer and make it up. I still remember, in his inimitable accent, he said, "A C from me is like A from anyone else." I said, "Well, you know it and I know it." He wouldn't do it. He wouldn't let me do it. He was really serious in many ways. For example, I knew a couple of girls, twin sisters, who rented an apartment—he lived with his daughter in a two-story townhouse along Riverside Drive, and he rented out the downstairs. They told me that they could hear him upstairs at night, stomping around and cursing, "No, no, no, stupid, stupid." He was grading the homework. What he returned to you, in red ink, was more than you had written. That's how serious he was. For examinations, he dressed up. Now,

regular examinations, just in a fancy coat, but for final examination, in striped trousers and a morning coat, I swear to you. You know, with a stiff collar and all that.

04-01:28:56

Burnett: What do you think that meant? What did he want to signal by doing that?

04-01:29:00

Leitmann: It's just that he was in that tradition. Of course, he had been educated at the Sorbonne, and I guess it was—he still lived those days. He was truly an outstanding person, and he was the founder and president of the Three-Dimensional Chess Society of the United States. He and his daughter were the founders. There's a wonderful picture of him in *Life* magazine. On the final page of *Life* magazine, they always had babies with three heads, that kind of thing. Ervand Kogbetliantz peering out—he had a little mustache, and always cut himself shaving—he always had a plaster here—between two of the chessboards, looking up like this. It's somebody that I just never forgot, and never will forget, because he didn't have the Nobel Prize or anything like that, but he was truly an outstanding person.

04-01:30:00

Burnett: Did you learn in that environment from him? Did you feel you learned more from him than other professors?

04-01:30:09

Leitmann: First of all, I needed to work harder, because he was so tough, of course. It meant more to me. I was sort of proud, in a way, that I made it, because, first of all, it took people two, three months to understand his lectures. I was more used to accents and all that. I remember, in the second semester, he had had heart problems. He sat down, and he let me write the lecture on the blackboard, as he dictated the equations. He had a very interesting way of doing it. At Columbia—maybe they still have it to this day—there is the wall on the entrance, the hallway, and then the U-shaped walls were solid walls, with windows, of course, but there were blackboards going all around. They could be pulled down. He would start the proof of a theorem, for example, on the left top corner. Wrote it here, then across here, then across here [motioning around a room]. Then he would run around like crazy with chalk and eraser, and then erase it and say, "I simplified it." I still have my notes. That's one thing I did, and I rewrote my notes. I have them downstairs. I left the whole page for every one of those lines. When he simplified, I would simply rewrite that line, only the section that he substituted on the next line, and I could do this down the page. I had my system, because I didn't have this way of writing all the way—

04-01:32:14

Burnett: You did a version of it for your notes.

- 04-01:32:18
Leitmann: That stuck with me, and it was a challenge in a way, to say, I'm not going to let this defeat me. As I say, I was able to get a lot of units. So I finished in three years for my bachelor's.
- 04-01:32:33
Burnett: Can I just follow up on that by asking you about the rewriting of the notes? Can you tell me why you did that?
- 04-01:32:43
Leitmann: It was a way of going over the material, making sure I understood it. Or if I had questions, that would give me a way of knowing where I needed more information.
- 04-01:32:58
Burnett: They've done studies that have shown that there's a physical process in writing and rewriting that commits information more to long-term memory, better than actually typing something out.
- 04-01:33:12
Leitmann: Yeah, possibly. Of course, in those days, we didn't have computers, so it wasn't a question of using Word or something.
- 04-01:33:20
Burnett: But it was a custom? It was something you knew how to do? Or you just felt this was your way of doing it, to rewrite it? You knew it would help you?
- 04-01:33:27
Leitmann: No, I think this one-page thing was my idea. Rewriting it was not my idea. I know people did that. It got to be even more important later on, particularly in the one year of graduate studies, which was a lot harder, and my grades were not as good in graduate school. I had essentially an A-plus as an undergraduate, and probably more like a C-plus in graduate school. But a C was a good grade in those days. There was no grade inflation. That was really very different. That came much later, this idea you can't have C's.
- 04-01:34:19
Burnett: Were there students who were kind of legendary at the time, either in your year, or year above or before you, that stood out as getting A's? Were there people who did get A's?
- 04-01:34:32
Leitmann: I really can't recall that. In fact, I don't remember any of my co-students. My best friend, who is still my best friend, is somebody who went to high school with me, Werner Ganz, and he went to CCNY [City College of New York] and became a civil engineer. He didn't go to graduate school. He lives now up in Sonoma County, because his youngest son was a vice president of Boeing. His son got his PhD at MIT [Massachusetts Institute of Technology] and became vice president of Boeing, and then the CEO and president of Boeing Northern Europe, and just retired from Boeing three years ago. So he moved

his father up to Sonoma, where he bought an estate. Anyway, another story. I try to stay in touch with people as much as I can. I write a lot of email letters to people. I'm one of those crazy people who answers right away. It drives people crazy.

04-01:35:42

Burnett:

I don't want to be a psychologist or anything, but you had enough action between the ages of twelve and twenty for most people in a lifetime. I imagine there's a certain push towards stability, and once you make a friend with someone, you want to keep that connection. Is that true, do you think?

04-01:36:03

Leitmann:

Yeah, and I really like people. Not every person I'm in touch with today, of course, but the people I like. The interesting thing—and this is again the question of guilt and responsibility—is that I had a birthday in May, and of the many email congratulations I got, over 80 percent were from Germans. I find that interesting. Even people that I didn't get very close to, but somehow I stayed in touch with. I'm very glad about that. Let's put it this way. It's very easy to sort of lie down and go to sleep and not be bothered. To me, it's just the opposite. I get up very early. First thing I do is go to my email and delete most of it, and then I start answering people. I've already, this morning, answered the person who sent me an article from *Der Spiegel*, and I sent it to a number of other people. I may send it to you. It's just a link. Once you have the link, you can get the translated version of it. I find that email is a wonderful thing to me. Really is. It keeps me in touch with the world, really.

04-01:37:53

Burnett:

Absolutely. Let's maybe return to Columbia. You said in graduate school, you did a master's degree, and you did a thesis. Is that right?

04-01:38:04

Leitmann:

It was called a project. It's the same here. You can take more courses and a project, or fewer courses and a thesis, and I chose the project route. It was on a topic called secondary electron emissions. It was going to be not theory, but a lab. I used, for my experiment, a small part of Enrico Fermi's lab in Pupin Hall, which was the physics building at Columbia, which I almost ruined, because I had a terrible time getting low enough pressure to make this work, this experiment.

04-01:38:59

Burnett:

So you had a vacuum?

04-01:39:01

Leitmann:

Yeah, I had a vacuum system. We used pumps to try to get low enough pressure. This has to do with electrons, or particles, bombarding, say, a metal surface, and then, as a secondary effect, electrons would be coming out of that. They used that sometimes for amplification, that kind of thing.

04-01:39:30

Burnett: Oh, like a vacuum tube?

04-01:39:31

Leitmann: Like a vacuum tube.

04-01:39:33

Burnett: A plate? A plate inside, and the electrons—gotcha.

04-01:39:35

Leitmann: A voltage difference, yeah. It's that kind of thing. It's still an open topic, by the way. I looked it up the other day, and it's amazing that it's, in a sense, an unsolved problem, because it's so complicated. We were warned about two things. First of all, in order to see whether there were leaks in the system, we put glass sections in these rubber tubes that we used for evacuating the system. Then you could hold a bottle with ether next to it, and if there was a glow discharge, it means that there was a bad seal. I knocked myself out with ether. They found me lying in the lab one day. That was one thing. The much worse thing was that you were warned never to let the pumps operate when you were not there. Not overnight, and certainly not on weekends. But I was desperate. I just wasn't getting a low enough pressure in the system, so I let them run. These were water-cooled pumps. The line broke, and when I got there Monday morning, it had flooded the lab below me, on the floor below me. So I spent a lot of time cleaning equipment that semester, and the damage was so bad that the president of the university came to inspect it. The president of the university was the future president of the United States. It was Eisenhower.

04-01:41:14

Burnett: He had been the general of the army that you were serving in.

04-01:41:15

Leitmann: General of the Army, that's right. He came to inspect. I think he must have said, "Give this guy his degree and get him out of here." [laughter]

04-01:41:27

Burnett: But it was a great experience, and you got a taste—so solid state physics was what you were interested in?

04-01:41:36

Leitmann: Yeah, at that time.

04-01:41:37

Burnett: At that time. One of the questions I had, and I don't think we've really explored this, because of the defense-related projects that were going on there, how do you end up in the next phase of your life? Is it through contacts at Columbia? How does that happen?

04-01:42:03

Leitmann:

I got my master's degree in 1950, the end of the academic year, May or June, whenever the semester—and I had to look for a job, of course. Somebody from Caltech, a professor from Caltech, who was also—at that time, JPL already existed.

04-01:42:31

Burnett:

The Jet Propulsion Laboratory?

04-01:42:31

Leitmann:

Yeah. Came to Columbia to recruit people for jobs. Caltech had something to do with it. Now, Caltech had two operations. They started JPL in the thirties, I guess, and then started what turned out to be United States Naval Ordnance Test Station in the Mojave Desert for the Navy. That was the counterpart to JPL. Then the Navy took it over completely during the war, and just as, I guess, the government took over JPL—it was associated with Caltech—it was no longer part of Caltech. That's how I got my first job, through that.

04-01:43:25

Burnett:

So there was a notice that was posted, and then you answered an ad, or there was a—

04-01:43:32

Leitmann:

No, there was a notice posted. He came to Columbia and gave a talk. You could come to the lecture hall. He'd answer questions. Then he had the paperwork if you wanted to apply, the usual kind of thing. That's what I did. I applied for this job. I had two reasons, maybe, for going to California, because I had an aunt in San Francisco, and another aunt in Los Angeles, and their families, of course. My mother's older sister was in Los Angeles, and my father's older sister, one of his sisters, was in San Francisco. I knew I was going to have family there. It made it much easier. It was difficult on my mother, because my grandmother, her mother, had died in 1947, so then she was going to be essentially alone. But she had a very good job, and was very involved.

04-01:44:43

Burnett:

She worked in a—was it in a jewelry—

04-01:44:46

Leitmann:

During the war, she got a job in a company that made artificial diamonds for machinery, for—

04-01:44:54

Burnett:

Drills and things like that.

04-01:44:55

Leitmann:

Yeah, this kind of thing. Owned by another refugee family. Their name was Finder. Then, after the war, he went into the jewelry business, and she went on and she became a designer. She didn't retire, actually, until 1969. Lived in the same apartment that we had when I was a kid in high school there, and was

still under rent control. I remember what she paid. In 1969, she paid seventy-two bucks for that apartment.

04-01:45:41

Burnett: Oh, my God.

04-01:45:42

Leitmann: Yeah. I still remember that.

04-01:45:51

Burnett: So you moved out? Where did you go?

04-01:45:55

Leitmann: I moved to the Mojave Desert. But my mother had a brother there. He died in the middle fifties, but at least when I left, my Uncle Paul, still lived in the next district. He lived in Elmhurst, and she lived in Jackson Heights. So it wasn't quite as bad as it would have been otherwise. I started at US Naval Ordnance Test Station, USNOTS, in the research department. They had a research department. They had all kinds of other departments. They were doing primarily the development of mostly rocket-operated armaments. There was some indication, once the Space Age started, that they were also involved, of course, since rocketry was sort of the main tool, but it started strictly as, again, just as JPL was, a military operation. JPL became part of the Air Force. Again, I really enjoyed myself there. I worked in—their main building was Michelson Laboratory, after the first US Nobel Prize winner, [Albert] Michelson. He was a physicist. I really went into more applied science and engineering, because their research had to do with applications. It wasn't research just for the sake of research there. I became interested in really what became what's called aeroballistics. In other words, the trajectories and the effect of perturbations—

04-01:48:20

Burnett: Wind speed, something like that?

04-01:48:21

Leitmann: Yeah, the effect of perturbations and the environment on the trajectories of rockets, which actually is quite a complicated subject. It's very highly nonlinear. You couldn't get along with the engineering for linear systems. These systems are not linear systems. That got me interested in that sort of subject. Then I got into optimization through that, how to make these better, more accurate, through control, because these rockets had either control through their nozzles or through their fins.

04-01:49:05

Burnett: Can we pause for a minute? Can you tell me about what you knew about rockets in World War II? When did you first hear about the programs the Germans had, for example?

- 04-01:49:21
Leitmann: Well, very early because the Germans were bombarding London with rockets. That was really my first exposure. In fact, it was first a V-1, which was a pulse jet. It was more like a—
- 04-01:49:37
Burnett: A putt-putt.
- 04-01:49:38
Leitmann: Yeah, a putt-putt. The few weeks I was stationed in Great Britain, I remember we had leave, and I went to London to a theater, the Royal Haymarket.
- 04-01:49:54
Burnett: Oh, yeah, you told me that story.
- 04-01:49:55
Leitmann: Yes, I remember that, now that you bring up rockets. You could always tell that it was getting dangerous, because when the putt-putt stopped, you knew it was going to come down. It was a Shakespeare play. I don't remember what play it was at the Royal Haymarket. We were sitting in the theater, and the putt-putt stopped, and about five minutes later, there was a big explosion right outside the theater.
- 04-01:50:25
Burnett: For probably five seconds.
- 04-01:50:27
Leitmann: Yeah, not five minutes. That was my first personal exposure to rockets. I was not exposed to the V-2s.
- 04-01:50:37
Burnett: November of '44, I think.
- 04-01:50:41
Leitmann: Yeah. There were a lot fewer. There were a lot more V-1s than V-2s. Maybe I touched on this, how fortunate we were, in fact—when I say we, the Allies—that Hitler was an anti-Semite, because certainly the physics of nuclear reaction and the whole thing was perfectly well-known, and German physicists were just as smart, and some were Jewish, some were half-Jewish. Hitler just forbade them to work on that, because it was Jewish science. He said it was that damn Jew that started all this. They had to concentrate on what he calls the weapons of—
- 04-01:51:40
Burnett: Retribution?
- 04-01:51:41
Leitmann: Retribution, retaliation, or something. *Vergeltungswaffen*. That's where the "V" came from. So they concentrated on making rockets at Peenemunde, which got to be their rocket station. I had mentioned, in fact, I guess that first book that came out of Europe in engineering, called *Ballistics of the Future* or

something like that, by two Dutch professors. They had a very nice appendix to illustrate this, with documents and the records of the Germans. They found all those records, of course. It's amazing how many of their own people they killed in this, because they had the control maybe two hundred feet from the rocket. They just sat there behind some sandbags with their instruments. These damn things were very unstable. When you don't have the stability due to the fins, because you have no speed, it had to be done with the nozzles. A gyroscopically controlled nozzle is a very complicated thing. I think 80, 90 percent of them just fell over at the beginning and took out the control room. So there weren't that many. Most of them were launched from the Netherlands. That was my first exposure to rockets.

04-01:53:19

Burnett:

I think almost as many people died making the rockets as were killed by them, in the V-2 program, I mean. It was an enormous investment. The V-1 and the V-2 programs, they spent 50% more on R&D and manufacturing than the US did on the Manhattan Project.¹

04-01:53:42

Leitmann:

I'm not aware of that.

04-01:53:43

Burnett:

Yeah, it was a massive investment, apparently. I want to confirm that independently, because I just read this. But it's an astonishing investment. But all this to say, the reason I bring that up, let's say it was a significant investment in research, and it did promise a kind of payoff, because there were no airmen who died. It was very costly to produce, but there was a promise of having this delivery vehicle with which you could attack people kind of at will. I think you said earlier in another session, they didn't focus on the payload. They were all worried about delivery. But they did deliver this expertise around ballistics, and as we know, many of those folks were rounded up by the United States, by CIC.

04-01:54:41

Leitmann:

Operation Paperclip was that, right. We were competing with the Russians.

04-01:54:47

Burnett:

Yeah, and they took their own chunk of people.

04-01:54:50

Leitmann:

They got some chunk of the German—yes. That was not really science; that was essentially engineering. So that's how I got into engineering, actually, from pure science. I think that, and where the jobs were. The exposure that I then had, of course, to rockets was through my job, and these were humongous rockets that they did at Redstone Arsenal, which was essentially

¹The Germans spent roughly 50% more on the combined V-1 and V-2 programs than the US did on the Manhattan Project. Frederick Ordway, III and Mitchel Sharpe. *The Rocket Team*. Apogee Books, 1979.

continuations of the German ones. I think the largest rocket that I had anything to do with was called Big Dick. That was the code name. It had a range of three, four hundred miles. The station, USNOTS, is probably one of the largest government-owned operations, even today, in terms of land. It's a huge piece of land in Southern California, the Mojave Desert. Even at that, on occasion, they landed off-station, up in the Sierra.

04-01:56:09

Burnett:

My God. These are a bunch of different projects that you were not involved in. They had a reservoir, a dam, basically a manmade lake, where they worked on submarine torpedoes, for example, because it is the Navy after all. Did they eventually develop the Polaris missile program? Later, after you're gone?

04-01:56:36

Leitmann:

I think that may have been later, submarine-launched rockets. Weapon Able, which I was involved with—called Weapon A—that was meant to be submarine-launched, and the idea was that, particularly if you launched it when the submarine was up, but underway, the effect of the wind was a perturbation. So we had a test track there, where I was in charge of, actually, even though I was, in theory, in charge of experiments there. We had a locomotive with a Buick engine on this track, and we pulled this rocket launcher. In fact, the book on—

04-01:57:26

Burnett:

*The Mavericks?*²

04-01:57:27

Leitmann:

The Mavericks, which is a book, in fact, on that—well, really, on that particular operation—has a picture of me standing on one of those rocket launchers, but no attribution. It just shows it as a picture. They didn't identify the little person that stood next to it. But I have the full picture, and I think maybe it's in your file. There was even an article in *Popular Science* on the same project, with that same picture. Of course, once the Space Age started, or the competition with the Russians, that got to be—but that wasn't until the middle fifties. Hey, that's when I was in China Lake, early fifties, so we actually overlapped with the first *Sputnik* launch and all that.

04-01:58:32

Burnett:

Yeah, I guess that's '57.

² Elizabeth Babcock, *Magnificent Mavericks: Transition of the Naval Ordnance Test Station from Rocket station to*

Research, Development, Test, and Evaluation Center, 1948-58 Washington, DC: Naval Air Systems Command, 2008.

04-01:58:35

Leitmann: I was there until '57, so it just overlapped. It was the initiation of the Computer Age. When I started on my, later on, PhD dissertation, I was still trying to use an analog computer. It wasn't adequate, but you know.

04-01:59:00

Burnett: A mechanical—digital—

04-01:59:01

Leitmann: It wasn't mechanical. It was an electronic analog.

04-01:59:03

Burnett: Differential analyzer, is that right?

04-01:59:05

Leitmann: Yeah. It really was a very exciting, fruitful age. Both the Space Age and the Computer Age sort of started at that time, so I was very fortunate, and I was fortunate because it also provided jobs and money, lots of money. I have a little article in a journal called *Space Flight*, I guess, British journal, called "George Leitmann: Rocket Dynamicist" or something like that. It was sort of autobiographical. All this reminded me. I have a footnote in there that I owe my education to Hitler and my job to Stalin. [laughter]

04-02:00:08

Burnett: Fair enough. That would make sense. It's true that the *Mavericks* book does talk about them bringing in a Goodyear electronic differential analyzer in 1951, a Reeves Electronic Analog Computer. Basically, it could do in twenty minutes what it would take a whole team a full day to do. Then, in '53—this is really early—you get a brand-new IBM 701. Is that what you were working on?

04-02:00:49

Leitmann: I think that's what I was using for my dissertation eventually.

04-02:00:52

Burnett: There were only nineteen of those in the world at that time, and it could do thirty-six million calculations in an hour.

04-02:00:59

Leitmann: Yeah, with some vacuum tubes.

04-02:01:02

Burnett: Yeah, absolutely a digital computer. Part of what's special in your situation is this access to processing power. Is that a feature? Is that something that made some of your research possible?

04-02:01:22

Leitmann: Actually, I didn't do, personally, a lot of computing. No. I really worked, most of the time, with equations. But I became interested in nonlinear systems, of course, because most of these systems could not be linearized, and you would lose a lot of information if you did that. Once I started going for a PhD, which

is another story, that became very important. That's interesting again, because somehow, unfortunately, from my point of view now, I rushed through things much too quickly, both at Columbia, because I had big gaps in my knowledge. If I had taken two, three years more, I would probably be much more proficient in mathematics, particularly. That had an effect later on, when we get to my academic life, as to what portion of my research was pure theory, and which application. I sort of always combined that by getting either graduate students or post-docs who were interested in application. That was a very nice way to work together. That worked out very well.

04-02:03:02

Burnett:

That's interesting. We should probably come back at some point to talk about this question around education and the best way to do it. I imagine, these days, it's impossible to get the kind of breadth and depth that you're talking about, because there's such a high degree of specialization and so much to know. I think you're saying, even for the time, you felt like you were kind of ill-prepared by not having a fluency or facility in other kinds of mathematics that might have helped you.

04-02:03:41

Leitmann:

Yeah, and more simply, because I always had a schedule, whether it was to get through school, or to go back to school, and all that interfered with taking more than necessary, which I regret, of course. But that's life.

04-02:04:06

Burnett:

There's only so much time.

04-02:04:10

Leitmann:

Are we sort of getting to the point where I return to school?

04-02:04:15

Burnett:

We are getting to the point where we return to school, but we should probably stop now, speaking of not enough time.

04-02:04:22

Leitmann:

Oh, yeah. Boy, time flies.

04-02:04:24

Burnett:

Yeah. We'll continue next time for session five.

04-02:04:28

Leitmann:

Terrific.

Interview 5: July 19, 2018

05-00:00:19

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History series. This is session five, and it's July 19, 2018. We are here in the Berkeley Hills. We left off session four talking a little bit about China Lake. You had just arrived. I just wanted to start with a point of curiosity. You're hired there as a civilian. Can you talk about the differences in culture between the civilian and the military supervision at China Lake? I'm assuming that almost everybody in the civilian side had been in the military, because you've just come out of World War II, and you've got all of these adults there who probably have had experience in the military. I'm wondering if you can talk a little bit about the culture at China Lake.

05-00:01:23

Leitmann:

Well, first of all, being essentially an offshoot of Caltech, just as JPL [Jet Propulsion Laboratory] was, there was already a very strong connection to civilians, and I guess it was really from [Theodore von] Karman, who was sort of the father of all these institutions. They were taken over fully by the military during the war, of course. The operation of Caltech, JPL, became part of the Air Force. China Lake became part of the Navy, sort of a counterpart. Not as prestigious, because it wasn't directly associated with a university. The influence of the military was really primarily administrative. I would think that 95 percent of the workers there, the personnel, were civilians. At least at China Lake they were. So there was usually an admiral who was the military chief of the station, and then a civilian technical director. They were essentially in rank of equal level. So the technical director really was the head of the civilian component. Just turns out that, when I went there—I didn't know that—the technical director was, in fact, the head of a family that my not-yet-wife, whom I hadn't met yet at that time, was a close friend to, and that's what brought her there, in fact, eventually. That's a little side remark.

Most of us lived on the base. The nearest town was Ridgecrest, which was essentially outside the base, down the street. It was a little Western town. I can't really think of anyone who lived off the base, although that was perfectly okay. There was housing for married couples, individual houses on the base. The singles lived in what really looked like motel units. They were fine. I had my own apartment in one of these units.

05-00:03:59

Burnett:

Air conditioning, I imagine?

05-00:04:00

Leitmann:

Yeah, everything was very nice. The interaction between—at least at my level—between the naval, mostly naval—some Air Force, because there were also other pilots there—was minimal, except that, very often, some of the technical personnel, the technicians, might have been Navy people. Of course, we obviously worked with them. There was, at least within my seven-year

experience, very little social intercourse between the military and the civilians. That was probably not entirely true at the high levels. I went there as, I think, a GS-7. I was a GS-12 when I left. I guess the highest rank was GS-15 at that time. I think there are now special ranks for political appointments. Certainly my own interaction was entirely with other civilians. Now, it isn't true that all of them, or many of them, were former veterans, because there were people quite a bit older than I was who were there, people in their forties, even fifties. One that comes up, will come up later, is Bernie Smith. Bernie Smith was probably in his early fifties when I met him first. Some of the senior people were older. The head of the division that I was attached to, the research division, was Dr. Ivar Highberg known as Oily Ivar, and he was probably in his middle, late forties.

I was probably one of the young and very low-level persons there. That is really why, in answer to your question about the rocket program, I didn't know any more than any other person who wanted to be informed on that from the newspapers and whatever. We all knew, by then, that the United States was embarked on a rocket program. Although that didn't really become paramount, or even highly known, until the beginning of the Space Age. By the way, the Navy was much less involved than the Air Force, and also less successful. You may remember there were a lot of mis-launches by high-altitude rockets, and that may have eventually interested me in looking at that problem when I started on my dissertation. They had a lot of them fall over. That was true, of course, also for the big rockets, except the Germans had already worked on that, by that time, for almost ten years, and their rockets fell—90 percent fell over and killed the launching crews. But they had overcome that problem, largely. Redstone Arsenal, for example, didn't have many accidents.

05-00:07:34

Burnett:

But that overcoming was an engineering, almost—I don't want to put words in your mouth, but it wasn't a theoretical solution to the tremendous fail rate of the rockets? They had kind of—not cut and try, but they had walked their way to an optimum? Is that a fair thing to say?

05-00:08:03

Leitmann:

Well, at least they were moving in that direction. It's true that, just as in the Manhattan Project, the final solution was strictly an engineering one. The physics was well-known by us, by the Germans, by everybody else. There were some little things, relatively small physics and chemistry new things going on, but the rest of it was strictly engineering. The Manhattan Project, it was the idea, how do you detonate a bomb? I mentioned before, previously, the Germans didn't have a clue. Fortunately.

05-00:08:47

Burnett:

It's interesting you should say that, because I think the Naval Ordnance Test Station was originally supported by Manhattan Project funds to develop the explosives that would achieve criticality, that that was the original purpose.

05-00:09:01

Leitmann:

I didn't know that. Found that out, I guess, when you started reading the history of—which is only now six months ago that we even knew about that. No, I started looking, mostly thumbing through, the book on the section of my tenure there, so '50 onward. The previous history was completely unknown to me. There was no attempt even to tell us about the previous history.

05-00:09:28

Burnett:

“Need to know,” right?

05-00:09:31

Leitmann:

There wasn't a brochure saying, “We started in the thirties, and blah, blah, blah.” The place where I worked was a laboratory named after Michelson, who was the first US Nobel Laureate, in the early 1900s. It had a number of divisions, and I happened to be put in the research division—research department, I guess—division, yeah. There was chemistry, and different engineering divisions. I don't know why I got put in the research [division], but that's where they were looking for people, I guess. It's that simple.

05-00:10:27

Burnett:

You were recruited, so they knew what level of mathematics you were proficient at?

05-00:10:32

Leitmann:

Yeah. You had to fill out a lot of forms. They came to places like Columbia, and probably, obviously Caltech, and other places, looking for people. Whether they saw any great need, you've got to remember, this is just prior to the war in Korea. Whether there was some idea that there was a need for things that eventually were developed there, I suppose at very high levels, this was probably already in the plans, because the sort of routine weaponry that was already well-known—and it was, in fact, considered there, because it was called the Naval Ordnance Test Station. It didn't say “development” or anything like that. It sort of grew into development from really just a place where there was testing and improvement, obviously, modernization, and new ideas brought in, but certainly not as a research station. That came about just about the time I went there, that it became a research station.

05-00:11:54

Burnett:

That seems to be right. The Bureau of Ordnance—right around the time you're there, maybe a year after, possibly—made China Lake responsible for the technical direction of all rocket research and development for the Navy. So that's where it's happening.

05-00:12:10

Leitmann:

Yeah, and so that's probably why they were recruiting people, I suspect.

05-00:12:13

Burnett:

That would make sense.

05-00:12:15

Leitmann:

And in different areas, ranging from propulsion chemistry, that kind of thing, all the way to control and optimization, which wasn't really called that at the time, but what you might call systems theory. It really dates back many years, that whole area, because, even in World War I, that was an area of great interest. Norbert Wiener, for example—certainly a well-known person—he was, in fact, an officer, a second lieutenant, in the Army Ordnance Corps during the First World War, to work on artillery: aiming and making artillery more precise. That's a field that was certainly considered by the military already in the First World War. This is really a continuation of that, as wars come and go, as they do a lot these days. I was at a level where none of that kind of thing was even considered by me, or even made known to me, or even mentioned, because that's not what they hired me for. That's true in other fields I was in, like the CIC [Counterintelligence Corps]: very little of giving you background and introducing you to the field and the requirements in a concrete way. You sort of grew into it by osmosis. I think the same thing was also true in China Lake. They didn't say, "Now, that's what I want you to work on." Once in a while, they would say, "We need to work on making trajectories more precise," and that sort of was the general area that I really sort of glided into. I went in at the end of 1950, and I think by the beginning of '51, then the next couple of years, that became the area that I slid into. The section that I got into was called aeroballistics, and that's really what that is. There's the interior ballistics, which is the propulsion kind of thing, and then the exterior, which is really the trajectory determination, programming, optimizing, and so on.

05-00:15:03

Burnett:

That's an important distinction, because until this period, military forces had been interested in trajectory of projectiles for a very, very long time. That was the big science in military history: when things go up in the air, and how do they come down.

05-00:15:24

Leitmann:

That was essentially what Norbert Wiener worked on.

05-00:15:26

Burnett:

Right. But these are now—we can call them rockets, but to be precise, they're self-propelled ballistic devices. They carry within them the propulsive force.

05-00:15:40

Leitmann:

Yeah, and that can be then used in a way to influence the trajectory.

05-00:15:45

Burnett:

Right, and that has enormous consequences. And I don't think this is a stretch to say that it's more complicated than an artillery shell, for example.

05-00:16:00

Leitmann:

Yeah, it's true, certainly. You have more to work with, and so to influence things towards the aim of that piece of equipment, you have more

possibilities, and therefore more ways of exploring and developing. That's certainly true. There's also the whole idea that these are much longer-range, sometimes—sometimes not—so that you have considerations of, for example, using the propulsive device in a more economical way, so as to maybe use less fuel, and then have the possibility of a larger payload. That's why the V-2 was really, in a sense, a failure, because here was this huge vehicle, with an enormous fuel supply, and the payload, which was just a small percentage of the apparatus that took the vehicle there. Then it becomes important to say, what can you do to increase the payload vis-a-vis the initial weight, which is essentially the fuel, a propellant? So that brings in another area in planning, and therefore the associated mathematics.

05-00:17:31
Burnett:

In the theoretical division, you were—I don't want to get ahead to your dissertation, necessarily, but you are working with the intellectual heritage of Robert Goddard, who sets up the basic conceptual apparatus for the rocket. He's got the ideal rocket. Is it at that level of abstraction where you're trying to take that idealized rocket and bring it to earth, so to speak?

05-00:18:03
Leitmann:

Yeah, into a better model for the actual device, certainly. Certainly. Goddard's—we're not really getting ahead of ourselves, because he had already written a paper on this in 1919. He started working on rockets before the First World War, as a very young man. He was an engineer; he was not a mathematician. I'm certainly not, and never was, but I had a little bit more mathematics than Goddard. When he wrote his paper he used a simplified model. Very interesting, because it's sort of a rocket which has infinitely many stages. The stages you want to get rid of, because it's this dead load to propel, and his model, sort of the stages went off—

05-00:18:59
Burnett:

In a smooth line.

05-00:19:00
Leitmann:

The case went off at the same time the propellant went—which is a cute idea. He did realize that there was a compromise between two things that kept you from—in the case, say, of a high-altitude rocket that you fire vertically—from doing it the best way. There was the aerodynamic drag, the forces that slow you down, on the one hand. Another force that slows you down is just gravity. So the question is, there's nothing you can do about this, but how can you arrange the trajectory so as to get the best compromise? In other words, two bad things happen, and then you want to go to the less bad one in some way when you program things. He realized that, and he actually tried to do it without classical mathematics, by arguing that you divide the trajectory into short time periods, and during each period, you assume you have constant acceleration. Then the question is, how do you divide up these things for every stage, in other words, change the acceleration, which would be the propulsive part of the system, to best divide the two bad effects? Because if

you go too fast, which is very good vis-a-vis gravity—you get out of the gravitational field, because it gets smaller and smaller, and so you don't sit in very heavy gravity — but then you go too fast, and the aerodynamic drag goes up. That kind of compromise is really sort of the basic idea of optimization in that particular problem. There are comparable problems in other cases, where you have to—you can go even to economic problems, where you want to make a lot of money, and on the other hand, not pollute too much, because that costs you. There are these compromises in almost everything we do, if we think about them.

05-00:21:33

Burnett:

So benefit-cost analysis can be part of that. In the history of economics, a lot of those key figures, a lot of them are inspired by John von Neumann. So there is a kind of lineage coming from operations research, ballistic research programs, filtering out.

05-00:21:55

Leitmann:

Yeah, these are very related in terms of the basic philosophy. The criteria are different. So that's probably not jumping ahead. I became interested later on in other things than aeroballistics. I began to work on this really early in 1951, because the first couple of months was just getting used to the place. There were other people, some about my age — I'm still in touch with some of them, particularly at Christmas — and others who had been working on that kind of thing much longer, and from different points of view, like Bernie Smith. He wasn't interested a lot in the mathematics, but he was interested in the problem, and however way you wanted to attack it, that was fine with him, but that wasn't his primary interest, and other people like that. My direct supervisor was a man by the name of Dr. Newkirk, and he was a good mathematician. He was more interested in allowing me to work on that kind of stuff.

05-00:23:31

Burnett:

Just to clarify, when you started, there was, I imagine, a meeting, or a set of meetings, where there was a kind of orientation —

05-00:23:45

Leitmann:

There was real little orientation. That's what I tried to tell you. Just like in CIC. I never had any orientation. I sort of got plopped in there and sort of the—

05-00:23:55

Burnett:

And they said, "Go."

05-00:23:55

Leitmann:

The established people there gave me the orientation, and I think that pretty much happened there, too. It's interesting how little planned orientation — maybe it's better now — I don't know, I suppose it is — happens.

05-00:24:16

Burnett:

There were people who were already there, and I imagine it's informal conversation about, "What are you working on? What needs to be done?" Or did they also say, "Here's what we're working on, but if you have ideas, you can go in that direction?"

05-00:24:34

Leitmann:

Yeah. There were, for example, concerns about precision, the effect of perturbations on what you're trying to do. Clearly, with rockets, you want to aim them in such a way that they hit the target more and more closely. The same with artillery, except, in the case of rockets, you can do things as you go along. With artillery, there you are, and it goes where it goes. That's certainly true. But it's the same idea, that you want to do things that improve performance, in our case, come more closely to a target, or to carry a payload to some other mission, as in the space program. All that, in the end, fits together. You could do that in optimization, which was, essentially, when I got there, still the calculus of variations, there was no Pontryagin maximum principle in existence yet. Maybe people had thought about it, but it hadn't been published or anything. Maybe use the theory, then, to say, no matter what you want to minimize, which is a function of certain things—some of which you can manipulate, others, they're part of the physics, the chemistry of a problem. Then you say, well, in my particular case, that thing I want to minimize or maximize—maybe minimize fuel consumption, or maximize a payload, or a combination of things. But the general theory would allow you to say, I have so many variables that I want to influence, and then, depending on the class and what kind of variables they are, the theory fits in the sense that one of those variables, or number of variables, are what you're interested in. The more general you can make it—because very often, at the beginning, you don't know all the things you want to do—that made it useful for someone to look at problems in a more general way of saying, I have one particular system, and I want to do that. Anyway, in the research division, you could do that. I think, in the more direct engineering divisions, of course, you were stuck with what you got.

05-00:27:13

Burnett:

That's a question, then. When you're talking about working out a theory, you're talking about making decisions about what variables you want to focus on in your formula, developing the formula, and plugging in some numbers, and seeing if it works? There's no experimental relation, in other words?

05-00:27:36

Leitmann:

No, but there is a theory that says, if you do this kind of thing, then you will minimize, or maximize, the variables, the end values over time, so that you can cover a large class. You can also say what kinds of physics or physical objects can you model for which you can develop a theory, and that's sometimes very restrictive, sometimes overlooked. Right at the beginning, I ran into this, basically a semantic problem. The classical calculus of variations, which is the basic tool that had been in development for 200 years,

allows you to look at things in a very abstract way, not necessarily demanding that you get a solution, but simply saying, here are the conditions. If you can solve that problem, you've got the solution. The kinds of problems that you run into aren't even thought of by the time you develop something. Then, suddenly, you'll say, I can't do that, because you can't vary that particular variable in a real system, or it's restricted. How much you can do, how big can you make it, how smooth does it have to be? All that isn't really in the original model, in classical calculus of variations, and much of that came out of the University of Chicago math department in the thirties, and a man by the name of [Gilbert Ames] Bliss, and allowed us later on, to treat problems, particularly problems in which what you can do is constrained, much better than before.

Eventually, when the Russians got into it, which they did, in the late forties, they came up with something, which became an iconic thing. There was a mathematician by the name of [Lev] Pontryagin, and he and his graduate students worked up what really was something that had already been considered in the United States and other places, certainly, ten, fifteen years earlier, but not put into the kind of language that is useful in engineering problems. It was really, in that period, in the late forties, early fifties, when I got into it, that people began to realize that that way of putting the problem is more useful to engineering thinking, although the problem in mathematics had already been posed, and to some extent solved. That's sort of the continuous struggle, saying, "who did it first." Well, it depends on who did—

05-00:31:09

Burnett:

When you say "putting the problem," they framed the mathematics in such a way that it could be both understood and applied and accessed by engineers?

05-00:31:27

Leitmann:

It's framed in such a way that the engineer will understand better what you're trying to do. I think that, later on, my dissertation—I was looking at it this morning when I found a misprint on page 19—I dealt with two models, the simpler and the more complex model. The more complex model, to this day, would be more difficult to solve than the simpler one, simply for mathematical reasons, because the number of variables, and in terms of what classes of variables they are, was much larger, sufficiently larger, in the more general model than in the more restricted one. The idea that allowed one to use additional conditions in mathematics became much more useful. Maybe there's somebody, whoever listens to this, will see that, in the classical calculus of variations, and in almost every field, there are both necessary and sufficient conditions for what you are trying to achieve. Usually, the necessary conditions people look at first, because they say, if I tell you whether your system satisfies necessary conditions or not, you immediately have one answer. If they don't, then it can't be best. So you can already rule out whole classes of problems or strategies or whatever. The sufficient conditions say, if you satisfy those conditions, you'll definitely have the best, and then it's the

question of what does “best” mean in a particular problem. So people are constrained, and certainly, as engineers, start to look at the necessary conditions.

Now, the necessary conditions don’t say the answer that you get by solving these conditions will be optimal. But for some reason, the question of optimality took over, and people, for years, and even to this day, will say, “The optimal solution is.” But what they’re really saying is you have found candidates for optimal solutions. I fell into that trap for about a year or two, and then it occurred to me, what do I mean by optimal? What we meant in the beginning was that these were conditions that were necessary to be optimal. I’ve been chided by some colleagues over the years, saying, “Ah, don’t worry about that. If it isn’t optimal from those conditions, it’s a special case.” Stuff like that. People like to claim things for what they are interested in, or able to do, or successful in, sometimes a little bit more in too broad a way, and I tried to be very careful about that later on.

05-00:34:53

Burnett:

Are you suggesting that some scientists, in different fields, perhaps, confuse optimality with desirability?

05-00:35:04

Leitmann:

No, not desirability. No, no. I make a distinction between what I call an optimal strategy and an extremal strategy. An extremal strategy — you can use other terms, of course — means that these are solutions that satisfy the conditions from which your optimal solutions spring. In other words, if something is optimal, it will have to be a member of that set that satisfies your conditions. But it doesn’t mean it is optimal. So you will get a much larger basket to take strategies from, because there are, in many cases, many more solutions that are extremal, but not optimal. Particularly among more engineering-directed applied mathematicians, there is that desire not to be diverted, or saying, “Well, those are special cases. Every one of those is either an optimal solution, or if it isn’t, there’s something crazy about it.” I used to have discussions with even friends of mine, some at Stanford. Those people, they sort of feel that I’m pedantic. I’m willing to admit to that.

Actually, what I’m really saying is that I want to warn my readers about what I’m telling them, that this may not be optimal. If you’re smart, you will advance the theory and come up with more precise ideas. I did sometimes; other people did. Sometimes people proved, in fact, that what is only extremal is, in fact, not optimal. Or, in fact, even the other way around. There are cases in control theory where you come up with extremal solutions, and they are, in fact, optimal if everything is exactly precise. But if you make the smallest perturbation, what was an optimum is now suddenly a pessimum. I have examples in my book — not my own example, but I quoted — of a system in which, if you have exact ways of applying the strategy — and this is, say, minimizing the time to get somewhere under certain conditions — it will be

optimal, because it will truly minimize the time for me to get from here to there. But if you make the slightest mistake — for example, have a small delay in your strategy — it now takes an infinite time to get there. You'll never get there.

05-00:38:29

Burnett:

You missed your connection.

05-00:38:30

Leitmann:

Well, it begins to go back and forth, and never get away from that point. If you go along, and now you change your strategy and you change it, and change it properly, you will get to that place in minimum time. But if you get here and now change the strategy, but you have somehow delayed — no matter how little time — you will suddenly have to go back and forth at that point, just to satisfy the necessary conditions. Now you'll never get there. That happens.

05-00:39:02

Burnett:

Presumably, the more conditions you have to satisfy to reach an optimum, the greater the chance—

05-00:39:08

Leitmann:

The possibility. Yeah, and it has to do with stability, stability meaning how much you can rely on what you're doing, in a mathematical sense. Those problems that have that property of suddenly making the best the worst are unstable in a certain functional sense, because a very small perturbation really screws you up. So you have to build that into the possibilities.

05-00:39:44

Burnett:

That sounds so particularly appropriate for rocket research, because you can imagine—or not imagine. We have all of these cases of the incredible complexity of getting a propulsive or propelled projectile to go where it's supposed to go, in exactly the right way. There are so many different things that can go wrong at each point of its trajectory. From my limited knowledge of space programs and things like that, there was this tremendous effort to build in redundancy around your choices, so that you minimize—even if you were to have these kinds of perturbations, there would be some kind of counteracting thing that would preserve stability.

05-00:40:43

Leitmann:

That, in turn, points up the importance of being able to sense, to measure what is happening, because if you suddenly realize this is happening, you can now do something else. You don't stick with it. On the other hand, it also points up, in the example I gave, usually engineers think that if you have what they call a feedback strategy — in other words, you base your strategy on observing the environment, and as the environment changes, you change the strategy leading to the best solution. But that's, very often, exactly that kind of system that is unstable. Because what happens in that simple example that I gave you is you realize where you are in function space. In other words, in the

space of position and velocity. You know where you are precisely, but if you don't know it precisely, you'll be doing the wrong thing. Measuring, in a good way, a sensible way, becomes very, very important. So the whole idea of not just doing the best, but doing it with the best information, and not assuming too much of your instruments, for example, because knowing where you are and how fast you're going depends on how you measure those things.

05-00:42:26

Burnett:

And how fast you can measure them.

05-00:42:29

Leitmann:

That's right. If you don't measure them fast enough, you're going into a wrong place, and then the strategy should change, but you don't know it. That's exactly what happens in that example.

05-00:42:43

Burnett:

My goodness. I have a question about where your sources are coming from, and how you learn about Bliss at Chicago, for example. Is there a library on the base? Or are you going back and forth to Caltech? How does knowledge circulate, in other words?

05-00:43:14

Leitmann:

Actually, the way you know what to look for and where to look is, when you read a paper and the references. The references, very often, have a lot of references themselves. That's really one of the best ways. Now, of course, with the internet, it's very easy to look. Relatively easy, certainly, compared to what was available to me sixty years ago.

05-00:43:43

Burnett:

How big was the literature in rocket research at that time? You joined two of the major societies in 1950. When you start, you join the British Interplanetary Society, which is a wonderful name, and the American Rocket Society.

05-00:44:23

Leitmann:

Which became, now, the AIAA, which is the American Institute for Aeronautics and Astronautics. It changed from Rocket Society to its current name in the early sixties.

05-00:44:14

Burnett:

They hadn't been around for too, too long as societies. I think the twenties, they start.

05-00:44:20

Leitmann:

Something like that. The British society was not as prestigious as the American Rocket Society, but it was one of the few places where you could publish, and where you could read. The number of journals was much, much smaller, and much of the information, even in those days, for example, was restricted, confidential, secret. What had actually been already found may be in a secret document.

05-00:45:02

Burnett: So you didn't have access to the—

05-00:45:05

Leitmann: Oh, no, I had Q clearance, which is higher than secret. But unless you can browse, so to speak, through the literature, you don't really get the greatest number of new sources.

05-00:45:25

Burnett: It says in the book that we were referencing, the *Mavericks* book [Elizabeth Babcock, *Magnificent Mavericks: Transition of the Naval Ordnance Testing Station from Rocket Station to Research, Development, Testing, and Evaluation Center, 1948-58*, Washington, D.C.: Naval Air Systems Command, 2008], that Peenemünde expertise was brought into the Naval Ordnance Testing Station by Levering Smith.

05-00:45:36

Leitmann: Yeah.

05-00:45:38

Burnett: So it was there, but I imagine who got to know those things or look at those things —

05-00:45:43

Leitmann: These are people who, first of all, had access to confidential or secret information. Then they could bring it to China Lake, simply because, again, everybody there could work on that stuff was cleared to do it. But it wasn't in the open literature. That's the point. Very often, people published in the open literature by hiding. In other words, making it look more like mathematics than engineering, or not taking examples that might be of a restricted nature. In fact, one of the reasons I once got into trouble with this was — I should have never had clearance. I never worked on nuclear things, per se. Somehow, I think one of my very first papers, in fact — I gave it at a meeting in London — was something about a nuclear rocket, but it really had nothing to do with nuclear energy or anything like that. It's just that nuclear rockets have certain constraints. It's very simple. There are two kinds of constraints that are major. One of them is thrust. You have an upper limit on how big the thrust is that you can achieve in the system you're interested—

05-00:47:14

Burnett: Otherwise it blows apart, for example?

05-00:47:16

Leitmann: It's simply restricted by the physics that you know at the time, the engineering that you know at the time, more so than it blows up. The point is that you can just get so much thrust out of a rocket, and they're constantly, of course, working on getting higher thrusts. But for rockets like nuclear power, rockets that get their energy from that source, rather than explosions, they are power-limited. The rate of work these can do is limited, rather than their thrust magnitude. I wrote this little paper — really, I should have called power-

limited rocket. But since that looked more glamorous to me, I guess — this is before I started going into academia. Because during that time, I couldn't publish very much, because almost all the stuff that we did there was classified. So I thought maybe I'd sort of do that, and that got me into trouble later on, because they assumed that I should really have Q clearance. It wouldn't have made any difference whether I had it or not, because I wasn't working on the nuclear part of the rocket, so to speak. It was the exterior ballistics. That by itself, I think, is interesting, and people are often plagued by that. You will see, in the literature, people who are very interested in applications to a particular problem, and then they will do research and work on that and publish on that, and people who want to work on classes of problems, and then when they give examples, there are mere examples. They can't get you into trouble so much. It's very interesting.

When the Russians started working on this, I had a talk. I got to know — well, Pontryagin only socially, but three of his coworkers. Two of them I got to be good friends with. I once asked one, I said, "All this — the mathematics, basically — has already been done." By that time, I was told, particularly by mathematicians, "There's nothing new, what the Russians are doing." The answer was, "We tried to look at what Bliss was doing in Chicago, and just didn't understand what he was doing." Well, it was bullshit, because even I could understand what he was doing. So there was the excuse. The reason they finally did work on it is because the Soviet government was very interested in that topic of optimization, mostly for military reasons, obviously. They were told, "If you show real progress in this field, you'll get the Lenin Prize." That really pushed them. So they didn't really want to look too closely at what had been done before. And the way they put the problem was very helpful. Now we had already done it, two or three of us, in the States, but not exactly the same way. What they call control variables, which are variables in the differential equations that don't appear with their derivatives. You can choose them. Because the system is a description of the dynamics, so you get differential equations. Then there are variables we used to call decision variables, or strategy variables. Different people that I worked with and knew used different names, but that's what the Russians eventually called them. The engineers loved that, "control variables."

05-00:51:16

Burnett:

So it's the same thing?

05-00:51:18

Leitmann:

It's exactly the same thing. You can, however, get more [with the Russian system], because the major overall condition for optimality is called the Pontryagin maximum principle. So there is a function that involves the ordinary variables of the system and the control variables, and then a certain function of those variables has to be maximized to be optimal, namely the control variables. So at every point, you have these variables that have to belong to some constrained set. You can't have infinite thrust and all that.

Then you can say, okay, now that function, called the H function, has to be maximized at every point of the system with respect to those control variables. One of the most classical conditions that says that is called the Weierstrass condition, which was already known in the nineteenth century. So that H function is the E function in the classical theory, and in that case — again, nobody talked about control variables, but the derivative of those functions, that don't appear in the equations, are what have to be maximized in the Weierstrass — so it's exactly the same condition. When you write it down in modern language, it's the maximum principle. Then there are things that the Russians have done in addition, and other people of course, but basically, there is no difference between that nineteenth-century condition and Pontryagin's maximum principle, which employs the H function. So, it's the old question "Who did what first?" in a sense.

05-00:53:26

Burnett:

You mentioned the Russians got going on this presumably later than you did.

05-00:53:33

Leitmann:

No, no. The first mention of Pontryagin's maximum principle was later, yes, later than when I looked at it, but in terms of the decision variables, I already had — and so did other people — papers in the late forties.

05-00:54:06

Burnett:

The Russians have, obviously, their own rocket program. Now, were Katyushas used in World War II? The Katyusha rockets?

05-00:54:15

Leitmann:

Yeah.

05-00:54:16

Burnett:

So they would use them in the bombardments of the cities?

05-00:54:19

Leitmann:

Yeah.

05-00:54:19

Burnett:

They were wildly inaccurate, but they were—

05-00:0054:21

Leitmann:

Well, there were big batteries of them.

05-00:54:23

Burnett:

They were massed rocket barrages. So they had a rocket program, and they also took folks from Germany and brought them into their own research program.

05-00:54:42

Leitmann:

When you say from Germany, I don't know. Certainly, by the middle thirties, *Peenemünde was in operation, in '34, '35. By that time, there was a strict separation between Nazi Germany and Stalin's Russia, until they got together*

for a little while. So I don't think there was much exchange. What I guess is sort of interesting is — because I asked that this particular man I talked about — name is Gamkrelidze— he was one of the four authors of the book that Russians first came up with in 1960 or '61, something like that. I said—his first name was Revaz—I said, “Revaz, what made you work on this problem?” “Oh,” he said, “because we tried to find out”—and then he winked at me—“we wanted to find out how to use as little fuel as possible in our airliners.” Aeroflot was really the outfit. Aeroflot couldn't care less. It was, of course, the military, but he said, “That was really the reason we went into this work.”

05-00:55:55

Burnett:

Wink, wink. [Laughter] So you are drawing on the research of G.A. Bliss at Chicago, and you're beginning to explore these questions of the initial mass of the rocket. So these questions come to you before you get the idea to go to school, is that right? These are problems that you're working on—

05-00:56:26

Leitmann:

I was already working. In fact, the reason I did a fairly fast job on my dissertation was that, first of all, I already had picked the topic, which is sometimes a long process for people, and secondly, I had started to work on it. So that helped a lot, no question about it. You had to register for two years in a doctoral program. That was the minimum time. So you had to be a student for four semesters. And I had a master's degree, but that was in physics. It usually, in those days, took about five, six years. It took me two years. In fact, only one year on campus, because the second year I spent back on the job. That wouldn't have been possible without a little bit of initiation, I guess maybe you might call it.

05-00:57:28

Burnett:

Is this the right time to talk about that opportunity? How did that come about?

05-00:57:23

Leitmann:

Well, let's see. I had been at China Lake—'53, I think, early '53. The Office of Naval Research, which was essentially our mother outfit, had established this very generous fellowship for people to go back, only for their doctorates. It was only a doctoral program. Then, as a curious side story, an anecdote, I thought I would apply for this fellowship. So I talked to Ivar Highberg, the head of the research department—Oily Ivar—and I said, “I'm thinking about applying for this. What do you think about it?” He said, “Let me ask you something.” He said, “Here I am, Dr. Ivar Highberg, and my toilet breaks down. What do I do? I pick up the phone, I call the Navy mechanical people that fix the toilets and everything. I say, ‘This is Dr. Highberg. My toilet’—five minutes later, they're there fixing my toilet. Now, suppose your toilet breaks down, and you say, ‘This is George Leitmann.’ He says it will take five weeks before they fix your toilet.” So my impetus, my reason for getting a doctorate, was really to have my toilet fixed quickly. [Laughter] I found out later on this is actually not such a trivial consideration.

05-00:59:24

Burnett:

It helps. I think status in society is not unimportant.

05-00:59:28

Leitmann:

Absolutely. Absolutely. I think it's unfair. It's essentially irrelevant. But it works that way. This is how I got into saying, "Okay, I'll apply." I got the second fellowship, and now I had to look around, where do I go to school? I think it must have been either late '53 or early '54 that I actually got the fellowship. The question was to start school, then, in the fall of '54. In those days, life was—maybe it still is in some places—much simpler. All you had to do to get into most schools — certainly in Europe—was to have a professor who said, "Yes, I want this guy—or girl—as a PhD student." That's all it took. I found that the most renowned, at that time, aeroballistician was a man by the name of Raymond Sängner who was a professor at ETH in Zurich. By that time, I had written two or three papers. I had written the papers. I don't think they came out until '56 or so, but at least I could send manuscripts, because this was early '54. He accepted me. This was for the fall semester of '54. I sold my car, I sold my hi-fi.

Nancy and I were almost engaged. That's another story, how I got to Nancy, the lady to whom I've been married sixty-three-plus years. We were going out together. Let's put it this way. And suddenly, I had this fantastic offer to go to Switzerland, and she was sure that she wasn't going to ever see me again. So she gave up her job and took a job in San Francisco. Downstairs, there's one of those steamer trunks, old trunks, that still has the label on it, "US Consulate Zurich" on it. I was going to go by boat, by the way. It must have been the first week in August, I got two visitors from the Office of Naval Intelligence, fellow special agents. I was Army, and they were Navy. But anyway, they said, "We understand that you are planning to go to school in Zurich." I said yes. They said, "Well, I'm afraid you can't. Switzerland is a nest of spies." Remember, this was the height of the McCarthy era. "You can go to Germany. You can go to Great Britain. You can go to Italy. Go to France. If you try to go to Switzerland, we'll just take your passport away."

05-01:03:01

Burnett:

To Switzerland?

05-01:03:02

Leitmann:

Yeah. So I was really screwed, because where am I going to get in? School was going to start two weeks later. I think in Zurich, it was the middle of September. There were two people who were consultants at China Lake. One of them I had known already for some time before that, quite closely, who was a professor in the math department, Steve Diliberto. The other one I didn't know so well, and that was a younger professor in mechanical engineering at Berkeley, Werner Goldsmith. We were sitting at the swimming pool that weekend, and I told them my sad story. First of all, Diliberto, who had become very interested in my work — in fact, published a couple of papers on this — he said, "You will have my support if you want to" — I couldn't get

into the math department, of course. I wasn't a mathematician. Werner Goldsmith said, "If you work with me, I'll get you in." I was only his second student, so he was very eager to get a graduate student who already, essentially, was established. I don't think he really had a clue about what I was going to do. Really. He worked and became famous in collision problems, sort of the father of collision mechanics at Berkeley. And that's how I got in. In those days, it was possible to do that. The deadline for applying at Berkeley was long gone. It was like April of '54. April 15, I think, is when you had to have it in, and here it was August. I got in. I think that wouldn't be possible today, because there's an admission committee and all that. So that's how I came to Berkeley. That's how I came back to Nancy, because she was already in San Francisco, and two weeks later, here I was in Berkeley. It worked out.

05-01:05:18

Burnett:

Had you guys broken up?

05-01:05:20

Leitmann:

No, no, no. Somehow, I was going to be gone two, three years in Zurich. Anyway, she was pessimistic about it. I assured her that I wasn't going to forget her. It only took two weeks to get together again. So that was very strange.

05-01:05:47

Burnett:

Let's just follow that through. How did you meet? It was through someone you were working with?

05-01:05:53

Leitmann:

No. Nancy had very close friends when she was in Cambridge—Boston. That Cambridge. She did her graduate work at Boston University, because she couldn't get into Harvard. She's still PO-ed about that. Radcliffe, I guess. She lived in a co-op house in Cambridge, and a couple also lived there. They became very close friends. The Bothwells. It turned out that he [Frank Bothwell] became the technical director at the USNOTs and he got his PhD at MIT, with Norbert Wiener, by the way—small world. They were very close friends, and she had finished her graduate work, and she had just come back from France, where she was teaching at a *lycée* for a year. She said, "I'm going to take a trip through the United States," with one of her friends, close friends, and end up with the Bothwells as their house guest for a while in California. Then she liked it so much here, she decided she's going to get a job on the base and stay, because the technical director had a very big house, five bedrooms and all that. Although they had six kids. She was essentially more permanent than I was when I got here. But she didn't come to California until about '52. That visit to the Bothwells started in '52. I got to know her through the Bothwells. But I didn't really get to know her well through the Bothwells. She was always interested in the theater, and she had become a cast member in a small theater company called the China Lake Players. One Sunday afternoon, I went to the China Lake Players. I knew that she was in

the play. It was a Noel Coward play called *Blithe Spirit*, and she played the second wife. So then, when I saw her there, I thought I'd become more serious about the relationship. That's how we got together.

05-01:08:25

Burnett: You were hooked by the dramatic—

05-01:08:28

Leitmann: Well, actually, I'll be a bad boy. I said, now that I've seen her onstage, I'll check her out at the swimming pool. [Laughter] I have to admit that. Then we were in a Russian class together that same year. UCLA had an extension course in Russian at China Lake, and she sat in front of me.

05-01:08:56

Burnett: That's wonderful. I think a pattern in how you narrate things is that it's very matter-of-fact. I'm trying to appreciate, again, how extraordinary the circumstances are. You are in rocket research, on a secret military base in the middle of the desert in California, at a time when this is the most high-stakes end of the military apparatus at the time. This is the cutting edge. They figured out how to build a bomb. They're now working on—

05-01:09:48

Leitmann: On a delivery system.

05-01:09:49

Burnett: How to deliver it. That becomes the story of the Cold War. I'm sure you didn't know it at the time, because there was no Sputnik yet. There had been no—

05-01:10:03

Leitmann: This was prior to the space program being a major driving force, of course, eventually.

05-01:10:11

Burnett: But this wasn't just a job, was it?

05-01:10:15

Leitmann: Well, I like my work, certainly. There's no question about it. It was exciting, first of all, and then I loved being in California, and then, of course, I met Nancy. It was really after we had got together closely that the Zurich thing came about. That's when she thought she wouldn't see me again. We had already been close enough by '54. She came in '52. I met her maybe in early '53. In '54, I had the fellowship. So this is all one period. I certainly had no notion about the strategic value of what I was doing, from a much grander viewpoint. It was another job, that's true, but it was a job that, A, I felt fairly qualified for, and B, that I enjoyed, and C, that gave me the opportunity to go and get a PhD. I don't know whether I would have done that without that. I had a job, and I did have the GI Bill, but the GI Bill only took me through the master's. Tuition was low in those days, but it would have been a real hassle to go back to school.

05-01:11:40

Burnett:

You'd said once that — you'll cover this probably another time—when someone remarked that your life had been altered by what happened in Europe, I think your reply was that, if you had stayed in Europe, you probably just would have entered the family business, right? Or something like that.

05-01:12:12

Leitmann:

It's quite true. It's quite true.

05-01:12:17

Burnett:

And when you were recruited for China Lake, at that time, were you thinking, "I just want to make a living, I want to have a family, I want to have a life, and I want to do something challenging and interesting?"

05-01:12:35

Leitmann:

Yeah.

05-01:12:38

Burnett:

So the pattern, if there's a pattern, is, as a child, you were raised in crisis, at a certain point, in your adolescence, you were buffeted from one country to another, and then ended up in the United States, had a very happy time there, and then were enlisted in the war effort and fought with distinction there. You're demobilized, you go back to school, and you're recruited. You're coming back into this orbit of military affairs. Is it just because the world was saturated with that, and those were the opportunities there? They're ramping up. There are opportunities for research? Or was there a calling for you to some degree?

05-01:13:43

Leitmann:

I always wanted to be in science after high school. Starting out, I was interested in biological systems. Then the idea of going into engineering came not much later, because, after all, I went from high school into the Army. I was already sort of pointed—I did well in mathematics in high school—into that area. By that time, it was fairly clear I wasn't going to go into business. Let's put it this way. But I was here, of course, already. I had an uncle, I mentioned before, Uncle Paul, my favorite uncle. He was a chemical engineer, and I admired him a lot. So there's no question about it, that there was that influence. I know he was extremely pleased, although he died in his early sixties, that I had got a PhD. He was very pleased with that, of course. There isn't any one thing that can say that this is what did it. I alluded to this before—I guess I had mentioned it—that there was a footnote in an autobiographical article I wrote for a journal called *Space Flight* that I owed my education to Hitler — the GI Bill — and my job to Stalin, the rocket program. These humongous events in the world have, interestingly enough, effects on individuals, in a very intimate way, I think. That's certainly true. This is a very general condition. People go into things. The lucky ones get sort of pulled into things that they like, that are interesting, that are remunerative. Others get to work in shipyards, and get asbestos lungs. This is why I say I've

been very fortunate. I've been pushed and pulled into things that worked out extremely well.

05-01:16:21

Burnett:

These are central stories of each age. There's war, and the violence associated with that. You were involved in that, and could not escape it. Then, in the Cold War, you're involved at the apex of military research and development. You had those opportunities, but you're central to some of these major stories.

05-01:16:54

Leitmann:

Again, this reminds me. Did I really want to, say, for example, become a professor? I remember going to a technical meeting at MIT, '52, '53, that period. I looked around. I said, "Gee, this is the kind of place I'd like to be, a university." I was being internally urged to do that, which may have been another criterion for going for that fellowship, for all I know. It's more complicated than saying it's this, it's that. It's always a combination of things, and things that are unpredictable. They're things that are completely unpredictable.

05-01:17:56

Burnett:

And as it happens, Berkeley was a great place to do engineering, or engineering science, and physics and chemistry for that matter. It was an absolute center on the West Coast. So it wasn't such a tremendous sacrifice; it was yet another equally good opportunity. What's different, I suppose, is that, you're right, normally, when you choose graduate work, you're going to work with somebody. You have an idea—

05-01:18:26

Leitmann:

Again, I fell into with whom I worked.

05-01:18:28

Burnett:

Right. I also imagine, then, that you were set up to be more independent at Berkeley, because you, in a sense, had to be. Your thesis advisor was not an expert in what you were doing, or didn't necessarily have a deep interest in what you were doing.

05-01:18:52

Leitmann:

Well, it's interesting in the sense that I was not an engineer, so I had to go for a degree in engineering science. So I have a PhD in engineering science. Number two, I was hoping that I could concentrate on theory and not have to do a lot of computing, because in those days, it was really a pain in the neck, computing. I remember that Nancy used to send me off to the computer center at China Lake, when there was a way to use the computer, because there was less traffic and all that. She woke me up at midnight, gave me a sandwich, and I trotted off to the computer center. I must have collated a million of these IBM cards, because in those days, making programs was really—

05-01:19:47

Burnett: It's a physical process.

05-01:19:49

Leitmann: I never took a course. There were no courses in computer science. You had to write your own programs and all that. It was Fortran. I don't know and I didn't like it. I really didn't. In fact, I was manic-depressive during six months, because one day I thought I was finished, and I drew the curve and made a conclusion, and then I looked at it the next morning, and it wasn't so good. I had to go start all over again. I did a huge amount of computing, first with an analog computer, and that wasn't good enough, and then with a digital computer. Again, I was lucky that I had got to the point where Werner Goldsmith would accept the amount and quality of the computing that I did, because he insisted that I do real numbers. He wanted to see the curves and analyze the system from that point of view, not just give a bunch of equations.

05-01:20:49

Burnett: Was that valuable to you in the long run?

05-01:20:54

Leitmann: It was valuable only in the following sense, that I knew that, even if I would work then in other areas, I should work both on theory and applications, or at least show that this is useful for a reasonably interesting example. I would then, particularly among the postdocs I got, try to get those that are very good and eager to do computing as well. So I did almost no computing since I came here. It's very interesting.

05-01:21:33

Burnett: Your dissertation, it is interesting, because you actually walk the reader through trying to use the analog electronic computer.

05-01:21:47

Leitmann: It failed. It wasn't good enough.

05-01:21:48

Burnett: Then you then work on an IBM 704?

05-01:21:53

Leitmann: Something like that.

05-01:21:54

Burnett: It was a fixed-point arithmetic for it. What you were working on was too complex and required too much precision for what the available, very good, high-powered computers at the time could do. Your research is actually pointing forward to a time when you would have computing—

05-01:22:24

Leitmann: That's why I was manic-depressive, because it looked okay one day, and then the next day, clearly this wasn't good enough. I would have much preferred even to do this with an analog computer, because to me, as physics, not a

mathematics trained person, this was a true experiment. Every time you use an analog computer, you're writing the equations for a real electronic system. So you don't do numerical analysis. What you're doing is physics to get the answer.

05-01:22:55

Burnett:

That's absolutely fascinating. I wanted to ask you about that, because I know very little about electronic analog computers. You write circuit diagrams?

05-01:23:04

Leitmann:

That's right.

05-01:23:06

Burnett:

So can you tell me about how the circuits—

05-01:23:08

Leitmann:

No, you start the other way around. You say, I have a certain equation that I have to consider, and then find the circuit for which this is the equation.

05-01:23:19

Burnett:

That's amazing. Then, in effect, with the electronic analog computer, you do a kind of circuit design that the analog computer then computes.

05-01:23:30

Leitmann:

Right. Then you have to build in the knobs you have to twiddle, those are the control variables. They just didn't work fast enough. The accelerations, the changes in velocity or whatever, were much too high to do this on just simple circuits.

05-01:23:54

Burnett:

In your dissertation, you call for a floating-point system that would allow you to compute this. Your colleague, [William] Velvel Kahan, in computer science, ends up forcing IBM to put floating-point arithmetic into their computers.

05-01:24:12

Leitmann:

So I lucked out. That's right. I probably would still be working on them. [Laughter] That's why I say it's so fortuitous to be able to do that. I'm very grateful, because, as I say, ever since then, it's all been good. All been good. And, I have to say, there were people who helped me. There was a man by the name of Selfridge, who checked my programming. I was very grateful to him. And Diliberto was very useful, because he was interested in nonlinear differential equations. What I faced were nonlinear differential equations, of course. They weren't super fancy equations, but nonetheless, they had to be nonlinear, because once you linearize them, you lose a lot of the real description of those systems. Things just came together. Nancy and I talk often about it. She's not aware of where I was fortunate, but she knows I was fortunate, because I was pleased. So obviously, things must have been going well.

05-01:25:42

Burnett:

I want to ask you about some of the people that you were drawing from, or who influenced you. There's H.S. [Hsue-Shen] Tsien at Caltech. Can you talk about what he was working on with [Robert] Evans? Is that right?

05-01:26:04

Leitmann:

Yes, well, he was working on a lot of things. First of all, although he was Chinese, he was a full colonel in the US Air Force. He had an honorary appointment as a colonel. He was very famous. He was sort of the chief scientist, or among the chief scientists, at Caltech. I had come across his paper very early. Maybe it was 1951, '52. I can look it up. I guess it's in my thesis here somewhere. That's the page where I found a misprint. Let's see. Must be very early. Tsien, yeah, 1951. *Journal of the American Rocket Society*. So that was one of the things, Tsien and Evans. This must have been just after I came to Berkeley that I decided it's just a trip down to Pasadena. I had just come across his paper. This was maybe 1954. Three years earlier. Essentially, I wanted to meet him, and secondly, get his advice. I went to his office. I wrote to him or called him — I forget now — and he said sure. He was sitting in his office, surrounded by open boxes and reports on the floor, everything. He was a very unhappy person; he had decided to go back to China.

05-01:28:06

Burnett:

Now the People's Republic of China.

05-01:28:08

Leitmann:

Right, was then the People's Republic of China.

05-01:28:10

Burnett:

Just newly so.

05-01:28:11

Leitmann:

Yeah, and he wanted to go back and do good there. He was a native Chinese, of course. The first thing that happened was that they took his colonel rank away, of course. Can't have a guy going to China and be a colonel in the US Air Force. Number two, what he did was he had authored, co-authored, supervised lots of work there, and every report or paper that he wrote, he was packing in boxes to go take it home to China. Well, they wouldn't let him do that. So they ransacked all his stuff, and it was lying there. He was really PO-ed [pissed off].

05-01:28:53

Burnett:

You had arrived just as the Secret Service had come in?

05-01:28:56

Leitmann:

Yes, he had become, essentially, persona non grata, and they wouldn't let him take anything, except himself and his personal belongings. So he wasn't happy. I told him that I was inspired by this paper of his, and Goddard's paper first, 1919, I think, and then his paper with Evans, and a couple of other people. There was a German author by the name of Hamel and two or three other people. Very few papers had appeared in the literature. I think I showed

him the manuscript for a couple of my papers that appeared in '55, '56. He said, "You're wasting your time." He said, "Everything" — what we now call optimal control theory, but whatever it was called in those days — this was before the maximum principle, so he wasn't aware of Pontryagin. He says, "I've done everything in that field; there's nothing else to be done." So he really discouraged me. I was lucky I decided I'm not going to take his advice. He was trying to be kind to me. He said, "You're just going to waste a lot of time, and you're just not going to get anything new in this field." He was very wrong. But then he, very soon after that, realized that, because he became really a big wheel in China. He was the head of their atomic energy program. He was the president of their academy of science. One of his brothers got the Nobel Prize just maybe fifteen, twenty years ago, a very distinguished family. They say the most distinguished family in engineering in China.

05-01:31:00

Burnett:

So he influenced you in an indirect way.

05-01:31:06

Leitmann:

Well, I didn't believe him. Firstly because I thought I had already done something beyond — so if I could do it at my stage, there must be more to it.

05-01:31:16

Burnett:

There's more to do. Exactly. There's Bernard Smith, who worked on liquid fuel designs. He was just a kind of supervisor?

05-01:31:28

Leitmann:

He was quite high up at China Lake. His wife, in fact, was a secretary in our department, and we became very close personal friends. A very interesting but strange person. He became the chief engineer of the US Navy. First became the technical director at Dahlgren, which was a big naval testing ground in the DC area, and then he became the chief engineer. He died fairly recently, five years ago, something like that.

05-01:32:04

Burnett:

And Pierre Saint-Amand? He was a geophysicist. How did you work with him?

05-01:32:13

Leitmann:

Again, I think it was mostly a social meeting with him and his family. Again, an interesting guy. He had a family. They had a goat, Madame Nhu, named after the president of Vietnam, tied up on their apple tree. From his point of view, geophysics, he was interested in the satellite program.

05-01:32:44

Burnett:

Like remote sensing?

05-01:32:45

Leitmann:

Right. We actually wrote up a proposal for — it was Project MOUSE or something like that. Why was it MOUSE? Minimal Orbital something US

something—I don’t know. Anyway, it’s somewhere in the big maw of the Navy. It never got anywhere.

05-01:33:14

Burnett:

I think you wrote about it, that it was somehow connected to the National Advisory Committee for Aeronautics. And then that’s as far as it got. [Prof. G. Leitmann: Rocket Dynamicist, *Space Flight*, Vol.13, No.10 (Oct. 1971): 384]

05-01:33:24

Leitmann:

That was NACA at the time.

05-01:33:28

Burnett:

Basically the advanced version of rocket science—

05-01:33:31

Leitmann:

Maybe it was something that came through them. That’s possible, yeah. Anyway, it wasn’t of major interest. I got in touch with a lot of interesting people at that time. I still have the letters. For example, I got a long letter from the president of the Flat Earth Society in England, and he tried to dissuade me from working on satellites, because he said — actually, his society believed that the earth was an inverted pyramid, and I was going to hit the earth. “Those orbits are going to hit the earth. There’s no way of getting around that.” It’s really interesting. Derek Lawden, one of my personal icons. First of all, a tremendously cultured gentleman. He was a professor, at the College of Advanced Technology, Birmingham, working on spaceflight. Then he headed the math department at Canterbury University, New Zealand and returned to the United Kingdom until his death in 2008. Again, about the 1950 period, he wrote some of the first papers, particularly on space trajectories, going from one orbit to another, that kind of thing. He had written a book called— [*Optimal Trajectories for Space Navigation*, Butterworths, 1963]—a classic volume in the field. In his dedication to me he wrote, “I could have never written my book without your input.” Our friendship — didn’t become a close, personal friend, but a professional friend — was all by mail. This was before email, of course. He had written a paper in which he showed that rocket orbits, rocket orbital control, transferring from one orbit to another via rocket, in a gravitational field—no aerodynamic effects, just gravitation of these bodies—the earth, the moon, Mars—had the property that, no matter what you wanted to optimize, whether it was maximizing the final mass, or minimizing the fuel consumption, whatever, it was all bang-bang. In other words, you always use maximum thrust or no thrust. So either you coasted, or you used maximum available thrust. That’s very attractive to engineers. You don’t have to fiddle around.

05-01:36:30

Burnett:

A kind of putt-putt machine, basically.

05-01:36:31

Leitmann:

Right. In the theory itself of extremal control, there are what are called singular arcs, in which there’s a programmed control that varies with time. So

it's not the maximum or minimum; it's something between the upper and lower bounds of the thrust, but the thrust itself varies in time in some optimal way, or at least an extremal way, in other words, satisfying the necessary conditions. He made some local assumption — I don't remember it now. I have a folder downstairs which has that correspondence, in fact. I said, "It doesn't seem right to me." I said, "Maybe I'll try some very simple gravitational fields, potential fields, linear, quadratic, or something." I found out that I could satisfy those conditions for singular arcs on the trajectory, and satisfy the extremal conditions. That's what I wrote to him. Of course, he was smart enough to realize immediately where he had made his little mistake. But it was a big mistake, because it made the difference between the possibility of not having that nice bang-bang control, which is very attractive. Then he said, "Well, if that's the case, let me take some actual gravitational field models and try it and see whether you can satisfy those conditions," and he could. Then he found something what is now known as the Lawden spiral, a trajectory which takes you along an extremal arc with a non-bang-bang control. To this day, people have not proved under what conditions they're optimal or not. There are a few cases where you can show those are not optimal, but in general, that's still an open problem. That was certainly an important influence on me, and we kept corresponding. He then moved to New Zealand and became the chairman of the applied math department at Christchurch University in New Zealand. Then I got to be a member of the PhD committee for his son there. We got to be fairly close. Then in 1962, '63, he won a major award of the American Rocket Society, one of their big medals, which he could not receive. He couldn't get a visa, because he was one of those British Communists of the Oxford-Cambridge class, the very theoretical—

05-01:39:29

Burnett: Yes, very genteel, armchair Marxism.

05-01:39:32

Leitmann: Right. He could not get a visa nor could he get the medal.

05-01:39:41

Burnett: You're just in the Cold War environment all the time, and you're reminded periodically: here, no, you can't go to Zurich. Your friend cannot get a visa. You realize that there are these boundaries. Even though science is international, to some degree, you're reading about Russian research, and in later sessions, we'll talk about opportunities where you actually met some of your Russian counterparts — Soviet counterparts, rather. So there's the open communalism of science on the one hand, and a very real Cold War in exactly this area of research that you're doing.

05-01:40:30

Leitmann: And we may get it again. [Laughter] It's interesting. By the way, in that very same period that we're talking about, I think it was probably right after —

when was it? When I had my trouble with losing my Q clearance. That was just about that period.

05-01:40:54

Burnett: Late 1953, there was a temporary revocation of your Q clearance.

05-01:40:58

Leitmann: That's right. Actually, I got my fellowship after they decided I'm okay, because I probably wouldn't have got that fellowship from ONR [Office of Naval Research].

05-01:41:09

Burnett: I'm not sure if you've — I may be misremembering this — but that you've actually told the story of that —

05-01:41:21

Leitmann: We may have mentioned it looking ahead.

05-01:41:24

Burnett: Yeah. But it was because of a 1944 letter that you had written to the USSR—

05-01:41:30

Leitmann: Well, there were a number of things. I obviously have a file, an FBI file. I haven't asked for it, but I clearly have. That's right.

05-01:41:41

Burnett: I think we have talked about the reasons why, but can you talk about the actual meeting where you had to defend yourself?

05-01:41:51

Leitmann: Yes. What happened was—the paperwork is down somewhere. I got the notification from the Civil Service Commission or Committee or something like that, that said that there's a question about your fitness for Q clearance, and there will be a hearing at the Federal Building in Los Angeles before a committee. So I went down to Los Angeles, after I visited my aunt and cousin. Just briefly, there were four things. One of them was that I was the information NCO in my unit, the combat engineers, and every Friday afternoon, we spent an hour reviewing the week's news. There had been a report somewhere that the Red Army was accompanied by prostitutes, that the government provided prostitutes for their soldiers to keep them happy. Everybody was interested in this. I said, "I'll find out. I'll write to the Soviet embassy and ask them." And I did. I got a letter very quickly. Of course, the return label said "Embassy of the Union of Soviet Socialist Republic." That stuck out there on top. I got a one-sentence answer: "The information you have is incorrect. Yours sincerely." Somebody reported me. No question about it. I mean, how did it get into my file? Then my family were always social democrats, my father and my mother. My mother, to her dying day, was. It was the Henry Wallace election. What was that, '48?

05-01:44:03

Burnett:

Wallace was '44, I think. [Wallace ran as the nominee for the Progressive Party in the 1948 presidential election in the United States.]

05-01:44:06

Leitmann:

Forty-four, okay. There was something called the American Labor Party in New York. My mother had become a member of the American Labor Party, and I became active in the Wallace campaign. I was at Columbia at the time. So it must have been—

05-01:44:28

Burnett:

It must have been after.

05-01:44:28

Leitmann:

It must have been later. It must have been in the '48 election, because I was at Columbia at the time. That was in my file, obviously. Then, when I went to Columbia, I took two anthropology courses from B.J. Stern, famous social anthropologist, who wrote a very big book called *When Peoples Meet*, which was highly regarded. But he was also the editor of *Science and Society: A Marxist Monthly*. So he was a Marxist. It was a two-semester course, and I forget now whether it was the first or second semester he assigned our reading the *Daily Worker*. Instead of finding it in the library, I actually subscribed for it for a semester. That was enough. Those were the four things. Of course, they knew how my mother and I had voted—or registered. You register for a party. In those days, the American Labor Party was a registered party. So that's what I was accused of. I got up and I said, "I'd like to ask the committee to allow me to make a statement." They said, "Of course." They were very nice. So I pointed out that whatever I did, whatever the motivation was, what I did, i.e., write to the Soviet embassy, registering for a legal party, voting for a legal candidate, and subscribing to a paper, a newspaper, were all legal, and in fact, were incumbent upon me as a person who believes in democracy, that I was simply exercising my constitutional rights. And I got applause. They actually applauded. I got everything back, job and clearance.

What I did lose at the same time, however, was my commission. I was a second lieutenant in military intelligence reserve, and I got what is called a discharge under honorable circumstances, not on honorable discharge, which means that it's a discharge for the benefit of the government kind of thing. I didn't do anything illegal, but it was for their benefit. Then, when Kennedy became president, they sent me the certificate for an honorable discharge. I was moved from discharged under honorable circumstances to an honorable discharge. [Laughter]

05-01:47:30

Burnett:

That story just tells so much about the environment that you were in, or the times that you were living through.

05-01:47:40

Leitmann:

It didn't, in any way, impinge on my life otherwise. It gave me a big headache all the time, these things, but it didn't hold me up in promotions. I got my fellowships to go to Zurich. ONR gave me funding when I was a professor here. It didn't haunt me, other than giving me — and I don't sleep well anyway. I slept exceptionally badly in those days. That was really the only effect. It's another interesting experience. If you look back on it, you say, gee, look at the things I lived through.

05-01:48:28

Burnett:

Times of great suspicion and fear abounding. I do want to talk just a little bit more about the dissertation. What's striking about it to me — and I haven't read any other dissertations on rocket research in this time period — it's laid out almost as if there wasn't very much about this subject. Each chapter begins with a simple model, and then you move through steps to making it more complicated, which, to me, seems like something where you would need to educate any would-be reader of this dissertation. In other words, by comparison to more recently published dissertations in other fields, in which you are doing a literature review at the beginning, and there's a wide range of things, and then you narrow it down—

05-01:49:39

Leitmann:

I have relatively few references.

05-01:49:42

Burnett:

It's amazing, actually, to me. You start with the kind of idealized rocket of Goddard, and a simple-ish equation of motion. You talk about some of the problems with a real rocket, and that there's the center of mass, for example.

05-01:50:05

Leitmann:

Well, there was something inside the rocket. Most of the models that — and the Russians are very proud. They had a guy who wrote that simple equation down already a hundred years ago. It has his name. It's called the Tsiolkovsky equation (1903) which treats a rocket as a variable mass point. This is contradictory, because in Newtonian mechanics, a mass point, a particle, has fixed mass. This is a particle of variable mass. Zero dimension. When you write the equations for that, you get that simplest model. The people still use that, by the way.

05-01:50:57

Burnett:

You add things, like a mass acceleration term, accounting for constant drag, for example. Then, what I'm curious about, and it's really just because these equations become so important in the 1950s and sixties, in other fields as well, these Euler-Lagrange equations. Can you talk about what they are and what they do? They have a long history. They were developed in the mid-eighteenth century. Then they come to life in the middle-twentieth century.

05-01:51:32

Leitmann:

Well, yeah. This has to do with how you decide to go about getting the conditions for optimality, which is what these equations are. They simply say a system has to be a solution of those equations, in addition to the model equations — we call it the state equations now. That comes about because, in the classical calculus of variations, and for example, in Pontryagin's maximum principle, what you're doing is you are comparing what you assume is an optimal solution to neighboring solutions, which presumably are not optimal, or they're certainly not better than optimal. What conditions arise when you do that, when you look at the equations for those solutions? You do that in, say, the optimal control theory of Pontryagin by making "small"— you can define "small" mathematically by saying something depends on the parameter epsilon, and that epsilon is as close to zero as you wish. Those are the Euler-Lagrange questions. In other words, they come about, and they come about in many different ways, but for some reason Bliss did not talk about, they appear also when you simply look at the sensitivity of solutions. In other words, how sensitive is the outcome of something due to the neighboring solutions? For example, these lambdas. They're called adjoint variables in different fields. In economics, they have a very definite economic interpretation. They are simply sensitivity of, say, what you want to minimize or maximize — say profit — to what you do. So they're sort of sensitivity variables. Bliss, by the way, had worked on that in the twenties, and then went to the calculus of variations and somehow didn't make the connection. Maybe he did, but didn't publish on it.

05-01:54:07

Burnett:

In economics, it's almost like elasticity. Elasticity of demand is like the sensitivity of one variable with respect to another.

05-01:54:13

Leitmann:

Right. That's what they are, essentially, elasticity variables. We're jumping ahead now to later research. They're also related to, say, in dynamic programming, they arise in terms of being partial derivatives of things. The values of those partial derivatives would have to do with the effect on the outcome with respect to variables in the system variables. If the outcome is V , and the variable is X , then the partial derivative of V with respect to X — if you say what is the value of that at a given time? Those are the lambdas. In other words, they are the sensitivity variables. This presupposes that you have a fairly smooth function for your outcome. Then it gets very complicated when you don't. All these things are related. Then, later on, when I worked with [Austin] Blacquière on the geometric theory, that again is very much related to dynamic programming, but more from a geometric point of view. Those are points of view, then, where you look at global optimality, once you say what do you have to have globally and geometrically, for example, the variable that determines your outcome, utility function, sometimes called. But in a region of space, your function space, rather than looking at local things. Then, once you have that, you can say, along those surfaces, if you look locally, then you end up again with the Euler-Lagrange equations. There, you

start again with partial derivatives, because it's your geometry of the outcome, the utility function, with respect to local variables. Again, you say, if you now are along a particular trajectory, so at any given time, what is the value of those variables, they again turn out to be those adjoint variables, the lambdas in this case. You get them in ordinary function theory, where they're called Lagrange variables. In other words, they're penalty functions. If you adjoin constraints to something you want to do, you adjoin them with those lambdas. That got to be sort of a trick, and unless you actually analyze where these come from, you haven't got a complete theory. You get people who just do it. They say, if I add something which has to be equal to zero to something that I want to maximize, you don't change anything. Well, then you say, what the hell are you doing?

05-01:57:38

Burnett:

It's important to understand what they mean.

05-01:57:40

Leitmann:

You know, why are you doing this? People are doing it because it's been done before, where it was done, where it was proved, and now they say, well, if you want to, say, you have certain constraining equations, we'll just add them on. They say some function of your variables is equal to zero, and then you add it with a lambda in front of it to what you're trying to minimize, you're not changing anything. You can do that, but that's not a justification for doing it.

05-01:58:11

Burnett:

In a future session, maybe our last session, it would be great to talk about—a complaint that a lot of scientists and researchers make is that people can get stuck in sophistication for its own sake, and they can say, “Well, that's really fancy. I'm going to use that, I'm going to apply that,” without fully understanding what that sophisticated mathematics does.

05-01:58:37

Leitmann:

Well, it goes both ways. You can say also, “If it gets too fancy, then that's a case I'm not interested in.” You get the counter-argument. I got involved, later on, in looking at sufficient conditions, not just necessary ones. I have a colleague in particular, a friend actually, who used to tell me, “You're wasting your time. These may occur, but they're so rare.” I don't know what that means, they're so rare. If they can occur, then you never know. It depends on how you look at the world. I'm risk-averse in many ways. I discussed this yesterday with our host at lunch. Looking at sufficiency conditions is like buying insurance. It's that kind of thing.

05-01:59:34

Burnett:

Let's have a whole session talking about that. I think that's so interesting.

05-01:59:36

Leitmann:

It's interesting.

05-01:59:37

Burnett:

It really is. Just to finish up talking about the dissertation, you were looking at nonlinear phenomena, and so you needed ways to explore that mathematically. Did you have that in your equipment, in your arsenal of mathematics already? Or were you learning something at Berkeley or at China Lake? Did somebody influence you?

05-02:00:07

Leitmann:

I did not have that in my arsenal. The mathematics I took at Columbia, both for bachelor's and master's degree, was really very elementary. You've got to remember, this is sixty-five years ago. The mathematics that was taught in those days is practically taught in high school now. Maybe not understood when it's taught, but really. Partial differential equations was a graduate course; you didn't do partial differential equations before that. You sort of touched on advanced calculus, but very peripherally. I really regret it. A little bit too late to do anything about it. My mathematics is very sparse. I have to get a lot of help, and very often, I will have what I think is a good idea, take it through the simple cases that I can deal with, and then somebody will come along, and if he finds my basic idea interesting, he or she will do better with it. That happens all the time. It happened to me a lot of times.

05-02:01:24

Burnett:

In your graduate work at Berkeley, who was a big influence on you? Or were there courses that really helped you when you were at Berkeley on your graduate work?

05-02:01:34

Leitmann:

Courses at Berkeley?

05-02:01:35

Burnett:

Yes.

05-02:01:35

Leitmann:

No, because not being an engineer, they made me take very basic engineering courses. Except, for example, my own thesis advisor taught a course, two-semester course, in continuum mechanics, fluid mechanics maybe. I forget. Anyway, that involves partial differential equations. But in those days, you could teach a course at Berkeley with one student. He taught this course. He was a very tough guy. Students were trembling just looking at him. I signed up, since he was my own professor, when I took courses still at Berkeley that one year — I took courses only for one year — in that two-semester continuum mechanics course, I and another guy were the only two students. He gave a perfectly formal lecture Tuesday and Thursday, two-hour lecture —

05-02:02:45

Burnett:

To two students?

05-02:02:46

Leitmann:

Perfectly, in a loud voice. There's a funny side story about this, because second semester, I was the only student. I was already engaged to Nancy. This

must have been 1955. I said, “Werner, Nancy wants to go shopping. We’re engaged. Is it okay if I don’t come to Thursday’s class?” He said, “Oh, no problem, George. No problem.” So the next Tuesday, I didn’t understand what he was saying. I said, “How did you get from equation five to equation seven?” He said, “Well, you weren’t here for the Thursday lecture.” He was nice about it. He let me look at his lecture notes after that. He gave, by the way, a formal lecture to me.

05-02:03:40

Burnett: Just alone? As if you were a giant audience.

05-02:03:43

Leitmann: Didn’t make any difference. He was very conscientious, a tremendously conscientious, very hard worker. He wasn’t going to repeat what was in the Thursday lecture for me.

05-02:03:59

Burnett: It’s almost as if he were standing in front of an empty auditorium giving that lecture.

05-02:04:05

Leitmann: Well, it was essentially empty. The reason was that he only had so much time, and it was filled up. He gave a very full lecture. If he had stopped to bring me up to date, the whole thing would have been out of whack. So he gave me the Tuesday lecture on Tuesday, yes.

05-02:04:27

Burnett: That’s fantastic.

05-02:04:29

Leitmann: By the way, I was the speaker at his memorial service, and I told that story. Everybody knew what I was talking about. [Laughter]

05-02:04:41

Burnett: Well, there’s so much more to talk about, but let’s pause for now, and we’ll continue talking next time about — you graduate, and you transition from a graduate student to becoming a professor. We’ll do that next time.

05-02:04:58

Leitmann: Fine. Just in time.

Interview 6: September 7, 2018

06-00:00:17

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History Series and this is our sixth session. And it's September 7, 2018 and we're here in the Berkeley Hills. The last time we talked you were finishing up as a graduate student. You'd finished your dissertation and you're about to enter the next phase of your life. But to get us there, why don't you tell us a bit about your personal life in the mid-1950s, as well.

06-00:00:53

Leitmann:

Okay. I think we had already mentioned that I had met Nancy. We were almost engaged by the early '54 years. Then the question of my getting a very, very handsome fellowship, I guess is what you might call it, towards the PhD. Then it was, as I mentioned, my boss, Dr. Highberg, who convinced me that I ought to go for that because he has a doctorate, and fixing the toilet on a naval base is done more quickly than for somebody without one. So I decided I'd better get a doctorate. And then I had to pick a place to go and the most eminent person in the field that I was going to do my thesis in and had been working even on the job was Raymond Sanger, who was a physicist and one of his specialties was ballistics. In those days all you had to do was apply to a professor, eminent professor, without going through all the paperwork. And if he or she said, "Yes, indeed, you're welcome to work with me," that was it. So that's how I got admitted to ETH Zurich, I guess the MIT of Switzerland.

So everything was resolved. Nancy, for example, thought she would not see me for some time and so she quit her job at China Lake and moved to San Francisco and got a job there. So that must have been the middle of '54. I was getting rid of all my stuff, already packed the steamer trunk and it was waiting at the pier in New Jersey for me. And then early in August I was visited, as I already recounted, by two gentlemen from the Office of Naval Intelligence, fellow special agents, I guess.

06-00:03:10

Burnett:

Yeah, right. Did you mention that to them or they knew already, of course?

06-00:03:13

Leitmann:

Oh, no, they knew that. And they told me I could go anywhere for my degree but not in Switzerland because, remember, it was the height of the Cold War. It was a nest of spies. Which probably was true, you know. They just didn't discriminate whom they were allowing the spying. So there I was stuck. And as I again recounted quickly, I mentioned these to two professors from Berkeley who were consultants, one from engineering, Werner Goldsmith, and the other one from the math department whom I had already known before because he was interested in the work I was doing at the time on the equations of rocket motion, general notion of variable mass systems. He had written a couple of reports at China Lake about this. One is called the Rankin-Leitmann formula. They were able to get me admitted to Berkeley almost

immediately. I was going to work with Werner Goldsmith, who was a very young professor. I think he had just become an associate professor. So that was one fortuitous consequence of having been denied the possibility to go to Switzerland. So that was an immediate plus, of course. That became more and more obvious as the years progressed, how lucky I had been. And the other one on the more personal side was the fact that I could reunite with Nancy just a couple of months after we had parted. And so we became engaged right away when I returned to Berkeley, September, I guess, or beginning of the semester anyway, 1954.

And then almost an immediate consequence was that we were married the following January, January '55. Just as an aside, my mother came out from New York. One of the two aunts that I had in California, Aunt Adele, was involved in making little festivities. She was going to plan the wedding dinner. It was all going to be family. There wasn't going to be anything official, and, again, to please my mother, since I had become an agnostic after *Kristallnacht*, I agreed that I would be married by a rabbi. It was in one of the old synagogues which I simply don't remember now. What I do remember is Nancy and I were married privately in the rabbi's study and he was a doctor of—divinity from Harvard—

06-00:06:29

Burnett:

Yes, divinity.

06-00:06:32

Leitmann:

Doctor of divinity from Harvard. So that's not usually what rabbis are. Also in the conversation with him afterwards the question came up that Nancy was perfectly willing to be converted to Judaism since she was a Protestant atheist. He said, "No, no, no." He said, "A, it's not necessary. B, you're doing it just to please your mother-in-law so we're not going to do that." So that ended that.

06-00:07:06

Burnett:

Did her parents come to the wedding?

06-00:07:10

Leitmann:

No. Her father had already died and she had a stepmother, because her mother died in childbirth of her brother. They were two or three years apart. So, no, nobody came from her family. And from my family, actually, the only ones who were there were my mother and the immediate family of Aunt Adele's. She had two sons, one who lived at home all his life, Charlie, and my cousin Al, who was married by that time. So that was about it. That was my wedding dinner. Oh, and the best maid, Nancy's friend Anne. She came. She was essentially a native of this area. And that was it. It all worked miraculously well and that became more and more obvious as time passed, that this unhappy consequence of the Cold War and the McCarthy era still really had some very good outcomes. I marvel at it. I really hadn't thought about this for a long time. It just came back to me how well that all worked out.

06-00:08:43

Burnett:

Yeah, it's amazing. And you are still with Nancy and this is—

06-00:08:46

Leitmann:

Sixty-three plus years.

06-00:08:48

Burnett:

Yeah. [laughter] That's amazing.

06-00:08:54

Leitmann:

I think there are two reasons for this, quickly. A, we are diametrically opposed in terms of personality. She is very optimistic and very pragmatic. So, for example, I am a person who is not only not optimistic, I'm a pessimist, maybe by history, and also a terrible worrier. And to this day her advice to me, but she knows I don't heed it, is that, "What you can't change you can't worry about. It's silly," she says. One of the things I worry about is the fact that what will happen if our kids and grandkids have problems, I won't be able to help them. That's the thing that exercises me when I can't sleep. And she thinks this is ridiculous and, of course, she's absolutely right. But you can't change those things. It's a personality thing. And so we were a zero sum game, which worked out very well. We had sort of the optimal solution.

The other thing is right from the beginning I had to make an agreement that, first of all, that was a financial agreement that I make the money and she spends it [Narrator addendum: Just kidding]. And the other one was that we would not try to change each other. We might learn from each other. That's exactly what happened. She, for example, usually went to bed very late when I met her, two o'clock in the morning, and then sleep late. She used to sleep late and I got up early. And to some extent, now, again, we're still learning from each other because I have to wake her up in the morning. She still likes to sleep late from the days when she stayed up until two o'clock in the morning.

06-00:11:02

Burnett:

She's catching up.

06-00:11:04

Leitmann:

Yeah, sort of. Okay. I've been very fortunate in this and many other respects. This will show up when I talk a little bit about the consequences professionally of one we already know. I came to Berkeley. A, got my degree at Berkeley. B, however, it turns out I got my degree in '56, in the fall of '56, or June of '56, I guess. We had already moved back to China Lake because I only spent the first year in Berkeley for my degree and passed my exams and then went back to work, so to speak, at China Lake to finish my dissertation, which I did. So '54 through '56 was my time, two required years to be registered, I guess. Then, really to my surprise, in '57, the department where I got my degree invited me to join the faculty. At that time the chair of the ME department was Professor Walter Soroka, which by the way means "noisy bird" in Russian, a really awfully nice man. But I did learn something else, just as an aside, in this process, and it shows you that I wasn't very bright

diplomatically and I learned a lot from that. While I was doing my dissertation, which basically involved dealing with mechanical systems with mass change, rockets, for example. Exhaust leaves. For big rockets that's a major part of the mass of the rocket. In a V2 90 percent is the fuel. So you can't have this business "we'll assume that the mass is almost constant..." It just doesn't work.

So our chair at the time I started my degree was a gentleman by the name of Merriam. I forget his first name. And he had written a very successful undergraduate book in mechanics, primarily dynamics, particularly because he had very good examples and most of the books in those days did not have very good examples. But I found out in the process of doing my dissertation that he had some examples where he was absolutely wrong in the way he solved them and, in fact, he had the wrong equations for mechanical systems of variable mass. There are these very nice examples of, say, having a pail of water on a scale and adding water, then how does the weight go up or dropping a chain or a spool of wire across that either unrolled or rolled. So the mechanics of the system that you're trying to describe is a variable mass. He had that completely wrong. And I made a big mistake. I gave a student seminar, these one-hour seminars while you're doing your dissertation, and I devoted it to pointing out the mistakes in his book. [laughter] That occurred to me pretty quickly. So I wasn't his favorite person. But he was very smart. Of course, he understood immediately what I said. I mean, it wasn't that he didn't understand it. He just did what a lot of people did incorrectly. He got himself an NSF grant to work on variable mass systems and so he had a very nice grant. The only other aside is he was going around lecturing on variable mass systems and he was at—maybe it was at Carnegie Tech. Anyway, one of the tech colleges back east. And he gave a lecture on this. And one of the people during the question and answer period got up and said, "Well, isn't this in Leitmann's thesis?" And his reply was, "Oh, yes, but he was one of my students," in the sense that he was the chair of the department. [laughter] So that's another aside. He was very wealthy and his book made a lot of money. He bought land in Hawaii and had a big estate on Maui. So he did very well. And then he rewrote the book with a co-author and they got it then all right in that edition. That book was used for a number of years then. So that taught me not to do that again. And, as you know, since then was always been my policy when I think I find something wrong in somebody else's work I get in touch with that person and that's been very good because very often it led to cooperation, for example, and at least friendships. Say with Lawden, for example. We talked about that.

06-00:16:30

Burnett:

Sure, right.

06-00:16:32

Leitmann:

So that worked out extremely—

06-00:16:34

Burnett: Yeah. There's a strong social component to science that we often forget about, don't we.

06-00:16:40

Leitmann: Right. Which, by the way, brings me to something which may be interesting just for any potential listeners in the future. If they want to get more details, particularly on the technical aspects of what I've been doing, there are two sources. One, of course, is my university webpage, which has both a CV content and then bibliography, 315 or so items. I lost my thread. Oh, yes. And what is now the social network for science, called ResearchGate, one word, and there you will find hundreds of people, including me, with their data. So there would be a bibliography in more detail, there would be correspondence and all that sort of thing and it's very easy. Just ResearchGate, one word.

06-00:17:43

Burnett: For as long as there is an internet.

06-00:17:45

Leitmann: Right, that's right.

06-00:17:46

Burnett: And in case there isn't or there's an evolution to another platform of some kind—

06-00:17:50

Leitmann: Yeah, it'll be transferred.

06-00:17:51

Burnett: —it'll be transferred over. And then there's the Bancroft.

06-00:17:54

Leitmann: Yeah. So those are two very good sources—

06-00:17:45

Burnett: Sure.

06-00:17:57

Leitmann: —for anybody who wants to fill in the gaps, particularly on my career work.

06-00:18:02

Burnett: Can I ask you about the time you spent at Berkeley but you were also—did you get a leave from China Lake to do the year at Berkeley? Presumably?

06-00:18:19

Leitmann: No. That was the whole thing of this fellowship. Said that you keep your job as a fully paid person while you're working towards your doctorate. It was a fantastic thing. It was sort of implied that this means that you're going to come back. It is one of the reasons I did decide, in fact, to go back for two years afterward, '56, '57, until I came here in '57, in the fall of '57. And no. Again, it was one of these things. It's probably the best fellowship I ever heard of

because there wasn't even a time limit of how long they would support you for a doctorate. But it was sort of at just the beginning of the space age and there was just a lot of money around, you know.

06-00:19:12

Burnett: Yeah, yeah. And this was Office of Naval Research?

06-00:19:14

Leitmann: Yeah. And it was really the primary agency funding sort of applied math subjects. NSF almost had nothing at the time. And the other one which was major was the AFOSR, the Air Force Office of Scientific Research. But the ONR, the AFOSR are really the two funding agencies until maybe even the early seventies.

06-00:19:43

Burnett: So that's really interesting. So it's the two branches. It's the Air Force and the Navy, their research arms, that are interested in aeroballistics, for obvious reasons.

06-00:19:53

Leitmann: Well, in essentially applied engineering, you know, applied mathematics. They both had very large mathematics divisions. A little bit later there's a funny use of that resource, maybe middle sixties, that I encountered but we'll get to it very soon.

06-00:20:17

Burnett: Right. But it could be things like logistics, applied mathematics. As you did in your career, it was economics, it was ecology, fisheries, that kind of stuff.

06-00:20:26

Leitmann: Yeah, yeah.

06-00:20:26

Burnett: It has applications in many, many different domains.

06-00:20:28

Leitmann: Yeah, right. And it was pretty much the same kind of mathematics because it's a little bit like in analog computers where you do electrical circuits. But the things that you solve via those circuits, maybe in any field where the equations apply that you could write circuits for. That was really the whole thing. Well, this goes right back to really the importance of mathematics, whether you were interested in mathematics for its own sake or for future applications and even if you're not interested future applications come along later on. So it's obviously the language of science. Even in biology. So are the other sciences, other sciences. But mathematics is sort of a common language across the board. It's something that I always try to stress with students. I think they get more advice on this now than they used to. But, say, thirty, forty years ago, particularly with women—it wasn't until fairly recently—they simply were told, "Mathematics is for men and if you want to marry well or get a good job be a good cook," kind of thing. So that's a very happy

development in advising students. It's still not good enough but it's a lot better than it used to be, no question about it.

06-00:22:22

Burnett:

And the Office of Naval Research supports you for a long time. As far as I can tell, certainly in the 1960s, you're regularly getting funding from them to go to conferences and to put on conferences and things like that.

06-00:22:38

Leitmann:

Yeah, yeah. Oh, no, they had a lot of money. We sort of jump ahead. But, for example, they funded things like two or three week summer schools where both the pay and the circumstances were luxurious. That, of course, all changed in more recent years simply because budgetary—this is universally true. My European colleagues tell me the same thing, that the budget restrictions have been much more stringent than they used to be. Even in Germany, for example.

06-00:23:19

Burnett:

Did you notice, were there several phases of that? Did you notice that funding became more scarce in the 1970s, for example, with the fiscal crises in the government?

06-00:23:29

Leitmann:

Yeah, yeah, to some extent. It was personally not so important for me to do that because I was very fortunate. Over the years, I didn't count them exactly, counting my graduate students, post-docs, and visiting scholars, probably seventy or eighty people that came to work with me. And I think of those maybe only 5 percent needed support. The rest came with their own support. Which was terrific for me, of course. And, of course, I was in a field which wasn't experimental so I didn't have a laboratory to support. So it was really only maybe getting a summer salary or going to meetings or paying for numerical work on the computer. That kind of thing. So that I didn't have huge budgets at any time compared to the one, two, three million dollar budgets for people with big laboratories.

06-00:24:23

Burnett:

Right. With potentially tremendous payoff, though. So you might be a cheap date in terms of the investment of research but—

06-00:24:32

Leitmann:

Well, that's a judgment I leave to others. I very often needed support, student support, graduate student support, and that came almost entirely from those ONR, then later some NSF funding. So except for maybe my Iranian students who had enough money in those days when the shah was in. Of course, there was no problem. They came with buckets of money. So it depends on where they came from.

06-00:25:08

Burnett: So you had returned or agreed to return to China Lake to work there but you had then received this offer to join the faculty.

06-00:25:20

Leitmann: Yeah.

06-00:25:23

Burnett: So what was that like to—

06-00:25:24

Leitmann: I was very happy. Because there was nothing in writing that said you are required to come back or anything like that. It was sort of like a gentleman's agreement. It wasn't even mentioned orally but I knew that they were hoping obviously to get somebody who has a doctorate. But that was sort of *sub rosa*. It wasn't really spelled out. And I'm glad I did that because I felt like an ingrate if I didn't at least spend a couple of years. And really only used the salary the first year when I was at Berkeley. The second year I was on the job again. The *quid pro quo* was that during the night I could use the computer and they had a much better one than they had here, one of the IBM machines. It all worked. The more I think about all those aspects of my life, I can't believe how—and Nancy and I talk about that quite a bit with respect to other aspects, how lucky we've been, and certainly I have been, family-wise and professionally. Maybe it's to make up for my two-and-a-half years under the Nazis. Maybe there's some kind of guardian angel involved. I don't know.

06-00:27:00

Burnett: [laughter] Maybe. Did you ever encounter or think about Raymond Sängner at ETH in Zurich or is that a path you did not take?

06-00:27:15

Leitmann: Well, just a couple of months ago I looked him up and I found him. You can Google him. He was that well-known. And he died in 1962. I came across a newspaper article on him, really his obituary. I probably would have barely finished. Well, I would have. No, he was really quite a well-known person. He did very well without me. Let's put it this way. [laughter]

06-00:27:55

Burnett: Well, so you come to campus and they now have someone who's working in this domain. So now you've become that expert at Berkeley. What were your thoughts around developing a research program? Did you think, "I'm going to extend the work I was doing at China Lake and for my dissertation?" You joined the faculty the year of Sputnik.

06-00:28:29

Leitmann: That's right. So if I look at my bibliography, almost everything that I did at China Lake that got to be a report was restricted. I can put it on my bibliography now but I don't really care. But the first paper I published in the open literature was a paper in 1956, so before I came to the university, on cross-wind launching in really a professional journal called *Ordinance* that had

to do with my experimental work at China Lake. Because that was already in a sense unclassified by that time. For example, there was an article in *Popular Science* which shows me standing next to a rocket launcher. In that book that just came out, what, six months ago on the history of China Lake—

06-00:29:41

Burnett:

Yeah, that's right.

06-00:29:43

Leitmann:

—there's the same picture of me. In neither case is there an attribution to me but I have the original photographs and more of the same launcher. So it was possible to publish. And then I had already got in touch for my thesis with [Hsue-Shen] Tsien about Goddard, right, who was an icon, of course, a rocket pioneer from before the First World War. So the second paper that I wrote was called “A Calculus of Variations solution of Goddard's problem” in a journal called—in fact, maybe the second issue of that journal, second year, *Astronautica Acta*. And I mentioned the other day to you that the editor-in-chief was Wernher von Braun. So that was sort of funny. Then, in fact, I worked on related problems and published unrelated problems maybe for the first twenty or twenty-five of my publications. And some with other applications. For example, the determination of air density at high altitude by means of an earth satellite. So that was involved with high-altitude rockets in a way. Then I did become interested in what later led into game applications. I wrote a paper on optimum pursuit problems. So this now involved essentially somebody trying to catch another object, the other object trying to get away. And that I published in the *Journal of the Franklin Institute*, which was a very, very general journal, of course, and still is. Because I think you asked me why *Journal of Franklin Institute*. Because they published almost in every field and they still do.

06-00:31:58

Burnett:

So it didn't have a focus on control systems per se?

06-00:32:03

Leitmann:

No, no. And then one of the first papers that actually, to this day is being referenced, I just looked it up, there were recent papers, people presenting a paper at a meeting. They were Romanians. They referenced this paper. So it's amazing. It was called “On the Equation of Rocket Motion,” which was the simplest model that I have in my dissertation, published in the *Journal of the British Interplanetary Society*. I guess we mentioned there were really only two. That one so nicely called that. They weren't fancy about it. It still exists. It's still called that. The British are much more conservative in adapting titles. And the other one was then called the *Journal of the American Rocket Society*, which became the American Institute of Aeronautics and Astronautics.

06-00:33:08

Leitmann:

AIAA. So that, of course, is the current title of that organization. So it's the major aerospace professional society of the United States.

06-00:33:19

Burnett:

Yeah. These associations are either retooling for the space age, the coming space age, American—

06-00:33:30

Leitmann:

Well, and also for military purposes, very much so. What was published very often was applied to space-age things or commercial airliners. But very often the basic support that came from the military organizations like ONR, AFOSR certainly had behind it the impetus of developing theory that would be useful, could be extended and not give away the details of the systems. Yeah. So that really was the whole idea.

Again in the *Journal of the Franklin Institute* I had a paper on programming for maximum range and in the *American Journal of Physics*, optimal payload ratio relation for multiple stage rockets. So there were outlets in more general journals and they were maybe even more interested in the physics of the system such as the *American Journal of Physics*. Then there was a journal which still exists called *Jet Propulsion* where I wrote a paper on the vibrations of variable-mass systems. Then again the *Journal of British Interplanetary Society*. And then the paper that is again both still quoted and which actually got me in touch with Russian colleagues was a paper that I published in 1959. So I was still an assistant professor when I wrote the paper because I was an assistant professor from '57 to '59 and then got promoted to associate professor in '59, called "On the Class of Variational Problems in Rocket Flight" and that was in the *Journal of the Aerospace Sciences*, which still exists, in '59.

The first paper that I presented at an international meeting was actually in '59 in London, which has a little side story to it. It's called "Some Remarks on the Optimum Operation of a Nuclear Rocket", which really meant a power limited rather than a thrust limited rocket. But there was no objection to that. The little side story is that being sort of a freshman in the field I gave a twenty-minute paper, well half-hour, twenty minutes plus discussion, in a large classroom and then the important papers which were plenary papers were in a very large lecture hall, sometimes seating hundreds of students. What happened was that I was all set up to give my paper when suddenly there was this rush of people into the room. Finally they sat on the steps. And I couldn't figure out why. Then I found out that Theodore von Kármán had decided he was going to come to my little paper. Maybe he was interested, it had nuclear rocket in the title, or something like that. And when that was pointed out to me, I was very nervous. Had a huge audience. I must have had three, four hundred people in that classroom. They were standing in the hallways. And he was very nice. He raised his hand during the question-and-answer period and said he wanted to say something. And I said, "oh, here I'm in trouble." I forget now the details but all it was that he wanted me to call a certain equation by somebody else's name. He said, "I really like your paper," he said, "but I have a suggestion." I guess it was a historical note. He knew that somebody had done this earlier or

something. I forget now. So that was my first experience, direct experience with an icon. Tsien, of course, we had talked about, the Chinese scientist at JPL. He was really the first direct person of international iconic stature.

Maybe it's also a good idea to mention who people were with whom I became involved in professionally, even if only through correspondence, as in Lawden's case.

06-00:38:40

Burnett: Well, maybe before—

06-00:38:41

Leitmann: Yeah, yeah. Go ahead.

06-00:38:41

Burnett: Just for some context. You may have been a freshman in this field but you were a freshman in a field that was fresh, in the sense that a lot of this stuff is new or it is so new that people had been working in different silos, not realizing that they were effectively doing the same work.

06-00:39:05

Leitmann: Right.

06-00:39:07

Burnett: And that's something that you actually devote a volume to. I'm getting ahead of ourselves.

06-00:39:12

Leitmann: Yeah. Well, that came a little later. But yeah.

06-00:39:13

Burnett: But that's the context. This is all so fresh and you are right in the center of that new work.

06-00:39:22

Leitmann: Yeah. Again, how lucky can you get? I could have started to work in Sanskrit. No, I marvel actually at the good fate. No question about it. So another person that I became acquainted through his almost the same time publications, and we became very good friends, was Angelo Miele, who was an Italian, educated at the University of Rome who came to the United States after he had spent four or five years in Argentina after the war. He had got his degree in the University of Rome. His professor was a man by the name of Cicala, who was a very fine applied mathematician. He came to the States I think in the early fifties, '52, and started publishing pretty much in the same field— as I did, although we never, ever wrote a paper together. But we started to correspond. He was a timely correspondent. I think he was a professor at Purdue. He later moved, in the early sixties, to Boeing, and then back to academia. Stayed at Rice University for the rest of his life. He died about three years ago. Angelo Miele.

06-00:41:08

Burnett: I don't want to interrupt your flow.

06-00:41:11

Leitmann: No, no, no. That's fine.

06-00:41:12

Burnett: It occurred to me, thinking about science in the 1950s and sixties, and of course physics is the queen of the sciences. Chemistry is the big science and big science means capital intensive, very expensive research programs. And it occurred to me just how international, right out of the gate, this research, new burgeoning research area is. Immediately there are these international conferences. And it occurred to me how participatory it was because you didn't have to have expensive equipment necessarily to think through the mathematics and you had old traditional highly accomplished mathematics departments in places such as Italy, the Soviet Union, these other places. So is that part of the, no pun intended, equation in why this research community looks the way it does? It's so international.

06-00:42:21

Leitmann: Well, certainly true, particularly when it got to be space flight, that was something that everybody was interested in. I remember when I later on in '69 taught a semester in Argentina, University of Buenos Aires, the excitement. That was the year of the moon landing. People were more excited there than in the United States. I remember people sitting around television sets watching this. It was really something. So that is certainly true. Now, there were presentations, even at those meetings, of an experimental nature. People could talk about launchings and that kind of thing. Now people who worked on propulsion, interior ballistics, did more experimental work. They also did, of course, the theory. So there was a little bit of experimental work even for the exterior ballisticians. For example, when I was at China Lake I was involved with maybe fifty other people in running experiments launching rockets. But that very rarely got reported at meetings. Partly because it was restricted information, partly it just doesn't lend itself so readily to equations. So it's very fortuitous. Certainly, again, I say it's part of that whole thing that I sort of fell into. And the other thing, which is really, again, an aside, is I came to Berkeley as an assistant professor, either step two or three because I'd had prior work, of course, at China Lake. And I still remember exactly what my salary was. It was \$7,200 a year, which was just about half what my salary had been at China Lake when I left. I was a GS-12. And it's the best thing that happened to me. The only thing is we ate a lot of casseroles for a while. [laughter] So this also being sort of the burgeoning space age, gave me an opportunity to make a little bit of extra money by talking to organizations. I was invited all the way to Las Vegas once to a meeting of—I don't know what it was but it was not a scientific organization. I had to wear a tuxedo, I remember. And they paid me well for the trip and all that. And even more so, I became—I forget about the title—research scientist, I guess, at Lockheed, already in fifty-eight? Let me see, it's probably on here. Staff scientist in the

applied mechanics department at Lockheed Missiles and Space Company. I was there from '58 through '64 every Wednesday and in the summer during the week. And that salary essentially was as high as what I got from the university.

06-00:46:01

Burnett:

Oh, my goodness. Wow.

06-00:46:03

Leitmann:

Yeah, yeah. Particularly since the summers were included in this. And it paid for going to meetings, in addition. And I had a colleague who was in continuum mechanics in my department, Paul Naghdi, a very highly regarded one, who was also in that department but working on continuum mechanics. And we very often drove down together. It was always Wednesday.

06-00:46:29

Burnett:

And where were the Lockheed facilities?

06-00:46:32

Leitmann:

In Palo Alto. Yeah, they were right next to Stanford. So that really more than made up the loss in salary. And that really wasn't rectified for anybody until we got a Republican governor, Deukmejian, in the early seventies. Our current governor, who was then before that governor, his feeling was that professors should take a cut in salary because they get psychic satisfaction from their work. I still remember that.

06-00:47:11

Burnett:

I think you've quoted Governor Brown on that before, haven't you?

06-00:47:15

Leitmann:

Yeah, sure.

06-00:47:15

Burnett:

[laughter] Psychic satisfaction.

06-00:47:17

Leitmann:

Well, in a way it's true because it's exactly what I experienced. I certainly wasn't unhappy in taking a salary cut. So it was terrific. And actually, it didn't really impact my life. Even first four years, from '57 through '61, we lived in El Cerrito. We had bought a house. And in those days you could get a very nice house for twenty-two, twenty-three thousand dollars. That was already a pretty good house. And I think my mother lent me part of the money for a down payment, which was like \$3,000 or something like that. That, together with being able eventually to move to this location in '61, really didn't impact my—what do we even call it? My living conditions at all. I'm also very grateful for that. And the other nice thing, as long as we are talking about moving here, is we built this house and we moved in Thanksgiving of '61. It only took four months to build this house, by the way. And two of my closest colleagues, first of all, my thesis professor, Werner Goldsmith, lived two blocks from here. And the man who became my best friend in the department,

Professor Reinhardt Rosenberg, whose family had also immigrated to the United States. He was not Jewish. His family had converted a long time before that. His father had been, in fact, the dean of a very important university but the Nuremberg Law still applied. So as far as the Nazis were concerned they were Jewish. So there was an immediate sort of colony here five minutes from campus. And so all this worked out just beautifully. In retrospect I really didn't realize that but I really lucked out again. I can't overstress that. I'm getting to the point where it's time to be grateful and I am. Sometimes to people, sometimes just to circumstances.

06-00:49:58

Burnett: Yeah, yeah. You're thankful.

06-00:50:01

Leitmann: Yeah.

06-00:50:01

Burnett: And in that time were the hills [above UC Berkeley] just beginning to be colonized, as you put it?

06-00:50:08

Leitmann: No, no.

06-00:50:09

Burnett: Professors had been living up here forever?

06-00:50:11

Leitmann: And other people. It was actually the Rosenbergs who found this lot. Almost everything else except for the very large lot up the street which people never wanted to sell. It's like half-an-acre. This was all built up already when I moved up here. Every house that I look at now right out here was already here. Yeah. I think this is the last house that was built, with the possible exception, maybe the one—no, even the one next to us was already here, and above us.

06-00:50:49

Burnett: Well, and things began to fall into place. There's the Lockheed work and you become associate in '59 and then within a few years full. And so there's a pay bump with those. You're able to put yourself on a fairly good footing early in your career. You're still quite young at this point. And so that enables you to focus on the—[laughter]

06-00:51:17

Leitmann: And then in thinking back on what today to me looks almost hard to believe, that I went from assistant professor to full professor in six years, is really partly due, I think, to what I didn't realize until I was on the budget committee at the university. This is why I mentioned looking at the papers that I published between '57 and '61. My first co-authored paper was—I'll get to that in a minute—was in '61 also. So the first twenty-five papers were single author papers. And that was always a problem when we were considering

people for either hire or promotion. Who did what? The co-authors or the guy that was up for a promotion? And for my whole career up to full professor there was no such question to ask because all my papers were single authored. And I think that's again a pure happenstance.

06-00:52:27

Burnett:

Before you finished your dissertation, you had been working in aeroballistics. You were the head of aeroballistics.

06-00:52:38

Leitmann:

Well, yeah. For a part of that time, yeah.

06-00:52:40

Burnett:

For part of that time. But, still, this is before you finished your dissertation. So I think—what was the phrase they used in criminal justice—time served? So you had a certain amount of investment in academic work.

06-00:52:50

Leitmann:

Yeah. I'm sure that was taken into account and they could have appointed me as an associate professor, but they didn't. Didn't want to take that much of a chance. I rushed through my bachelor's and master's degree at Columbia also, so that the total time spent on campus was five years. I don't count the second year at China Lake. And I'm not trying to be cute but it does account for the fact that I'm certainly undereducated, particularly in mathematics. There's no question about it. And that had a consequence that I would take an idea so far and then it was very often a graduate student or a post-doc who knew more mathematics than I did or who introduced me to additional things that really sort of pushed the subject along. So very often the first paper I wrote might have been a note with an idea and then people would come along. This even happened four or five years ago, where I talked about a subject, I will get into it in a minute, and it wasn't really taken up until four or five years ago by, in one case, a friend and colleague at USC with his students. I co-authored a couple of papers with him just as a courtesy to me, I think. So I guess what I'm saying is that the way I pursued a subject was partially that I only could take it so far and the holdup usually was the required mathematics to push it further. And the other one was that I did want to get away from computing because it was such a nasty business when I wrote my dissertation. So I said I'm going to get students who are going to the computing. Or post-docs. And that worked out very well. And then the whole idea of applications very often came from the people that either came as post-docs or visiting scholars or students who had applications already either in mind or working on them and then thought working with me would be mutually a good thing. And it was. This is maybe in a sense what made the fields of my work much broader than it would have been otherwise.

06-00:55:41

Burnett:

You get pulled—

06-00:55:42
Leitmann:

If I had stuck with exterior ballistics I'd be stuck.

06-00:55:47
Burnett:

You get pulled in directions by the graduate students and they come with—

06-00:55:49
Leitmann:

And the post-docs very often.

06-00:55:52
Burnett:

And the post-docs. Right.

06-00:55:52
Leitmann:

Very much so and I'm very grateful. I'll mention a couple of them. I just want to take sort of examples. In fact, the paper number twenty-six, which was in '61, I wrote with a fellow, Stan Ross, whom I met when I was at Lockheed. He had graduated, got his PhD at Stanford after me. Well, about the same time as I got mine here, I guess, and started his own consulting company. And he was a student of Arthur Bryson's, who was very well-known, of course, and highly regarded. We became very dear friends. It was very tragic. As so many cases, he died, maybe in the seventies, very young. Excuse me. So how interconnected on the one hand and how really stochastic these things are is really amazing. Completely unplanned, many of these things. They just happened.

06-00:57:15
Burnett:

Well, let's accept for a moment that it's true that you had an abbreviated mathematics education. You would have liked to have had a more complete education to enable you to go farther. And that later students, graduate students, post-docs, would help you to complete or extend your thinking on things. In the late fifties and early sixties, because you're there at the beginning, you're beginning to get a sense of the scope of a field that is coming into being. Is that fair to say?

06-00:57:52
Leitmann:

Yeah.

06-00:57:52
Burnett:

Or is that too early to make that claim? Let's say by 1962, right?

06-00:57:58
Leitmann:

Well, for example, when I bring in, say, the Russian contributions into optimal control, called now, say Pontryagin and his students, they were mathematicians. And so the mathematics they used to get their results were much more mathematical than what, for example, I did eventually with Blaquièrè, who was a mathematician and a physicist. So, in fact, now it's maybe a good time to bring him in because—

06-00:58:33
Burnett:

Pontryagin, yeah.

06-00:58:35
Leitmann: No, Blaquière—

06-00:58:34
Burnett: Oh, Blaquière.

06-00:58:35
Leitmann: —from the University of Paris because in 1961. In fact, Rosenberg, who lived on the next block, went on sabbatical. And they invited a replacement visiting professor. Maybe Rosenberg even had met Blaquière before because Rosenberg was a big Francophile and he always went to France. And it turned out that, I guess, maybe the hope of Rosenberg's was that he would sort of continue in his footsteps and work on—Rosenberg's main interest from a mathematical point of view was non-linear differential equations. He was a little bit disappointed because as soon as Blaquière came here and I met him and we started talking, it turned out we were just looking at very related ways of optimization. So he started working with me. So these things are not—

06-00:59:45
Burnett: In '61?

06-00:59:47
Leitmann: Yeah, I think it was '61.

06-00:59:48
Burnett: Okay. I have a letter from December '64 from you to Austin *Blaquière* inviting him to participate—

06-01:00:01
Leitmann: In a meeting.

06-01:00:03
Burnett: —in a meeting at Colorado.

06-01:00:04
Leitmann: Oh, by that time we had started to work together. Oh, yeah.

06-01:00:05
Burnett: Okay, okay. I don't know if it was maybe tongue in cheek or something but it was a formal—it's "Dear Professor Blaquière."

06-01:00:15
Leitmann: Yeah. Because it had to be used at his university to give him leave or something like that.

06-01:00:19
Burnett: I see. I was hoping that would be a clue as to when you began your association with him.

06-01:00:26
Leitmann: No, I think I already met Blaquière in '61, early '62 maybe.

06-01:00:32

Burnett:

Wow. So he had come as a—

06-01:00:35

Leitmann:

As a visiting professor to replace Rosenberg, who was on sabbatical. In fact, on sabbatical at the University of Besançon in France. It just sticks in my mind for another reason. Again, fortuitous. Certainly not planned because there was no way to plan that kind of thing. And it was very interesting because, again, Blaquièrè was much better educated because France was always big in mathematics. He had a doctorate in physics, in fact nuclear physics, and a doctorate in mathematics. We also personally got along extremely well from the outset. So it was, again, a combination of things. And we became very close friends. In fact, just to look way ahead, I was to chair a symposium in Vienna many years later, must have been in the nineties, for *Blaquièrè's* seventieth birthday, which turned into a memorial meeting because he died on the way to that meeting of a heart attack.

06-01:01:57

Burnett:

Oh, my goodness.

06-01:01:59

Leitmann:

And we became almost like brothers over those years. We worked together until the late seventies and then he started working on other things. But we, for example, co-authored two books in the sixties. And one in fact was a book in French and the other one was an Academic Press book with one of his students and me. And then we co-edited probably four or five books. In passing I'm going to mention that of the twelve books that I edited or co-edited, I was also co-author of every one. I never edited a book that I didn't also contribute to. So sometimes one or two chapters, sometimes half the book. Again, I think there were five or six real either single or co-authored books and the other ones were edited or co-authored. So that's about, what, seventeen or eighteen books. That again was because I got involved very early in every respect in international things. Now, that may be part because I came from there. There's no question about that. And part because I worked in fields where there was, as you mentioned, a great international component to those. And that goes also for my students and certainly my post-docs. For example, the first two post-docs I had here came from Italy. And, again, since I was an Italian by conception that seemed very appropriate.

06-01:04:16

Burnett:

Do you mention that to them every time? [laughter] Endear you to the Italians?

06-01:04:22

Leitmann:

Yeah. I'm an honorary Italian because—yeah.

06-01:04:25

Burnett:

That's right.

06-01:04:27
Leitmann:

And Nancy did her graduate work in Romance languages, and French and Italian were the main languages. So all this sort of fits together. So that gave this kind of international flavor to really my whole personal life and my career, if you want to call it that. I'm always amazed at the amount of travel that I did. When I looked at it the other day I said, "Was I ever here?" Nancy and I didn't travel together, certainly not internationally, until our kids were old enough they could come with us. For example, my trip to Argentina. Or my mother retired and moved to Berkeley in 1969 and she could sit with them. So before that we traveled separately. I think the second international trip I took was actually to Japan. Must have been '61. Maybe that's in there somewhere.

06-01:05:45
Burnett:

Yeah, it should be in there. Just to summarize, when I went through your travel log, with all your boxes of folders of your trips, it's astonishing how much you traveled from the late 1950s until the 2000s. Multiple, multiple international trips per year. At this time you're going to these conferences but they're often the first annual international conference in X or the second international or the third. These are brand new meetings. I'm an historian so I'm used to the "18th Annual..."

06-01:06:27
Leitmann:

Yeah, right.

06-01:06:30
Burnett:

—right. [laughter] It's a brand new set of fields. And you can tell it's going by different names. Sometimes they refer to very discrete things, sometimes there are different names for some of the same basic sets of questions and techniques. But that's kind of where you're headed. Was it in your interactions with Russians and the Pontryagin circle where you began to realize that people were working in siloed disciplines on the same problems and coming up with similar and in some ways identical—

06-01:07:15
Leitmann:

And sometimes not even realizing that it had been done a hundred years before, maybe not quite as elegantly. After all, there was a time when there were no equations. People used words. But there's another aspect. I don't want to be cynical about this. But certainly in the military applications to rocketry, for example, where there was work done, or the space age, particularly once we got into a contest with the Russians, there was an awful lot of money. So these meetings would take place simply because there were sponsoring agencies which didn't exist before. So I think there is that additional aspect, which is more pragmatic but nonetheless it isn't just the science. I think it's just that there was more money.

06-01:08:09
Burnett:

And I don't want to put words in your mouth but I can't imagine that it—it must have been exciting because it was new, because the relationships were

new. These were brand new people in your life from whom you have so much to learn and they from you.

06-01:08:24

Leitmann:

It was very exciting. Again, I think I was very fortunate to be in this kind of general field, number one, and that there were so many interesting people, in some cases really nice people, with whom I could become real friends, such as Blaqui re, for example. He was never my wife's favorite person. She liked him but he didn't have a sense of humor. I'm sure I told you that story. We were driving along in Paris and Nancy—

06-01:09:04

Burnett:

When he was smoking?

06-01:09:06

Leitmann:

He was smoking, yeah. And Nancy said, "You'd better get a new car. Your ashtray's full." And he said, "No, no, no. A car is very expensive in France." [laughter] And he was serious. I rather liked that kind of thing because it sort of made it different from the way people usually react. They had a three-legged dog, for example, whose name was Hugo. Hugo. He used to carry him upstairs always when they went to bed. So there were all these little things. But on the other hand, when I first met him in the early sixties, they lived in a walk-up apartment house. In Paris there are a lot of these five, six story places you've got to walk up. And they had a toilet where the toilet seat was slightly cracked. You had to be very careful to sit down, not to be pinched. [laughter] Suddenly these things occur to me, ah-ha!

06-01:10:10

Burnett:

Right. But he had quite a mind.

06-01:10:15

Leitmann:

Oh, yes. He was—

06-01:10:16

Burnett:

And it was really stimulating to talk with him.

06-01:10:18

Leitmann:

Absolutely. And we had a very nice system. He very often stayed with us here when he was here for a few weeks at a time downstairs which had been essentially guest quarters. It was Josef's apartment and then it became guest quarters. And he smoked cigars. But the other thing is in the office, in my desk we had a bottle of really good French cognac. And when we thought we had solved something we always had a shot of cognac.

06-01:10:54

Burnett:

Oh, nice.

06-01:10:54

Leitmann:

And very often we hadn't solved it so that—[laughter]

06-01:10:58

Burnett:

In hopes of inspiring the breakthrough. Yes, yes.

06-01:11:01

Leitmann:

Yeah, yeah. I mean there are these little things that really are not part of the field but make the thing maybe more human.

06-01:11:08

Burnett:

I don't know. I think of that as part of scientific practice, maybe not cognac per se but the social interaction, the ability to work with someone closely over years. That's a relationship and I think that that's really vitally important to whether someone collaborates or not.

06-01:11:34

Leitmann:

Yeah. And I think you have to get along. If you don't get along then it probably doesn't work as well. And you're more forgiving. Again, we're a little bit complementary. He knew more mathematics than I did but I was a little bit more oriented towards applications. So very often the examples we picked, say in writing a chapter in our books or something like that, were influenced by that. And also because of the stupid questions I asked in the mathematics, I very often found mistakes. And this is true to this day. People very often, until very recently, worked with me because I was so critical. And here people really know what they're doing. And then I said, "How the hell did you get from here to there?" and then they say, "Uh-oh." So this is one of my contributions that I could make over the years, is that I was a little bit maybe too particular. Some people like it, some people don't like it. We won't push it too far ahead but one of the reasons, because you brought it up, is that I edited what really was probably my first book. I think it was '61. Let's see.

06-01:13:04

Burnett:

Sixty-two, wasn't it?

06-01:13:06

Leitmann:

Sixty-two, maybe.

06-01:13:08

Burnett:

Optimization Techniques?

06-01:13:09

Leitmann:

Yeah, *Optimization Techniques with Applications to Aerospace Systems*, Academic Press. By that time I had met another icon: Richard Bellman. He certainly fits that class. And it brought it back to me when I looked up the other day his birth and death dates, which I knew approximately. First of all, he was a scientist at RAND [Research and Development Corporation]. RAND was the Air Force think tank. And the other person who was in that think tank at the time was another person who was in a sense an icon, with whom I became not as good a friend as maybe with Bellman, was Rufus Isaacs, who was one of the first people to work in what's called differential games. These were all things that came out in the middle sixties. I think Pontryagin's book, the English edition came out I think in '61 or '62; the differential games book

by Rufus Isaacs in '64, '65. So I was very interested, seeing there are so many things going on, I'll put together an edited volume. And *Aerospace* was sort of the come on. It's interesting when I tried to invite people or made up the list to invite people to write for this thing, I picked some people that I knew personally. Ted Edelbaum, who died very early, I think maybe has the first chapter in the book. Miele wrote two chapters. Bellman wrote a chapter. I invited everybody that I thought I could find and essentially had some contact to aerospace or applications. The one person who turned me down was in fact Arthur Bryson at Stanford, because he wanted to know who else was going to be in this book. And he said, "No, I don't think I want to be associated with some of those people." And it's too bad because he was a first-rate guy. But it's interesting.

06-01:16:04

Burnett: Do you have any inkling as to why?

06-01:16:05

Leitmann: No, he just didn't think much of them. For example, I don't think he thought much of Bellman.

06-01:16:12

Burnett: Interesting.

06-01:16:14

Leitmann: But he never told me who the people were. He simply said, "There are some people I'd rather not be associated with." It's a free country. So I got to know people. For example, one of the first people I met in the Soviet Union, and I think my first trip must have been '63, '64, something like that. There's a conference that you mention here. Was it Optimal Control? No, Pontryagin came to the States first time in '64.

06-01:16:53

Burnett: Oh, the Second All-Union Congress on Theoretical and Applied Mechanics?

06-01:16:55

Leitmann: That's it. So that was '64.

06-01:16:58

Burnett: Yeah, February. Early February.

06-01:16:58

Leitmann: Right. So [A.I.] Lurie was a really highly regarded, very famous mathematician in Leningrad. I met him on that trip when I went to Russia. In fact, I still remember that I got a whole bucket full of—what's the stuff that the fish have?

06-01:17:29

Burnett: Caviar?

06-01:17:30
Leitmann:

Caviar.

06-01:17:32
Burnett:

Was it good?

06-01:17:34
Leitmann:

It wasn't the best but in the Soviet Union that wasn't so easy. Awfully nice man. And, again, already in those days some intimation of anti-Semitism in the Soviet Union. Stalin, you remember, started that whole thing with the doctors plot before he died. You know that whole thing? Lurie certainly was more than qualified to be a full member of the Soviet Academy of Sciences. He never made it beyond corresponding member. He had a son and a daughter who was a ballerina. And a wife. He was another icon that I met. And then his son Konstantin was able to emigrate in the early seventies. So Bellman and I were sort of sponsors of the famous Lurie's son, who was a mathematician essentially in partial differential equations, was his major field. And we got him a job. Got him. We recommended him to Worcester Polytechnic Institute and he's there to this day as a really senior professor and we are still very close, the son and I. They came to visit us a few years—and the daughter—I think his sister died. But, again, a contact that is partially predicated on the field and partially simply on my general interest, or whatever you call it, in matters international because it meant so much to me. I don't know how this all happened because at the time it wasn't really by design. It may have been inspired by but there was certainly no plan for me to work on an international kind of platform. Really not.

06-01:20:02
Burnett:

You mentioned earlier a self-deprecating description of asking dumb questions. But I do want to get a handle on that because I think there is something special. You have a quasi-insider status. You're kind of on the periphery of these different domains. You aren't a full mathematician by your own lights.

06-01:20:33
Leitmann:

Nowhere near it.

06-01:20:32
Burnett:

Right, okay. But you were part of a theoretical division of an extremely applied rocket division.

06-01:20:44
Leitmann:

Yeah, at Lockheed, right.

06-01:20:44
Burnett:

Right. At Lockheed but also the aeroballistics program at China Lake.

06-01:20:48
Leitmann:

Oh, yeah, yeah.

06-01:20:51

Burnett:

So you're at the most theoretical end of applied engineering institutions and projects?

06-01:20:59

Leitmann:

Yeah. This may be partly a psychological problem because I've been accused of being pedantic. And that's true because I'm often puzzled and then wondering how did somebody get from here to there simply because I want to know. And sometimes that has a positive effect on the overall performance when that happens. So I was sort of just a pain in the ass for people who—but also appreciated simply for that because people wanted to do something correctly and sometimes things just get skipped or people think of arguments that say well, we don't have to look too closely or everybody knows kind of thing.

06-01:22:04

Burnett:

Well, I'm trying to think of other domains. I wonder if you are somewhat multilingual or multicultural when it comes to science and engineering. You've been in these different domains. You've talked to different kinds of thinkers. And so when you come to a mathematician who's working in a particular silo in a particular domain with a particular language, you come having talked to different people with different languages and you come to them and say, "Well, what does it look like when you turn it this way?"

06-01:22:35

Leitmann:

Yeah. I think that's partially true. I don't know how vital it is. But that's certainly an aspect of it. I'm pedantic in other ways, too, so it's not just in science. I try to be organized. I don't know if pedantic is the word.

06-01:23:01

Burnett:

Well, let's pursue that. So pedantic is someone who parades their knowledge, right, someone who is demonstrating—

06-01:23:08

Leitmann:

Well, critical. Yeah. Critical.

06-01:23:11

Burnett:

Demonstrate. And critical.

06-01:23:12

Leitmann:

Yeah. I really meant more in the sense—not that I'm trying to show off but I'm simply demanding as much rigor as I can hope for. I want people to be rigorous. And that was one of the ways that I always somehow got into an argument with Arthur Bryson, because he thought I was always looking for these special crazy cases, wasting a lot of time getting sufficiency when necessary conditions are enough. Not that he didn't know this because he was a member of the National Academy of Engineering and Science, certainly a very bright person. It's very interesting. Well, he wrote two books. His well-known book that he wrote with Larry Ho at Harvard an early optimal control book about the same time, in the middle sixties, '66, or something like that. He

does in his foreword thank five or six people with whom he has interacted and I happen to be one of them. But the interesting thing is his name is Arthur E. Bryson, Rudy E. Kalman and others with the middle initial E. He gave me a middle initial E. I was the only one who didn't have a middle—[laughter]

06-01:24:51

Burnett:

So he gave you one just to—

06-01:24:53

Leitmann:

George E. Leitmann. [laughter]

06-01:24:59

Burnett:

For engineering, I guess.

06-01:24:59

Leitmann:

And, as I say, he was very nice. He mentioned me in the foreword and he didn't have to do that because I had nothing to do with his book. It's a strange life, I guess.

06-01:25:15

Burnett:

I look at this book and you write the foreword and the foreword is extremely compelling because it really distills a lot of what we've been talking about for the last two sessions about optimization—

06-01:25:25

Leitmann:

Optimization techniques, yeah.

06-01:25:27

Burnett:

And when I look at it and I look at the people you've brought together and I see it's people from companies, private companies, it's people from RAND, from Los Alamos. You've got national labs; you've got universities; you've got laboratories. So you don't discriminate or you don't think this has to be an exclusively mathematicians conference or this and that. You're thinking about where can I find interesting people to pull all this together? So are you also perhaps an impresario? You're someone who brings people together. Obviously you do because you put together an edited volume and you've done that many times over and you've done that with conferences, too. You've put together conferences.

06-01:26:22

Leitmann:

Yeah. Well, I think that that's partially true. And, in fact, it's even true in Nancy's and my private life. We have many people who became friends by being brought together by us. There's a large number of people, a lot of them are dead already, but people very often mention that. They say, "You and Nancy are the people who got us together." We had wonderful dinners here, particularly in the seventies. Nancy is a great cook and baker and very gracious in general. Her contribution other than making me happy was that it brought a lot of people together and that doesn't hurt. It's not a planned effort but that's the way it works out. Yeah.

06-01:27:21

Burnett:

It's a personality thing and couples are like that, too, if they're just kind of welcoming people.

06-01:27:28

Leitmann:

Well, I just enjoy people. I really do. And that's why I eat so slowly. It drives people crazy sometimes. [laughter] A former professor at Berkeley said to me the other day, "You know, George, you talk too much," and then she added gratuitously, "about yourself." But she's one of those acerbic people. Used to be a German professor.

06-01:28:00

Burnett:

Well, that may be my fault because I'm training you to talk about yourself more. So this is my influence.

06-01:28:07

Leitmann:

Whatever, whatever. She noticed that.

06-01:28:10

Burnett:

[laughter] Right. So in this foreword for optimization techniques, you outline a couple of basic problems. When you're trying to design some kind of system you have to identify the goal and sometimes there are multiple goals and often there are tradeoffs between the goals. That's a really extremely elegant description for what all of this is. And then there's a second set of problems, which is what's the right technique to decide among those things that need to be—

06-01:28:53

Leitmann:

For that problem, yeah.

06-01:28:54

Burnett:

For that problem that need to be traded off. And this project you had identified that there were different "schools" of optimization that have gone by different names. And in one sense you said the schools just used different language to describe similar or even identical methods. So I was really, really intrigued by that. And you have alluded to that in our previous conversations in previous sessions. And so that was already evident before you set this up. This volume was to demonstrate that. You already knew. And you had to be one of those people, not necessarily the only one, but one of those people who really knew that there were these different schools. Or was that the immediate subject of conversation amongst everyone? Was that really clear?

06-01:30:00

Leitmann:

No, no. There were no other people planning this volume. That I know. It was partially maybe because I wanted to know about these fields and I thought that I would have to read this stuff now and that sort of forced me into doing this. It also was a little bit of an effort, maybe, to demonstrate that many of these things were done earlier or in a related way and this—having to do with, say, classical calculus variations vis-à-vis the maximum principle, vis-à-vis the more numerical methods like dynamic programming. Later on you will see

that this led to really interesting confrontations, even at meetings. And, again people sort of motivated by what they wanted to do rather than just to do the theory or just the application. And I always found that very interesting. I think it's also through a realization that became more and more obvious that I don't know very much and maybe this is one way for me to learn something in a forced way. Again, probably not deeply enough but nonetheless at least get me acquainted with methods. And then it allowed me to meet a lot of interesting people, of course, which to me is always a major aspect of doing things because it's a rare thing actually to be able to meet interesting people because usually it gets to be small talk, small talk conversation. That's very nice at a cocktail party but if you're more serious about it then that gets to be an awful waste of time and we don't have a lot. So it's not all unselfish, let's put it this way.

06-01:32:28

Burnett:

And maybe it was required because there may have been other older methods of doing things and there are different ways of slicing it up. It could be dynamic programming, could be calculus of variations, could be the Pontryagin maximum principle. But you were witnessing the development of a new field or set of fields. And in order to do that you had to look broadly. You had to think broadly.

06-01:33:10

Leitmann:

I think that's true, although I'm not sure that I was aware of that when I did it. Maybe it just happened because that's the way it happened. But I'm not sure that I planned this in any kind of rigorous way.

06-01:33:30

Burnett:

No, but you sensed an absence. There's a couple of things. One is there's too many people doing the same or similar things and they're not talking to one another.

06-01:33:43

Leitmann:

Well, that was certainly my point, is that this is one of the major problems I have and it's partly simply because of the volume of stuff that is being done, that there is a great lack of communication into neighboring or overlapping fields today because everybody becomes a specialist today and it's true in medicine certainly. But I think it's also true academically. More and more specialized courses. If you look at the curriculum, specialization is immense. Of course, given the volume of knowledge it's become almost impossible to be a Renaissance man. There are very few of those around. I think it's almost impossible. The other thing is the international aspect is really not to a minor extent because of my experience, again, if we're going to have the world go on we'd better go beyond just looking at science. And I feel that way about sports. It is a nationalist kind of endeavor, we're better than you guys. And I think that's very bad. It really bothers me because we're in such dire straits and it seems to me ever more so, just because people want to be in smaller and smaller groups. More little countries. As I mentioned to you, we exchanged

email, that I've been reading sort of a history, recent history of the Balkans, which is sort of a—you can always call it Balkanization of the world. I think culturally that's terrific but when it gets to be really a matter of national pride it bothers me because it seems to me a criterion which is really minor.

06-01:35:59

Burnett:

And so there's an element of science diplomacy at work in the—not you personally necessarily but that was certainly happening at that time.

06-01:36:06

Leitmann:

Yeah. Oh, yeah.

06-01:36:10

Burnett:

That this was bringing people together, the community of nations and science was the original sort of communalist or sort of communist endeavor.

06-01:36:19

Leitmann:

Yeah. This kind of thing started, of course, already in the nineteenth century. For example, Alexander von Humboldt was a prime example of that. He and Jefferson got to be very good friends. Must have been by mail. I don't even know if they met in person. I have to look that up.

06-01:36:42

Burnett:

I'm not sure. Yeah.

06-01:36:43

Leitmann:

But that really is something that interested me, that one could go beyond one's boundaries and gain an awful lot. Maybe that's one of the reasons I like to travel so much. And then I find that people are very much the same everywhere basically, which is again something that gets to be more and more lacking in our view of the world. And I think it's terrible. It's really bad.

06-01:37:21

Burnett:

As we've said already several times, there's a social dimension to this work. There's a human dimension to this. Yeah.

06-01:37:32

Leitmann:

Yeah, yeah.

06-01:37:35

Burnett:

Absolutely. So you contacted these people. Who do we have here? Richard Bellman? Robert Kalaba?

06-01:37:47

Leitmann:

Yeah. Well, Bellman and Kalaba used to work together but then they had a fight. But Kalaba, on the other hand, became a collaborator of my good friend at USC, with whom I've written a few papers, Firdaus Udwardia. In fact, we exchange certainly minimally emails three times a week. Poor man, actually he's only seventy years old and he's got a heart problem that will kill him very soon. In fact, I had email from him this morning. And he and Kalaba wrote

one of the first treatments of really a new way of classical mechanics. Started a whole new field. So all these people somehow touch somewhere and it's interesting.

06-01:38:50

Burnett:

Could you talk a little bit about Richard Bellman?

06-01:38:56

Leitmann:

Well, yeah. He started the *Journal of Mathematical Analysis and Applications* in the early sixties, '61, '62. Well, this is just about the time when that optimization technique book was being planned and it was planned for a series that he edited for Academic Press. So that's really probably how we first met. So it was just about '61, something like that obviously.

06-01:39:34

Burnett:

It was supposed to be volume one of a series?

06-01:39:37

Leitmann:

No, no. It wasn't volume one. But the series was a long series already.

06-01:39:40

Burnett:

Oh, I see. Okay.

06-01:39:42

Leitmann:

No, no. It was maybe a volume twenty-eight or something like that. But it was called *Mathematics for Science and Engineering*. Academic Press. So I think that was my first direct contact with him. Then when he started the journal he invited me to become an associate editor. He was what people would call a different person in the sense that—he told me later on why he did that. He was very often, if not insulting, at least challenging in his remarks in talking to people because he thought that would really get a good conversation going.

06-01:40:31

Burnett:

An icebreaker.

06-01:40:31

Leitmann:

And it's true. It's true. But he was a remarkable man in many ways. For example, he was diagnosed with brain cancer I think in 1972 and died ten or eleven years later. During that interim period, those ten or eleven years, working with a brain tumor that was operated on but really not completely, he wrote over a hundred papers and six books.

06-01:41:06

Burnett:

Amazing.

06-01:41:05

Leitmann:

In total he wrote 650 or 660 papers and forty-seven books. So he was a remarkable person. He had a way of doing that because he never proofread anything. He submitted it. He never re-read either a book or the paper. It either got accepted or not. He was very bright. His thesis professor was a very

famous mathematician, Lefschetz at Princeton. So he was obviously—call him a genius. I don't know, whatever you want to call it. A lot of people hated him because he was very acerbic. He told me later on that it just makes for interesting conversations. And then when he died, I think in '81 or '82, the journal had no editor. He ran this journal out of his back pocket at USC. He was a professor of mathematics, electrical engineering, medicine, and something else. So he had a quadruple professorship. My friend Firdaus Udawadia has the same thing. He has four titled professorships. So anyway, his widow found among his papers a little note saying, "After I die I want Leitmann to be the editor." Well, I was stunned because he knew I wasn't a mathematician. I knew it. And I thought about it a couple of months and then I accepted with the stipulation that I would only do it if there were co-editors, one a true mathematician, and I. And I did that for sixteen years. I was a co-editor for sixteen years, until early 2004, 2003, or something like that. So it was interesting. Just to give you an idea how unconventional he was, he and Nina went to dinner with us at the Blue Fox, which was at that time the best restaurant in San Francisco. It must have been the middle seventies, something like that. And those were the days when you had to wear a coat and a tie. He arrived in khaki pants, a sports shirt, a raincoat, and they almost didn't seat us. I think maybe they brought him a tie. I'm not sure. And they seated us close to the kitchen I remember. So he was always tickling the tiger's tail.

06-01:43:43

Burnett: Of course.

06-01:43:43

Leitmann: Maybe knowing that he was going to die very soon was first of all an impetus for really producing. So this volume of writing was no doubt—no doubt—I suspect partly that. He was a very good tennis player. And while he was dying of this brain tumor they lowered him in his swimming pool with a winch. So he was a strong personality, no question about it.

06-01:44:12

Burnett: Goodness.

06-01:44:14

Leitmann: But not likable.

06-01:44:14

Burnett: Yeah, yeah. He wasn't so concerned with that; he was concerned with the—

06-01:44:18

Leitmann: He couldn't care less. That's right.

06-01:44:20

Burnett: Right, right. Well, so he and Robert Kalaba were from RAND. Derek Lawden we already talked about, from Canterbury University, New Zealand. [Robert W.] Bussard from Los Alamos.

- 06-01:44:35
Leitmann: Yeah. I don't really remember where they came from. Yeah.
- 06-01:44:36
Burnett: So he's from Los Alamos National Lab.
- 06-01:44:38
Leitmann: Yeah. He was not a friend or anything. I just picked him from his references.
- 06-01:44:45
Burnett: And then four contributors from the aerospace military industries. So two from Grumman, one from Lockheed, one from Boeing, and one from General Electric.
- 06-01:45:00
Leitmann: Yeah. Kelley, I don't know who his co-authors were, but he was, let's see—I don't know whether by that time he had his own company. Yeah. He was with a space company. I've forgotten which—or aircraft company Kelley. Later started his own consulting firm called Dynamics Associates or something like that. Ted Edelbaum I think was from a company. He wasn't an academic.
- 06-01:45:44
Burnett: United Aircraft Corporation.
- 06-01:45:46
Leitmann: Yeah, yeah. See, I remember that. Personally a very fine person and certainly extremely good.
- 06-01:45:47
Burnett: He wrote on necessary and sufficient conditions. Is that what you were talking about in the last session?
- 06-01:46:01
Leitmann: Yeah. But I think he was really dealing with static problems in that field, not dynamic problems. Yeah. And he was again very much a careful person. Let me put it this way. Then there was Miele. Now, he was an academic but eventually he was at Boeing. So at that time he wasn't at Boeing yet. Who were the other people from industry?
- 06-01:46:37
Burnett: Well, I think the only point I wanted to make was the wide range. You pull people in from—obviously this is ballistics, but people who are thinking about problems and could speak to different aspects of the problems of describing an optimal system. And then there are the academics, as well. So national labs, universities, and private industry. That perhaps in that area would not be so uncommon because—
- 06-01:47:12
Leitmann: No, no. Because those were places where they'd work in that field.

06-01:47:17

Burnett:

Exactly, exactly. But I thought it spoke to the kind of—what's the adjective—catholic, ecumenical—bringing people in from different domains to talk about these ranges or problems. And then the year after, you were made full professor. So that must have been a good confirmation that you're on the right path. [laughter]

06-01:47:51

Leitmann:

I'm not sure how surprised I was. Maybe not as surprised as I am now, frankly, because I had more exposure to the academic field, which at that time was very small, of course. I'd been here, what, seven years or something like that and I hadn't yet served on the academic senate committees. I got exposed to, really, academia that way. So yeah. Was it a couple of years later I was invited to write a textbook on optimal control by McGraw-Hill.

06-01:48:31

Burnett:

Yeah, sixty—

06-01:48:32

Leitmann:

And there was a reason for that. They had accepted a book by Athans and Falb. The initial textbook had eleven hundred pages and they wanted something under two hundred pages just to set it off. So by that time, of course, I had started to discuss a geometric method for optimal control with Blaqui re and we had already written a few papers on it. Maybe we had already written or were writing the two books that we co-authored, one with one of his students. And we met a lot in France and in addition to the sabbatical he had that he was here, he spent time here very often. Somehow these things always sort of came together without a basic plan.

06-01:49:53

Burnett:

But you were relieved in a sense to be leaving the narrower domain of aeroballistics because—

06-01:50:00

Leitmann:

Oh, yeah. Definitely.

06-01:50:01

Burnett:

—in the interview in *Spaceflight*—when was that? Sixty-nine or later? You're quoted as saying, "I left because of—" you used the Latin for it but *panem et*—

06-01:50:14

Leitmann:

Circenses. Bread and circuses.

06-01:50:18

Burnett:

Bread and circuses of the space flight world. You said there was an aura of bread and circuses around it. It was the center of popular culture. There was lots of money being thrown at it. There was obviously the military-industrial complex involved.

06-01:50:43
Leitmann:

Yeah, absolutely.

06-01:50:43
Burnett:

Was that part of it? It had become a bit overwhelming for you?

06-01:50:48
Leitmann:

I don't know if I was so conscious of it. I think it was really more by exposure to other people that I broadened things rather than by saying I've got to get out of this.

06-01:51:01
Burnett:

Right. It wasn't a negative.

06-01:51:03
Leitmann:

I'm sure it wasn't that. Yeah. No, I think I was very fortunate with students, with post-docs, with colleagues. And it's interesting because I have very few collaborations with people in my own department. Roberto Horowitz was the chair of mechanical engineering for a number of years now. He and a couple of his students, we have maybe three or four papers together. I can't think of any other—

06-01:51:38
Burnett:

What about Lotfi Zadeh?

06-01:51:41
Leitmann:

No, I never worked with him. We talked a lot but no—

06-01:51:46
Burnett:

Yeah. And he came to some conferences. He gave papers at the same—yeah.

06-01:51:48
Leitmann:

Oh, yeah. Right, right. Also, he chaired the session I was in at a meeting in Los Angeles. Just as an anecdote, aside, and he was already old enough—although we weren't that much different. We were only about two or three years apart. So this must have been fairly recently. Probably in the late nineties maybe. It was a meeting at USC, I think, and he was the chair of a session where I gave a paper and they were fifty-minute papers. And after about, I don't know, ten minutes he started buzzing, which means your time is up. What happened was he had fallen asleep so he figured he better stop me because who knows what time it is. He was very apologetic later and they let me do the whole paper over again in another session the next day. [laughter] So these are little anecdotes.

06-01:53:06
Burnett:

That is hilarious. Well, yeah. Now I've lost my train of thought. You were being drawn to the world of optimal control, which was already expanding into all of these different domains. There are already people working in different domains. That's partly how you come to be aware of it. And so that becomes the promising field that you carry forward. I guess to ask a kind of

final question: I've already noted that there were many different conferences beginning, automatic control, optimal control, control optimization—basically they're all on the same theme. Do you get a sense of the discipline coalescing? Because there's the *Journal of Control Optimization*, isn't there? There's a *Journal of Optimal Control*.

06-01:54:15

Leitmann:

There is a myriad of journals. When I started there were maybe three or four. There must be twenty or thirty, maybe more journals, and in addition you're going to practically write your own journal now by going to India, that kind of thing. In a sense, it's too much because—well, just give you an idea how the whole publishing thing went. When Bellman ran the journal he ran it with just a secretary. He had no staff at USC. He accepted papers personally. He had associate editors but generally he had the last word. And there were twelve issues a year, each one maybe 150 pages. When I took over as co-editor of the journal after Bellman died, a year after he died, there was a mathematician, Boas, who was my co-editor for only three years and he died, and then my co-editor was the chair of the math department at Georgia Tech, Bill Ames. And we did that together for sixteen years. When I quit he carried on for another two or three years. That journal grew from twelve issues a year of 150 pages. We went to 24 issues within a year after I took over. But there was still only about 150 pages or so. Today it is twenty-four issues of three to four hundred pages every issue, twice a month. It has three editors-in-chief, twelve regional editors, and over a hundred associate editors.

06-01:56:36

Burnett:

Oh, my goodness.

06-01:56:38

Leitmann:

Yeah. There are two of them, telephone books, every month.

06-01:56:47

Burnett:

So a proliferation of journals, a proliferation of scholars working in those fields. It grows exponentially in—

06-01:56:54

Leitmann:

Yeah. Just look at China, almost no papers from China until about twenty years ago. Now half the papers that are submitted come from China. And they're good enough to be accepted.

06-01:57:08

Burnett:

Right, sure.

06-01:57:10

Leitmann:

Very interesting. The whole field of engineering and mathematics, applied mathematics, has grown tremendously because of more and more specialization. The concomitant of that is the poor guy who writes a dissertation now, he has no time to go to the history of the field because if he did that it'd be just five years preparation. And the loss of that in turn is

tremendous because things are sometimes not only done over again but not as well when you really delve into it, as they were a hundred years ago.

06-01:57:57

Burnett:

With computing and data storage and increased communications we're becoming a confessional society, where we talk and communicate a lot more than we listen and read because we're producing so much knowledge.

06-01:58:14

Leitmann:

Yeah, that's right. That's right. And that increases the problem, of course, because it's certainly exponential. Fairly steep exponential. If I just think of even teaching, we haven't talked about my teaching or getting into just the teaching end of it, I started giving the basic statics and dynamics course. I introduced a graduate course in optimal control. There was no such thing here, not even in electrical engineering. Even in the math department there was really no special course in the calculus of variations. It was just another field.

06-01:59:10

Burnett:

And so you did? You did teach—

06-01:59:16

Leitmann:

I didn't teach the method but taught—well, I taught a course which was calculus of variations and optimal control sort of together. Right. So the first little book I wrote, which was a counterpoint to that thousand page thing, I did get it in under two hundred pages.

06-01:59:35

Burnett:

Oh, the counterpoint to Athans and Falb.

06-01:59:37

Leitmann:

Yeah. But I was lucky that I could use this geometric method that Blaquièrè was beginning to develop, which in turn is very close to dynamic programming except it's much more geometrical. And allowed me to do, which would have taken much longer by, say, what Pontryagin did. It really is interesting because this is a book, middle sixties, which I'm very often reminded of because people would either contact me. I think one of the more recent ones was Dilmurat Azimov, an engineer/mathematician, applied mathematician who teaches at the University of Hawaii. Originally from Uzbekistan but educated in Russia. That was still the Soviet Union. He's probably a man in his forties. Who came to see me, contacted me and came to see me a couple of years ago. Made a special trip from Hawaii to here. It was only for three days. Because he said that when he was a student—I forget now. He was at a major university. It wasn't Moscow but I think a major university. His teacher recommended that book, that little two hundred page book. And he said that was his introduction to optimal control.

06-02:01:27

Burnett:

So that speaks to you. That's more, I think, evidence or suggestions, at any rate, that you are thinking like an insider/outsider. You're thinking about

breaking down the silos and like how can you make—instead of an 1100-page catalogue of optimal control theory, what does a two hundred page—you think in terms of examples, real examples that illustrate what is happening?

06-02:01:58

Leitmann: No. That book was primarily theory.

06-02:02:00

Burnett: Oh, wow. Okay.

06-02:02:02

Leitmann: Oh, no, no. My book and the Athans and Falb book was very heavy on theory, but mostly for linear systems. It's very interesting. It finally got whittled down to about 750 pages when they published it. I really don't know how conscious these things are. In retrospect you could say, "Ah-ha. This follows from that." This wasn't a planned development. It was the only book that had a very large number of copies sold because there were very few books at the time for that field. It was in the many thousands. Today if you write a book in that field, if you sell three hundred of them you're doing pretty well because everything is so specialized. So that was nice. I could still use more money at the time. And it went through either three or four editions. It's interesting because, as I say, I still get people, usually from strange places or not highly developed places, that somehow hit that early, that book either because it was recommended to them by a teacher or because they found it in the library or whatever.

06-02:03:35

Burnett: Is it because there's a low barrier to entry thing, as well? Because you mentioned Uzbekistan, and it's translated in the Soviet Union. It's translated into Russian. Or not it but *Optimization Techniques*, your first volume.

06-02:03:49

Leitmann: No, no. That little book was translated into Russian.

06-02:03:51

Burnett: Okay. But in '65 *Optimization Techniques* is translated.

06-02:03:58

Leitmann: Yeah, but only Russian, not other foreign languages.

06-02:04:00

Burnett: Oh, okay. Interesting, interesting.

06-02:04:01

Leitmann: No, no. Because almost everybody that time accepted English as the *lingua franca*.

06-02:04:08

Burnett: Right, for engineering and science.

06-02:04:10

Leitmann: Whereas in the Soviet Union they felt they were sort of standing by themselves at the time.

06-02:04:13

Burnett: Right. Interesting.

06-02:04:15

Leitmann: That little book was translated into Russian, into Japanese. Beautiful Japanese edition. In Polish and into German where I helped the translator add a chapter in two-person game theory. So that has an extra chapter. Yeah, I think either four or five languages. Yeah. But that's, again, true because this was in the sixties and there are very few books in the field. It's surprising. There were the outstanding ones like the Pontryagin book, which Pontryagin wasn't happy with because they sort of put it together haphazardly.

06-02:05:06

Burnett: I would just speculate. There was a new Nigerian technical university starting and they asked if you could come. You were unable to come to teach a short course. But I do think about barriers to entry and this is very, very sophisticated analysis of very complex systems, but you can do it without a massive research budget. You can do it in a classroom with people. So if you're in a poorer country—

06-02:05:43

Leitmann: Yeah, that's attractive.

06-02:05:44

Burnett: —and you want an educated, mathematically sophisticated elite that is having to deal with large projects, as there are development projects coming in. So it makes sense that it would have this pull in the so-called developing world. I don't know if that's the exact point that you were making.

06-02:06:02

Leitmann: Yeah. Well, for example, in the late sixties when I was invited to teach in Argentina for their winter semester, I think that it was probably that book and maybe the *Optimization Technique* ones that really got them to me. It was an OS professorship and so it was very nice. And furthermore, the deputy minister of science and technology had been a student of Miele but not made it with Miele because they had a big fight. Also, he had six or seven kids. Carlos Cavoti. Hell of a nice guy. Sometimes, and I'm not trying to put myself down, I consider myself a nice guy in a sort of very general sense because I like people. And I wonder if many of the awards I got for being a nice guy. No, no, I'm serious. I don't know.

06-02:07:12

Burnett: Well, if being a nice guy entails bringing people together to work on new problems, helping people, helping people edit journals, helping people with their work, yeah, you can see how that would—

06-02:07:27

Leitmann: It doesn't hurt.

06-02:07:27

Burnett: It doesn't hurt.

06-02:07:29

Leitmann: Well, I give you one example which I think maybe is a particularly egregious one. My first two post-docs were Italians. Giuseppe Basile and Giovanni Marro. I got a lot closer to Basile. They worked with me for a year. They worked in linear systems and so we never wrote a paper together. I did later on, a few years later, accept a paper for JOTA [*Journal of Optimization Theory and Applications*] on what essentially they had been doing when they were with me because I was an associate editor. So that was really my only connection. But Basile essentially became a second son to me over the years. His father was the dean of engineering at the University of Bologna. How much that had to do with my being nominated and elected to the Academy of Science of Bologna, which is one of the world's oldest—I'd not be astounded if he was the nominator. Let's put it this way. I never found that out. Very distinguished family, four brothers, and the only one unmarried until his forties was Beppe. But we got so close that I had a key. His parents owned a big apartment house. I know the address: Viale Pepoli in Bologna. If we went to Bologna we always stayed either in an apartment in the house because they had maybe an empty apartment or had the key to the apartment on the roof. And a terrible ending, however. I saw both Marro and Basile—and we're looking way ahead but that's okay—for my eightieth birthday celebration near Stuttgart. It was the Daimler-Chrysler executive center. Yeah, in 2005. That's the last time. Basile was one of the few real engineers that I ever met. For example, he designed a very sophisticated auto pilot for both boats and planes. He was an excellent pilot himself. In fact, I flew with him from Bologna to Ajaccio in Corsica when he was certifying the autopilot. And that autopilot was being produced for airplanes in the United States by a small company somewhere in Arizona. But he also very often stayed in the house of Rudy Kálmán, who is another icon whom I encountered. Very famous man. He died three years ago. The Kálmán Filter, which is a very sophisticated mathematical tool to deal with uncertain system control, he was another one of these persons like Bellman, not very well-liked because he had a very sharp tongue. Where did I meet him? I guess he invited me. He was chairing a session, first session of international conference on cybernetics in Zurich. And he was the first plenary speaker. They had a plenary speaker, one in the morning, one in the afternoon, and he opened as a plenary speaker in the morning. And immediately proceeded to insult half the audience. We had a lot of people who were interested because of the Kálmán Filter, such a fantastic thing, particularly from Africa. Young people who wanted to train themselves in advanced methods. But most of them, sort of operations, research, and economics, that kind of thing was a more practical thing. They weren't building rockets. And the first thing he said was, "Mathematical economists are idiots," and then tried to prove that by saying they don't even know what's

the dependent and the independent variable, that kind of thing. So that set the tone of the meeting. Then in the afternoon when I gave mine, during the question period he raised his hand. And he was known really to give you a hard time. No. He said, "I see there are not many questions." He was chairing that session. "So I didn't quite finish what I wanted to say this morning. We will use the remaining five minutes and I will tell you more." [laughter] That was Rudy Kálmán.

Or, for example, in 1988, it was the eight hundredth anniversary of the University of Bologna. And they had a special year. Eighty-eight it was. And every month one person got an honorary doctorate. It was Mother Theresa who got it in the first month of that meeting and Rudy Kálmán I think was the tenth or the ninth. And I was walking with him after lunch and he had just won the Tokyo Prize. That's a lot of money. And he very proudly told me, "I accepted it but with the proviso that they will change the money into dollars paying the fee and give it to me in dollars." He was that kind of guy. It got so bad at Harvard, he became a professor at Stanford. And, of course, you couldn't get to his classrooms, lecture halls where people wanted to be, to take Rudy Kálmán's course. Second semester he had thirty people left because he insulted the students. Students would raise their hands and say, "Excuse me, Professor Kálmán, but—" and then ask a question. And his standard answer was, "If you knew some mathematics you wouldn't ask such a stupid question." That was very often his answer. So that was Rudy Kálmán. This is sort of the counterpart to Bellman. Bellman wasn't that crass.

06-02:15:30

Burnett:

Right, right. It is a contrasting case. And I was saying that being nice is kind of a complement to an accomplished career, where you're opening doors to people, you're bringing people together, you're asking new questions and you're facilitating and building, and some of these accomplished people could be jerks. So that's another—[laughter]

06-02:15:54

Leitmann:

Oh. Well, no, of course. I'm not surprised. Those were sort of two prime examples. It's interesting. It's very interesting because you get the whole spectrum of people. I'm sure that's true in every field, of course. Maybe particularly in a technical field. People who want to be completely dedicated to solving practical problems. So, "never mind all the fine points" kind of thing, and then other people where life is the fine points. Makes life more interesting, of course. But I've certainly encountered both parts. And then who did what first. We talked about this maybe before. I'm not sure that we could—

06-02:16:52

Burnett:

The priority dispute?

06-02:16:54

Leitmann:

Well, yeah. Who did what first. Yeah.

06-02:16:57

Burnett: Yeah, yeah. We haven't. Does it fit into our timeline?

06-02:17:03

Leitmann: Yeah. I think that—

06-02:17:06

Burnett: You've talked about it generally, yes. Yeah.

06-02:17:07

Leitmann: Yeah. I know. But it's just about the timeline because Isaacs' book came out just about that time, middle sixties, on differential gaming. I think it was called *Differential Games*. And Isaacs, as I mentioned before, and Bellman, had been working at RAND Corporation in the forties. And Isaacs was convinced that the principle of optimality, which is sort of the short statement on which dynamic programming was based, was just a special case of tenet of transition, I believe that he had in three reports at Rand in the forties, late forties when he first came out with differential games, which became the basis for his book later. And he was convinced that Bellman stole this from his reports. And he spent many years of his life after that trying to prove that and, in fact, in 1966, that was one of the main entertainments at the Moscow Congress of Mathematicians, these two American mathematicians having this big fight. And it's just incredible how all-consuming that was for Isaacs, who later became a professor at Johns Hopkins. I was the master of ceremonies at his retirement dinner. So this must have been maybe already late seventies. Yeah. Both he and Bellman died in the early eighties. And I used the phrase that Isaacs was a legend in his own time. He fell in love with me because I said that. Suddenly I was his best friend. On the other hand, the Isaacs widow, Rose Isaacs, after he died, '82 or '81, used to go on cruises and she came through here on a Pacific cruise and stayed with us three days in Berkeley. And maybe the second or third day I mentioned at breakfast that Bellman had died. He had died a few months before that. And she said, "Good," just like that. [laughter] So it permeated the whole thing. People are so strange.

06-02:20:00

Burnett: Yeah. Well, let's pick that up as a theme perhaps next time. But we're in the middle sixties and then we can pick up and talk about the Moscow conference. I think that would be great.

06-02:20:10

Leitmann: Yeah. And there are a couple of other things we can bring.

06-02:20:12

Burnett: Absolutely.

06-02:20:31

Leitmann: I mentioned two of my early post-docs and, yeah, '65 the geometric method with Blacquière came out about the same time, their books and that sort of thing. So for example, the Polish edition of my little book, it was translated into Polish by the wife of a guy who became a very close friend and

colleague, Jan Skowronski, who later taught at University of Queensland in Australia and was also a visiting professor, or associated professor or something at USC [University of Southern California]. And we started a series of meetings together there every year, optimization and control meetings, that were always at USC until Skowronski died, and then I ran it myself. And then I initiated meetings in Europe. So, again, there's that international aspect.

06-02:21:28

Burnett:

Absolutely.

06-02:21:27

Leitmann:

Good.

Interview 7: September 26, 2018

07-00:00:21

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History series. This is our seventh session, and it's September 26, 2018, and we are here in the Berkeley Hills. When we last talked, we were talking about the birth of a new field, and perhaps several new fields. There were the beginnings of convergence. People had been working in separate silos, and you were beginning to address the fact that people were using somewhat different techniques, but kind of working on the same problems, and you wanted to bring folks together. That was the *Optimization Techniques* monograph in 1962. Before we go into much more detail about the 1960s period, I wanted to step back and talk a little bit about your family life, and if you could update us a little bit on the family situation.

07-00:01:36

Leitmann:

We talked about the fortunate happenstance that I didn't go to Zurich, and therefore immediately met Nancy again, who had essentially given up on me, I think. That meant that by 1955, we were ready to get married. That was January '55. That was really the introduction to a very happy first decade in the history, if you want to call it that, of my life. That was the personal part, but very much similar important things happened also professionally and personally, because many of the people I encountered became very dear friends. These things are coupled in a way, and it's very difficult to take them apart. I returned from Berkeley after the first year to finish my thesis, as we had talked about, back on the job at China Lake. Got married, however, during that first year here in Berkeley, and then our first child was born in '57. That was already back in China Lake, but also, in the same year, back in Berkeley, later in the same year. Josef is born in April of '57. We returned to Berkeley in August of '57, when I was invited to come back and join the faculty. Then Elaine, our daughter, was born already, of course, here, in '59. So one was born in the desert, and the other one in Oakland.

I was really, in many ways, the most consequential decade that I can sort of put together in retrospect, both personally and professionally. Personally, we just mentioned why. Professionally, because it's really, for a very large degree, the people that I was able to hook up with, meet, become close to both professionally and personally. Surely the most important, from a professional point of view, was Austin Blaqui re from the University of Paris, who had come here as a visiting professor to replace a Berkeley professor on sabbatical, but then immediately started working with me. That must have been '61 or '62 [1964]. It also meant Jan Skowronski who was from Poland, and who hooked up with me for really two reasons. Because he was interested in control problems—very prominent—so we corresponded on and off, and he came, actually—really emigrated—to Australia in the late sixties, '68, I think, and whose wife translated my little book, *The Introduction to Optimal Control*, in '68, into Polish. That was a particularly nice happenstance. Now

we were really in close contact, because he had also, at the same time, taken on a role at USC as adjunct professor. He would be at the University of Queensland, his regular appointment, and then, during the summer as an adjunct professor at USC. At that time, he began a series of meetings, to be at USC at the time—and they were, for the first five years—optimization and control meetings. They took place every year, and I participated, both personally and in planning, from then on. They had a meeting for my sixty-fifth birthday there, and that's one of the volumes that came out of this. He's probably, in retrospect, maybe not as important as Blaquièrre, whom I really regard as not only a good friend, but a teacher because he taught me an awful lot in mathematics, particularly, and with whom and his wife, Paulette, we became very, very good friends, until he tragically died on his seventieth birthday on the way to a meeting which I had organized in Vienna in his honor. That's much later.

I recall how many of the people who became close to me had tragic ends. One of them was one of my two first post-docs. There was Marro and there was Basile, both from the University of Bologna. They had just got their doctorates there. It was about '62. I worked with them, but not very closely, because the area they were interested in and contributed to was really in linear systems, and that was not my major interest. I did communicate the work they did while they were with me. They wrote a paper, and I communicated it, because I was an associate editor at the *Journal of Optimization and Control*, probably in '67 or '68. Took them that long to write it all up. As I probably mentioned in my last talk with you, Basile was probably one of the few real engineers I ever met. He was great in theory—the mathematics, the modeling and so on. He could invent or improve a system, build the equipment in his own lab, with his own hands, and then certify it. He came up with a very good autopilot, both for planes and for ships. In fact, I don't know whether I mentioned that when it was ready to be certified—and he was a great pilot—we flew together from Bologna to Ajaccio, the capital of Corsica, on the autopilot. That one was later on produced in the United States for planes—not for the boats—at a little company in Arizona. He would, very often, spend part of the summer at the University of Florida as a visiting scholar there. He lived in Rudolf Kálmán's house very often, because Kálmán, by that time, had his own yacht he was cruising in the Caribbean or somewhere.

It was on one of those visits, much later, 2011 or 2012, that he was there and he was taking one of his students, and that student's teenage son, on a trip on his plane. They were going to a neighboring airport, because the gas was cheaper there, and the engine quit on takeoff. There's nothing you can do with that kind of plane, because there's not enough room to glide, and they just crashed into the hangar and blew up.

07-00:10:17
Burnett:

Oh my God.

07-00:10:19

Leitmann: So that was the end of somebody who became almost a second son to me, as somebody later on did.

07-00:10:29

Burnett: Can I ask you a couple of questions? One is, you mentioned Basile as a real engineer, and Blaquière, you would probably describe as a real mathematician.

07-00:10:48

Leitmann: Except that he also had a doctorate in physics, and actually wrote a book on nuclear physics, but more from the abstract point of view, and he had students, PhD students, in physics.

07-00:11:07

Burnett: Can I ask, what is your identity? How do you define yourself as a scientist or a researcher?

07-00:11:18

Leitmann: I'll be very crass with myself. I think I'm a dilettante. I think that's the best title. The "D" is not the "D-R," but "D-I-L," because as I've often stressed, and really believe, ever more so, that I had, occasionally, a good idea. Whether it was really good, that's not my judgment. But then I had to stop short and not go into great depth with that, because I simply lacked the tools. Then I had the good fortune to meet people, whether a student or a post-doc or a colleague, who could say, "I think we can do a lot more with this," and then we were able to do that. So the basic idea was there, and then I stopped short, and sometimes returned forty years later with sort of finishing it up. I don't know whether I would really think of myself as other than a dabbler.

07-00:12:29

Burnett: Let's say that you oversee a number of projects, and you see them through to their conclusion. So let's say that there's that. But is there a way in which you're scanning across literature, or you're having conversations with different people with different areas of expertise, and things occur to you because of that? Where does the idea come from?

07-00:13:05

Leitmann: You mean the basic idea at the beginning?

07-00:13:06

Burnett: The basic idea at the beginning, yeah.

07-00:13:09

Leitmann: In the case of what we later call equivalent system-based optimization, that sort of came naturally, because I ran into a problem, or a set of problems, say, minimizing an integral, or maximizing it. I could do that by zero mathematics, by just observation. But then there was always something lacking. Let's take that concrete example. I want to, say, maximize the value of an integral. Then I noticed that in many of the cases that we were dealing with—for example,

even in rocketry—the integrand was essentially non-negative. They were quadratic forms, very often. Energies, that kind of thing. Kinetic energy, for example, is quadratic, in the velocity. So I could look at it and say, I know that the minimum value of that integral is zero. I know that, because I look at it, and I can make the integrand zero, for all values of the variables. Say it was quadratic. But then I couldn't meet the terminal altitude condition, because that means I could only go from a given state of the system back to the same state, and of course, in real problems, you want to start with zero velocity with a rocket, and go up to some height. You're not always at zero. Then it occurred to me that a variable transformation would allow you to do that, and that's how I got to that, really.

07-00:15:15

Burnett:

You are working with the theory, with the mathematics, with the language of expressing real things in the world, and you are recognizing that there is some kind of gap between the applied object in the world that you're dealing with, the process that you want to describe, and the language that you're using. There's a grammar problem.

07-00:15:44

Leitmann:

Yeah, and I think that one of the important elements of my doing this kind of thing, saying, "What is the simplest way I can do it?" is really, that's the only way I could do it. Then recognizing that makes it essentially useless, unless I do something else. Then, happily, it occurred to me maybe a change of variables. Then it had to be certain variables that preserved the minimum of that integral in the new variables. You must always go back from one to the other, and the transformation will allow you to do that for the given boundary conditions, and then the transformation back will allow you then to apply it to the original problem. The motivation—let's put it this way—was, in a sense, in real life, if you want to call it that, but I thought it was a nice idea. That's really the best. And until we could extend that to more general systems—I could not guarantee that whatever I wanted to integrate, say, is zero, before I make the transformation, until I started working with a mathematician, and we extended the quadratic integrand to just convex. A quadratic thing is sort of the simplest convex function. At that point, that mathematics got to be a little hairy, but the motivation, even of Dean Carlson, who is my collaborator mathematician, was, you've shown that this is fine for quadratic systems, and the next step is to see whether we can prove it for convexity, and that we did. There's a little bit of mutual inspiration involved, but the heavy work, the heavy mathematics work, very often, somebody else did.

07-00:18:10

Burnett:

We talked about this a bit off-camera, but this period that we're talking about is almost chaotic in the extent to which there are multiple research programs that are roughly concurrent, that are somewhat informing one another. So you're very busy, you're very active, but an idea comes along, and maybe you don't have time to pursue it. Did you have a black book with ideas? Did you organize yourself so that you could put—

07-00:18:15
Leitmann:

Call it “unsolved problems” kind of thing?

07-00:18:53
Burnett:

Yeah, something like that.

07-00:18:53
Leitmann:

No, actually, I didn’t do that. I didn’t do that, because I got so involved in the next thing. I mean, I knew I had done it ten years before, twenty years before, and then, fortuitously, somebody came along who either urged me to do more, and who said, “I feel that a lot more can be done because,” or somebody just arose and I said, “Here’s a mathematician willing to work with me.” So, happy happenstance. I can’t over-stress the fact that I’ve just been very fortunate, because that’s what made it possible to do that.

07-00:19:38
Burnett:

Some people who come into your life, some people arrive in the Berkeley sphere, in California, for example. Jan Skowronski is at USC in the summers. Obviously, he, based in Queensland, wants to come to the United States and work where the action is, or some of the action is, with respect to optimization work. But you’re also then going out to conferences, and as we stated in last session, these were these seminal conferences. It would be the second annual, or the first annual.

07-00:20:15
Leitmann:

Some pooped out, and some went on.

07-00:20:19
Burnett:

Some of these were extraordinarily important. Some of them were highly theoretical. Some of them were more applied to specific problems. You also became involved in organizing conferences. I think we talked about that a little bit, the one at the US Air Force Academy, and the way in which you picked people, irrespective of whether they worked in industry, or in government research labs, or in academia. You were quite ecumenical and welcoming in your approach. Some of them, you didn’t even know personally. You said you just read some of their references, and that inspired you to invite them to the conference.

07-00:21:14
Leitmann:

Yeah. That was true, for example, that first volume on optimization techniques. Most of the people I knew personally, but there were certainly some whom I never met personally, maybe not until later at a meeting, and said, that’s another way of looking at it, that they push. There’s a very interesting sort of side product of this. Things become the vogue, and so a particular method—to the extent that I know of subjects—I won’t mention them—where people became so enthusiastic that they had T-shirts at the meetings, with the name of that particular—you know.

07-00:21:59
Burnett:

When was that, roughly? In the seventies, probably?

07-00:22:02

Leitmann: No, even much later. Much later, yeah.

07-00:22:06

Burnett: So it's like a team.

07-00:22:08

Leitmann: The "school of," you know, kind of thing. Later on, maybe when we get to the late sixties, I'll give you a couple of examples how disturbing that becomes. I went to a meeting in Siberia. It was a US-Russia meeting. Two working languages were Russian and English. There were two people there, heads of schools, schools in that sense. They couldn't stand each other, and really insulted each other in public, particularly one of them, so much that the translators would suddenly realize what they were translating and said, "Now they go too fast," they said. "We can't quite follow what they're saying." This was a translation from Russian into English, so they didn't want us to know. These things happen. Human beings are territorial, I guess.

07-00:23:13

Burnett: Well, isn't there another layer to that, too, in that particular case? Because some of the meetings between Soviet scientists and American scientists had to do with the Cold War, at least effort, at detente, right? Not that there was detente at that time. It was the height of the Cold War.

07-00:23:38

Leitmann: That's certainly true. I think the Russians—the Soviets then—really felt that they were as good or maybe better in many things, and not getting credit for that. To some extent, that was certainly true. They had also a very hard time getting information, because most of that major publishing was done in English. But just as an aside here, one of the first papers of mine that appeared in Russian—I'm going to just look at it—it was very early. It was a paper—I think maybe '61 or '62. Yes. It's called "Optimum Rocket Trajectories," and was in the procedure of a meeting called ISS Symposium on Vehicle Systems Optimization, in New York, in 1961. That was in the proceedings. It appeared in PMM, which is the *Proceedings of Mathematics and Mechanics* of the Soviet Academy, the same year. The proceedings of that meeting, in English, didn't come out until the next year. So the Russian version of that paper appeared before the original English. It means, also, that they were really interested in the field, which is, of course, great, but it always, when I look at it, hits me that the PMM Russian version of that paper was a year earlier than the English version in the proceedings of that meeting.

07-00:25:38

Burnett: Was there any compensatory element to this? [Lev] Pontryagin is going to meetings, but I guess they were not worried about him defecting. But I imagine that—

07-00:25:48

Leitmann:

Anything but. No, in Pontryagin's case, first of all, he was blind. He always traveled with two graduate students, who had a tape recorder—in those days, it was a wire recorder—they took down every word he said. It was that kind of worship, or whatever you want to call it, of their icons. When we get to the Mathematics Congress in Moscow in '66, I've mentioned that in passing, that they were waiting for two Americans, namely [Richard] Bellman and [Rufus] Isaacs, to have a big fight. There's much more detail. By the way, there's another aspect to that meeting you might find interesting.

07-00:26:37

Burnett:

I don't mind, actually, talking about that meeting, unless it's too disjointed to go there.

07-00:26:44

Leitmann:

Not particularly. That had really three very interesting aspects. I'm not a mathematician, never claimed to be, and I wasn't going to go to that meeting, because this is the primary meeting of the International Mathematical Society. It's where the Fields Medal is always presented, at that meeting. I suddenly got a call, in '65, from a lady mathematician—she was wonderful, and I wish I could remember her name—who was the head of the mathematics division of ONR.

07-00:27:21

Burnett:

I think you told us about this. She needed an escort, effectively.

07-00:27:26

Leitmann:

Yeah, so we talked about that. Okay, so that was one aspect of it. That's how I got to that meeting, and to a few other places in Europe, where she had to go, and she just took me along. It's my initial exposure to being a gigolo, I guess. [laughter] Go ahead.

07-00:27:48

Burnett:

This is a trip to the USSR, the sixteenth to the twenty-sixth of August, 1966. The International Congress of Mathematicians.

07-00:27:58

Leitmann:

Right, I think it was the third one.

07-00:28:00

Burnett:

One of the things that stood out for me, and this is probably not important, but there was a reception at the Kremlin Palace of Congresses. This is a big deal.

07-00:28:11

Leitmann:

Oh, this was a big deal. There were 2,500 mathematicians in Moscow, from all over the world. It showed that, when they wanted to, they could actually do things very well, not only, in this case, socially, but the escalators all worked in that grand hall. They had a dinner, stand-up dinner, one night in that place. It was really interesting. Everything was just standing around. Bottles of vodka, cognac. You helped yourself to the food. Tables laden with everything,

but then there were strange people walking around in overcoats who were putting stuff in their pockets. People got off the street, into the Kremlin, to get into that meeting, and they were just stealing the food. That was certainly interesting, and it hit me, on later visits to the Soviet Union, that that was just sort of my introduction to how the system worked.

07-00:29:22

Burnett:

Did people tell you later that the escalators don't always work?

07-00:29:26

Leitmann:

No, but it was well-known, and from my uncle, for example, who worked there for ten years, how terrible things were. I must have mentioned the autobiography—well, the diary—it got to be an autobiography—of a young engineer from Berkeley, civil engineer by the name of Witkin, who wanted to go to the Soviet Union, for two reasons: because he was in love with their favorite actress, and because he was a Socialist. He thought he would help. When you read his diary, which came out against his wishes, because he burned all the notes but didn't know he had left one with friends—Berkeley Press came out with that about ten or twelve years ago. It's incredible how miserable the system was, from planning, inefficiency, bureaucracy. Just incredible. But if they wanted to do things, both physically, engineering-wise, well, they could spend a lot of money and do it. And, of course, when the Kremlin was involved, there was more than that.

07-00:30:48

Burnett:

Well, and certainly any hint of an association with rocketry and that kind of thing. These are very—

07-00:30:53

Leitmann:

Yeah, although the meeting was a mathematics meeting. I don't think it was that. It was simply showing off. It's a little bit like getting the Olympics to the country. This was the mathematics Olympics, and it meant, from the point of view of prestige, an awful lot to show the best face to the world, and they could do that. They could absolutely do that. So they did. But that contretemps between Rufus Isaacs and Richard Bellman was interesting, because Pontryagin sat in the first row, with his two graduate students, just awaiting the blow-up. It was already known that there would be one, because Rufus Isaacs just entered the regular, little twenty-minute, half-hour paper in most sessions, and Bellman was a plenary speaker. They were, in terms of prestige, not on the same level. They noticed immediately, of course, something was coming up, because the title of Isaac's paper was "Dynamic Programming and Differential Games," which was Isaac's thing, "and What the Former can Learn from the Latter." That was the title of the paper. So they said, "Ah-ha, here it comes," so they were all salivating. Rufus got up, and he said, "Well, everybody knows, of course, the mathematics"—because, by this time, his book had come out, too—"so I won't waste your time with giving a paper. I just want to give you the history of this subject." Then he went, how they were both at RAND. Bellman was his supervisor at RAND, and how this all

got stolen, because his three internal papers which he wrote at RAND about this were classified, so that couldn't come out. So it wasn't until the book, which is presumably based on these papers, came out that he could say, "Well, what's called the optimality principle is really a subset of what I call a tenet of transition in my paper." He talked about that. But the upshot was, I was having lunch with Bellman that day, and he actually broke down and cried, and he was such a tough guy. I mean, he was so hurt, because he was essentially called a plagiarist before the most important mathematics meeting in the world. He was crushed. Bellman wasn't easily crushed, because he was a tough cookie.

07-00:33:48

Burnett:

So your sense was that it was not intentional? If there was any—

07-00:33:52

Leitmann:

I have no idea. There's no way of proving it. Isaacs could certainly prove that differential games was born at RAND. Now, who took what from whom, who the hell knows? I don't think that Bellman needed that, because he never considered dynamic programming his major contribution to mathematics, never. It just became the most well-known, from the point of view of vogue, which, by the way, is also tied up with that meeting at Akademgorodok later on, when the two groups of Russians really got at it. Well, I might mention, since I already mentioned about the translator—

07-00:34:40

Burnett:

That was in '68?

07-00:34:41

Leitmann:

That was in '68, in Siberia, Akademgorodok. Somebody from the Pontryagin School was presenting a paper. They were one-hour papers. Fifty minutes, and then ten minutes question and answers. During the question-and-answer period, Nikita Moiseyev, who was the head of the computer division of the Soviet Academy of Science, got up, and he said—I remember the words verbatim, because they still translated that. That's what stopped the translation. He said, "If I hear the word 'Pontryagin maximum principle' once more, I shall throw up." That's when the translator said, "Now they're talking very fast, and we can't"—[laughter]—this was again one of these things where groups really hated each other. That, in Russia, probably had to do with who got more money or prestige. Probably the computer people got a lot more than the mathematicians, simply because they worked on problems of interest, rather than producing a viable and fancy theory. Nikita Moiseyev was a very important person. We'd got to be almost friends, but certainly good acquaintances, because we would go to restaurants at noon. Long line of people. I'm talking about maybe 1965, '66. He'd just walk up, and the door would just be open right in front of the line, and he walked in. It was really astounding.

07-00:36:30

Burnett: A different system.

07-00:36:31

Leitmann: Yeah, a different system. That's one aspect to it. I think maybe I've lost my thread at the moment.

07-00:36:38

Burnett: You were talking about the importance of that event.

07-00:36:40

Leitmann: Oh, one other thing that happened at that meeting, by the way, the '66 meeting, Steve Smale, from our math department, received the Fields Medal, and he was going to be presented with it. Two things happened. He got into trouble with NSF [National Science Foundation], because he had a big contract. He used to go to the beach in Hawaii and work on the beach, and somebody reported him. It's a perfectly good thing. He was a theoretical mathematician. That's where he produced his best work. Then when he left for Moscow, the Berkeley newspaper—I don't know what it was called—Berkeley had its own newspaper at the time—had a headline saying, "UC mathematician defects to Soviet Union," because he left in a hurry. They know he had this fight. That was well-known. On another day of that meeting, he and I were walking down to lunch, and the university, Moscow University, has a long staircase, maybe for 300 feet, going down the hill. I wouldn't do that today. Steve and I were walking along, and suddenly we were surrounded by Russian [Soviet] journalists. They were from everywhere. It must have been thirty or forty of them with their notebooks. They wanted to interview him on what? Well, Steve Smale was a well-known opponent of the war in Vietnam, and he was in the news about that and all that, and they wanted to know how he felt about US involvement in Vietnam and that kind of thing. He smiled and said, "You know, if I were in any other country but the Soviet Union, who recently invaded Czechoslovakia"—that's the period right—

07-00:38:37

Burnett: Oh, the Prague Spring [and the subsequent Soviet invasion of Czechoslovakia, August 20, 1968].

07-00:38:38

Leitmann: Right. He said, "I would be happy to answer your question, how I feel about the war in Vietnam, but not in a country who did that just recently." Well, all the correspondents disappeared in seconds. They didn't want to hear that, of course. They didn't write it down or anything. But two guys arrived from a black limousine, in leather coats. They actually picked him up, physically, and carried him down, and put him in the back of this limousine. I said, "Well, we'll never see Steve Smale again." He did arrive that evening at the North Vietnam dinner for the mathematicians. They gave a dinner, and he was invited as a guest of honor because of his opposition to the war. Really, when you think of the strange things that happen—you know, it's all at one meeting.

07-00:39:46
Burnett: That would have been at the Akademgorodok?

07-00:39:48
Leitmann: No, no, that was at the math congress. When was the—

07-00:39:56
Burnett: Prague Spring would have been '68.

07-00:39:58
Leitmann: No, there was something before that.

07-00:40:00
Burnett: Maybe Hungary.

07-00:40:01
Leitmann: Oh, no, Hungary. It was Hungary.

07-00:40:01
Burnett: Hungary in '56.

07-00:40:02
Leitmann: No, no, it was Hungary, not Czechoslovakia. You're right. It was Hungary, because he specifically said, "Given what the Soviet Union has done"—it was the Hungary thing, because he mentioned tanks. He said, "I would be happy to talk about this, but not in this country." It was a very brave thing to do, in a way.

07-00:40:27
Burnett: Absolutely. And how did you feel?

07-00:40:30
Leitmann: I took pictures of his being carried away, photographed them. I had a thirty-five-millimeter little camera, and I gave them to him afterwards.

07-00:40:40
Burnett: Did you just want to document the situation, just in case?

07-00:40:42
Leitmann: Yeah, because it was sort of like a movie. Here are these two guys that looked like the typical NKVD agents who seem—

07-00:40:49
Burnett: From central casting, yeah.

07-00:40:50
Leitmann: Yeah, with their leather coats and stuff, and they actually picked him up, and they didn't throw him into the car, but they took him away.

07-00:41:00
Burnett: It wasn't polite.

07-00:41:01

Leitmann: Yeah, they took him away. But he did show up that evening at the Vietnam dinner.

07-00:41:06

Burnett: Oh my goodness. Were there any consequences for him back in the United States? Or, by that point, the war—

07-00:41:13

Leitmann: No. First of all, he had the Fields Medal, which is more important than a Nobel Prize. The Fields Medal winners, on the Berkeley campus, have nameplates for their parking place. They say “Professor so-and-so.” It doesn’t say “NL.” Lots of those around. This is only once every four years for two or three people. No money involved, but prestige-wise, I think it’s actually a higher honor than the Nobel Prize. He later became, in fact, the president of University of Singapore, or the chancellor or something. I saw him maybe four or five years ago, last time he was on campus. I talked to him, reminded him of that day.

07-00:41:59

Burnett: I understand—this information comes from memory from twenty-five years ago—but my understanding is that the Soviets were very well-represented in the Fields Medal, and maybe that was later, not at the time. But later on, they became—

07-00:42:15

Leitmann: Oh, that was probably later on. Yeah, that was probably later on.

07-00:42:18

Burnett: That their talents were recognized.

07-00:42:20

Leitmann: Oh, yeah, that’s absolutely true. Of course, they were always terrific in mathematics. There’s no question about it, already from Peter the Great onward. It was that kind of tradition. So it’s not surprising. I ran into this myself, because I stopped going to the Soviet Union in ’74—that’s another story—but I was always in very close touch with those who dared, and there were some who dared, and kept correspondence up, and suddenly others sort of disappeared until perestroika came along. It’s very interesting.

07-00:43:02

Burnett: For you, personally, at that meeting, was that a positive? Those are heady, heady years. We could perhaps transition to talking about the tracks of research that you’re doing. We’ve talked about calculus of variations with respect to optimal control in the previous session, but you pursue a slightly different track, and that comes from exposure, I think, to—

07-00:43:39

Leitmann: Probably Blaquièrè.

07-00:43:40

Burnett:

Blaquière, I'm sure. I think the first evidence I find of your looking at the geometry of optimal control is a paper you give by that name, in the University of Michigan, at the Optimal Control Symposium, in October of '64.

07-00:44:01

Leitmann:

That was at that meeting where Isaacs and Pontryagin were. Wasn't it?

07-00:44:06

Burnett:

Did they have two dust-ups? Because I think that was—

07-00:44:09

Leitmann:

No, no, no. You're talking about Bellman now. I don't remember whether he was there, but I know that—it was '64, right?

07-00:44:23

Burnett:

Yeah.

07-00:44:24

Leitmann:

He arrived with a manuscript version of his book—Isaacs—at that meeting. I remember that. It was really the first time I met him in person.

07-00:44:42

Burnett:

When you said, in the '66 meeting, that people were expecting them to get into it, Isaacs and Bellman—

07-00:44:48

Leitmann:

It was known, maybe *sub rosa*, that there was an enmity. Then once the paper appeared on the program with the title I gave you, then it was clear there was something in the air.

07-00:45:03

Burnett:

The stakes are high. It involves not just prestige, but resources, and it's your reputation as a scientist, and that is everything, because some of these people do well, right? But a lot of them are not being paid what they could make somewhere else.

07-00:45:24

Leitmann:

Isaacs was already, by the time of that period, sort of the father of differential games. His book had great reviews. He was a very good mathematician to begin with, and just decided to go into a field where he could solve problems. And so his book is essentially a series of problems, and people liked that a lot. There was a friend of mine in Germany, Walter Breinl, who wrote, in fact, a paper in the early 2000s pointing out the great contributions that Isaacs made to game theory, of course, dynamic game theory, differential games. To this day, he's simply considered the father of that field, for good reasons, just as Bellman is known for dynamic programming, but he himself didn't think that that was his major accomplishment. Here's a man who wrote 650 papers in his life, forty-two books. I think of the last ten years of his life. He had a brain

tumor and was operated on, and he was truly a cripple at that time. He wrote a hundred papers in that ten-year period, and five books. Of course, he was driven, because he knew he had not too much time left. It's usually in retrospect, because when you live this, you don't analyze the situation. So I didn't analyze what was going on in Moscow. Too many things going on anyway. It was only later on, when you have time to think back on it, that you say, ah-ha, these are the connections. I never really analyzed it in any contemporaneous way.

07-00:47:31

Burnett:

There's this Optimal Control Symposium, and you give a paper on the geometry of optimal control. Looking at the geometrically—

07-00:47:41

Leitmann:

That was '64?

07-00:47:43

Burnett:

Yeah, I think so. In that conference, that's the paper you give. Pontryagin gives a paper on differential games. It's a lot of—

07-00:47:54

Leitmann:

I think that, again, that was not a subject that they, in any way, started. I think that he was somebody that they said, ah-ha. Sixty-four, huh?

07-00:48:12

Burnett:

I'm not sure if it was actually published, but it was—

07-00:48:14

Leitmann:

Oh, no, there's a paper.

07-00:48:16

Burnett:

Maybe it came out in '65, maybe later.

07-00:48:18

Leitmann:

No, there's one earlier one. Because there's a paper, "Geometric Aspects of Optimal Processes with"—

07-00:48:32

Burnett:

In '65, yeah.

07-00:48:33

Leitmann:

In '65.

07-00:48:35

Burnett:

That's with Blaquièrè and [K.V.] Saunders.

07-00:48:37

Leitmann:

Right.

07-00:48:38

Burnett:

Is K.V. Saunders someone you—

07-00:48:40

Leitmann:

He was a student. He was a PhD student of mine. I'm sure that I had—oh, here we are. In '63, I had a paper, in the *Journal of the Society of Industrial Applied Mathematics*, '63. It's called "Some Geometrical Aspects of Optimal Processes." It's item thirty-eight on the list.

07-00:49:10

Burnett:

Can you talk about the germination of that approach? I think we've already identified, basically, what it does for—

07-00:49:20

Leitmann:

It's much more closely related, first of all, to dynamic programming than to calculus of variations and optimal control à la Pontryagin. I think what made a difference is that the necessary conditions for optimality in dynamic programming, I arrived at very analytically. But the basic idea is that you start with looking at the global picture of optimality, and you do this in terms of looking at the minimum value of what you try to, say, minimize, over all the space that you're involved in. So, for, say, all initial conditions, you ask the question, what is the minimum value of an integral. We started with the same idea, whereas, in the calculus of variations, and the maximum principle, you start out by saying, suppose I have an optimal trajectory, and I vary the control in such a way that a new trajectory starts. What conclusions can you draw from that locally? In other words, what conditions arise when you do that? You don't compare optimality globally, but just locally, looking at neighboring trajectories to the optimal one. Those are usually not optimal, those neighboring ones, because of the uniqueness of solutions for those systems. Then saying, you know, what has to be true when I compare the non-optimal ones with the optimal one, and to do that at the final point, when you've finished your trajectory. From that, you get necessary conditions. Whereas we said, here are optimal conditions that hold everywhere in the space you're interested in, and then let me see what happens at a given point. Then you get local conditions that way, but you start out really—and in a sense, you do that in dynamic programming, too, because you say, now, if I'm at a given point, in terms of the value of the payoff, the minimum value at that point, what happens if I now go on and end up with a partial differential equation, the Hamilton-Jacobi-Bellman equation? Because Hamilton-Jacobi theory was already nineteenth-century stuff. We didn't end up with that. We simply looked at the geometry at such a point, in terms of tangent planes.

07-00:52:41

Burnett:

You're spatializing the problem.

07-00:52:43

Leitmann:

Yes, right, and for games we looked at the geometry in the book *Qualitative and Quantitative Games*, with Francis Gerard. We get geometric results, in terms of the surfaces in which these trajectories lie. Then, later on, I think with Gordon Mon, another PhD student, there's a paper—let's see—probably about the same time. No, maybe it was a little later. Anyway, Mon was doing

his dissertation on that, but he was already doing it on games. Again, that geometric method, once you go from a single controller to multiple controllers—in games, we call them players—then you can use the same methods. I'm sure that the Russians started out looking at games from the Pontryagin point of view. Very often, they are called Pontryagin games. It's sort of a two-pronged way of looking at it. It is nothing mysterious. And I say, the happy happenstance of Blaqui re and I meeting at that time—and he had already thought about this to some extent, but it hadn't gelled yet, so it sort of gelled when we started talking. I think this must have been '61 or '62 [1964] when he first came here to replace Rosenberg.

07-00:54:40

Burnett:

So he was talking about a geometric approach to optimization at that time?

07-00:54:45

Leitmann:

Oh, yes. Yes, he was.

07-00:54:48

Burnett:

Just to make sure I understand before we get any further, I was reading something that you wrote about—it was a geometric representation, a spatial representation, of Pareto optimality, and so it had, actually, an x and y -axis, and it had two concentric circles. They were almost like a Venn diagram. They were touching at a certain point, and this x -axis, or the bottom axis, moving to the right, was cost. This point was where neither incurred a cost in the—

07-00:55:38

Leitmann:

It was constant-cost surfaces, yeah.

07-00:55:41

Burnett:

Right. It's a way of spatializing the problem, and it's very beautiful, it's very simple, and very clear, but obviously points to very complex things.

07-00:55:54

Leitmann:

I think that was not a contribution by me or Blaqui re. Other people looked at that. It is such an obvious thing to do, of course, to demonstrate the meaning of Pareto optimality.

Maybe now we'll probably get into games pretty soon. The two main game types—and there's a third one called Stackelberg games involving a leader/follower scheme—have to do with equilibrium games à la [John] Nash, who was one of the pioneers in that field, and Pareto optimality, sometimes called efficient controls, which are due to Vilfredo Pareto, who started life as a railroad engineer, and then became a mathematical economist—really, basically, a Fascist in addition. The idea of equilibrium is if you have many controllers—call them players—each one has a control, sometimes called a strategy. They are members of a set of strategies. The whole set is, say, an equilibrium set. The overall strategy is an equilibrium one, because no player can gain anything by departing from such a strategy. It doesn't mean it's the

best, and in fact, it may be not unique. By departing, everybody else keeps his/her strategy in that set, and you depart. You can't improve your position. That's a non-cooperative game, because you assume that everybody else is a member of that set.

Now, in Pareto optimality, it's the opposite. You have, again, more than one player. There the idea is, if you had a Pareto set, if anyone departs and all the other ones keep their strategies, somebody will get hurt. So it's more a cooperative game. You're willing to give up something so as not to hurt somebody else. A very simple idea. The nice thing is that, mathematically, these definitions can be put in terms of just inequality statements of the costs, with and without changing of strategy. Then all follows just from the inequalities.

07-00:58:57

Burnett:

This sounds like this all goes back to [John] von Neumann, doesn't it?

07-00:59:03

Leitmann:

Well, it goes back to von Neumann, of course, but his games were not dynamic. That's the difference. Certainly the game contributions were much older—ten years older. Yeah, absolutely. So these definitions and these ideas were already—von Neumann and people of that ilk at the time.

07-00:59:31

Burnett:

If you're on a track to continue explaining, I don't want to interrupt that.

07-00:59:34

Leitmann:

No, no. It's just when we now talk about differential games, we know, presumably, what defines them.

07-00:59:44

Burnett:

This is at a time when so much of this is filtering out into other domains. Other sciences, even social sciences, such as economics, are taking this stuff up, and we'll talk about that.

07-00:59:58

Leitmann:

Even more so, probably.

07-01:00:02

Burnett:

I actually know another person I interviewed, Lester Telser, his book on differential games and economics comes out in '72, so this is around this time that this work is quite electric and being discussed. It's precisely at the moment when these other sciences are embracing a dynamic vision of their field. The economics goes from a nineteenth-century kind of statics to something that is actually quite dynamic. Are you aware of these applications, and are you targeting—I think in the introduction to one of the books that comes out in '66, '67, there is a claim that this is not just for mathematicians and engineers, but this is more broadly applicable, even to the behavioral sciences.

07-01:01:04

Leitmann:

I think this was already pretty much bruited about at the time. It's just interesting that people even make a big deal about this, because the time was ripe to go, say, from minimizing an integral to minimizing it with side conditions, and then to say, how about an integral, but for different, independent controllers, or players? People say, "Well, I can make this more general," and therefore more applicable. But not necessarily. Some people just took the mathematics as pure and saying, "I can make this better." Yeah, very often, that, of course, leads to applications. When it came to optimality, just simple, with one controller, that obviously arose already when I was at China Lake, because I was looking at optimization of trajectories. So there was certainly a motivation. But then that sort of silently, maybe, crept into more. I don't think you can really plan. These days, they, of course, do it, plan research. In those days, people didn't plan research.

Changing the subject completely, this also has other effects. There was a time when cooperation among scientists was minimal, because it was competitive on the one hand. This is in the theoretical sciences, but even, to some extent, in the applied ones. People did experiments even in the lab, usually one or two people. It got to the point where it got to be a few hundred people, and if you now go to CERN, it's thousands of people on one experiment.

I think I must have mentioned this before when I said how come I progressed so quickly academically, because unbeknownst to me, I was doing a very smart thing, namely not coauthoring papers for the first twenty-five years—ten years, anyway. That makes evaluation easier. But when you have a thousand people, then you say, "Who did what?" But today, that's no longer an important thing. Their doing, essentially, team research has come up very much so. The idea is, "we'll sort it out later." If somebody says something smart, we'll know he was the first one who said it, and that leads to fights. Even at the Nobel rank, there was a fight. Somebody at San Diego who said, "I did it first." I just lucked out that I preceded that period, and so that never even occurred to me at the time.

07-01:04:34

Burnett:

In terms of originating new ideas or new—looking at your career, it doesn't necessarily seem to be the strongest of objectives. It's interesting that we're kind of mirroring the cooperative and non-cooperative games. There's competition in science, and there's cooperation and collaboration. You seem to be much more interested in collaboration and the play of ideas.

07-01:05:10

Leitmann:

It partly harkens back to the fact that I was trying to fill holes in my knowledge, and so finding collaborators, either fortuitously—probably more so than particular people on purpose—and finding a fair number of students who wanted to do that, and were better-trained than I was in mathematics. So it's sort of a happy circumstance that arises when that happens. As I say, I lucked out. I'm not kidding.

07-01:05:45

Burnett:

There's one thinker who talks about planters and weeders in science. The planter is someone who is maybe a dilettante, someone who can go out and think about, oh, what about these connections? They end up setting up a new trail, or a new row to hoe, and other people come and tend that, and—

07-01:06:16

Leitmann:

Improve it.

07-01:06:17

Burnett:

—and improve it, and optimize it. I think that that's certainly part of the story. But you seem to be really one of those scientists who thinks in terms of setting off in a direction and seeing what kind of collaborations can emerge from it.

07-01:06:40

Leitmann:

Yeah, and again, I don't know whether I did that terribly consciously. It's a two-way thing, because very often a student will look for a professor where they can do that kind of thing. It's not one-sided. It's a combination of things.

07-01:07:07

Burnett:

Maybe there's a selection process, too, in your career, when you became an academic scientist. When you came to do your graduate work, there was no one to work with. You did want to work with someone. They were in Zurich. You weren't allowed to go. You came to Berkeley, and you went and you figured it out. There's this kind of initiative, and a plasticity of mind, perhaps, to be thrown into that situation and adapt, and it's possible that, A, they saw that, B, they recognized your previous work in a very practical, very important domain of aeroballistics, working with the military, and Berkeley seeing both the way you work at Berkeley, and the expertise, and the contacts, and the context that you bring with you from that domain.

07-01:08:07

Leitmann:

Could be. I certainly wasn't aware of that.

07-01:08:10

Burnett:

Yeah, that's what I mean, is that there's those kind of—

07-01:08:12

Leitmann:

In retrospect, you can sort of categorize things.

07-01:08:18

Burnett:

That's our privilege right now.

07-01:08:20

Leitmann:

Right. But it's not something that says, "Now, who the hell do I work with?" and sometimes "who the hell I could work with, but it doesn't work out." My Canadian colleague who urged me to go into this equivalent problem area later on, forty years after I had written down the original idea. We'll probably return to that, because that's when I go into—just in passing, since we're just going from optimality to games, there's one more contribution, primarily

because, again, of a personality involved. We talked about this before. In 1962—so, a little bit before that—a man by the name in S. Fred Singer approached me, wrote to me. There was no email then. He said he was about to edit a book called *Progress in the Astronautical Sciences*. It was published by North Holland, eventually. He said, “I’m inviting you to write a chapter on trajectory optimization.” This is my chapter, little monography by itself. It’s called *The Optimization of Rocket Trajectories: A Survey*, chapter four. That man, to this day, has a very interesting history. He’s exactly one year older than I am. He was born in Vienna. Of course, I knew nothing about this. He was, at that time, I think, already working. He was what you call an earth scientist. He was interested in geophysics, but in the atmosphere, not below the ground. So he worked on the Van Allen belt, that kind of thing. But he was interested in rockets, because they would be sent up there. By that time, we were doing that, in ’61. Turns out that he is the guru of the global warming—I wouldn’t say denialists—he doesn’t deny it, because he’s a scientist—but doubters. He has his own institute for that, very well-known, and reviled, of course, on the other hand. He’s a very strong personality. I had no idea, obviously, what I was getting into. This was just a field that I knew, and I was very happy to write a chapter. But again, complete happenstance, and again, a Vienna connection, which came up many times after that, again, completely unplanned.

07-01:12:07

Burnett:

I think he figures prominently in Naomi Oreskes’s book, *The Merchants of Doubt*, which is an interesting history of climate change—not denial—but climate change science that is critical, and that somewhat serves particular interests.

07-01:12:24

Leitmann:

In particular, his objection is that it’s man-based pollution that’s—he doesn’t deny that it’s happening. It’s warmer and all that, and there’s no denial, but he doubts that it’s human beings who are the primary factor in bringing that about. That made him very popular, of course, in some circles. In the seventies, when we had oil shortage, and people were talking in dire terms about their future, and then, a couple years later, it all dissolved, and there was plenty of oil around and all that, he was one of the first people who said, “No, no, this is all nonsense. This is no problem.” So he became, again, famous, with a more conservative view of the world. He’s still around, he’s still alive. I just Wikipedia-ed him, and there’s even a picture of him. We’ve all changed, obviously, but I recognized him.

07-01:13:32

Burnett:

So you only had contact with him for that paper?

07-01:13:36

Leitmann:

We corresponded after that a little bit. In fact, I think I didn’t berate him, but I asked him, about five or six years ago, whether he still believes this, and then he gave me lots of references.

07-01:13:52

Burnett: Did he change your mind?

07-01:13:54

Leitmann: No, he didn't change my mind. But you see, the reason he didn't change my mind is because I became a worst-case designer later on, because I'm very risk-averse, and I'm always over-insured. This is another aspect of being insured against damage in the future and so on, so I'd rather take the—might be the more conservative view of things, by saying, if this can possibly happen, we better watch out. Whereas people who are not risk-averse say, well, everything has a risk, and you're overemphasizing the risk.

07-01:14:36

Burnett: It depends on one's definition of risk and the nature of the threat. I don't necessarily encourage you to speculate, but I wonder about your life history, your biography, and also, perhaps, Singer's, which I'm not deeply familiar with. But given that he was a year older than you, and he was from Vienna, I can put two and two together and say that he probably had a difficult time as well.

07-01:15:05

Leitmann: But he drew a different conclusion.

07-01:15:07

Burnett: He drew a different conclusion. What do you think that was for him?

07-01:15:12

Leitmann: We never discussed it. We didn't get to be close friends, but almost friends. We corresponded for a while, and then again, I say a few years ago, because the whole thing came up, and his name somehow came up in the newspapers again and so on. Who knows?

07-01:15:32

Burnett: The threat for some, at least for some of the climate change critics, is that the thing that will collapse first is a free society, and so they believe that—

07-01:15:45

Leitmann: Too many laws and regulations.

07-01:15:49

Burnett: Right. So there's that. You can come out of the Fascist experience and look at that as a threat.

07-01:15:59

Leitmann: We didn't talk about that at all. But it's just interesting that these people pop up. Another one that popped up—we just talked before, and I don't want to diverge here—was one of Bellman's prime students, John Casti, whom I met maybe in the late sixties, second part of the sixties. He's about thirteen years younger than I am. In connection with these interviews, I wrote him a note last week. We lost contact with each other about five, six years ago, because I

stopped going to Vienna, and he lives in Vienna. At that time, he didn't speak a word of German, but he was an honorary professor at the technical university, with his own office and so on. It's quite possible that I met him through this avenue. I got updated. He's a very interesting person. "Interesting" has many meanings, there's no question about it. He has authored maybe twenty, twenty-five books, half of them more popular books, but still very well-written, on sociological problems, on science, everything. Very prolific. Quite a few papers. Nothing like his mentor, of course, who wrote 650 papers. Somebody that worked in many different places, at an institute in Vienna that was established during detente, IIASA, International Institute for Applied Systems Analysis. It was strictly a US-Soviet operation to begin with. It's completely international now. He worked there for a while. He was on the faculty of the Santa Fe Institute, which is a big think tank. He's a very smart guy, and a hard worker. Of course, he was never burdened with a family. He's had many lady friends, as he happily admits, but never really had to worry about a family. When I first met him, in fact, he had a brand-new wife, a little Chinese-American lady, American-born, much younger than he. I guess it was really his father-in-law who didn't like him. She was one of his students, graduate students. These people just arise out of nowhere, and then they just—some hang around, some don't. We'll get back to those.

07-01:19:18

Burnett:

I wanted to ask you, from a distance, it looks like the work on the geometric approach to optimization seems to lead into the work on games. Is that true? Can you explain how that works?

07-01:19:45

Leitmann:

I don't think it's a geometric thing. I think that the path to games was simply a generalization from a single player, a single controller, to more than one. I say, in my first book, that—not the *Topics*, but the other one. I have a little sentence at the beginning of the book that says, "One man's optimum is another man's pessimism." That sort of is the beginning of thinking in those ways.

07-01:20:19

Burnett:

You start publishing on this in the mid-to-late sixties, and you collaborate with a couple of folks, one of them G. Mon.

07-01:20:34

Leitmann:

Yes, he was a graduate student. For a while, became a preacher, but later on ended up at JPL [Jet Propulsion Laboratory].

07-01:20:44

Burnett:

This was on a class of differential games, two-player, zero-sum games?

07-01:20:50

Leitmann:

Right. Saunders was a student, and he probably predates Mon.

07-01:20:59

Burnett:

This talks about the behavior near discontinuity surfaces of the strategies. The meaning of surface there is quite specific.

07-01:21:08

Leitmann:

Remember, I talked about surfaces, a mathematical surface. In three dimensions, you can imagine it, but less so for dimension greater than three. They're the ones that arise in the geometric method. Generally, you can get results very quickly if the surface is "smooth" and has a gradient which is continuous. In other words, the normal vector to the tangent plane of that surface—it's a surface in multidimensional space. If that surface is smooth, then you can do lots of things. Blaquièrre and I did not make that assumption. We talked about just the existence of tangent planes. Now, these tangent planes, first of all, they can be vertical, so to speak, in the cost/state, which the gradient can't be, because it becomes infinite. That's number one. Number two, if those surfaces are not smooth—in other words, their gradients of the cost surfaces are discontinuous—then you come to a point where you have to either go along the boundary between the surfaces which have discontinuous derivatives [gradients]. Then what can you say, what kinds of situations can arise, that either take you there and let you go across into a neighboring region, which has its own tangent plane? Or where you go along the boundary of two of those, or where you are driven back into the—? And so this was really an analysis of all these different possibilities that can happen. It was that particular paper by Gordon Mon, and it came out of his dissertation.

07-01:23:24

Burnett:

And that would be a quantitative game, right? In the sense that—

07-01:23:30

Leitmann:

Well, it could be qualitative, too, because we're only talking about—when I say a trajectory, I don't mean a physical trajectory. I mean a solution curve. Here, we're in the space where the discontinuities arise, not along a given trajectory—for example, the controls, very often, jump. Control depends on time. Suddenly, it jumps to a different function. That's a different kind of discontinuity. Here, we're simply saying, we will subdivide the space into regions, and that was an idea that I actually learned from [Leonard D.] Berkovitz, who was mentioned somewhere—a mathematician, I think, at [Purdue University, 1962-90]. Then I said, if we do that, and let's just allow these surfaces to be defined on these subsets, what kind of things can happen when you meet boundaries among these surfaces? This would have happened with a qualitative game, too.

07-01:24:39

Burnett:

Okay, that helps. The earliest I can see this coming, there's a conference in 1965 in Saclay, France, the *Congrès Automatique Théorique*, in which you expand your work on the geometry of control optimization.

07-01:25:04

Leitmann:

Was there a paper?

07-01:25:06

Burnett:

[Narrator Addendum: Yes, there was; it is no 42 in my bibliography] This is something that you returned to the following year, in '66, and there was a summer school.

07-01:25:15

Leitmann:

Okay, the summer school, I remember. I don't remember that particular meeting, because, given my close association with Blaquière, I probably went to France fifty, sixty times, maybe more.

07-01:25:30

Burnett:

And how many times to Italy?

07-01:25:32

Leitmann:

About the same, and then later on to Vienna, probably about thirty, forty times.

07-01:25:40

Burnett:

So just these really tight connections with people that you collaborated with, who were friends of yours.

07-01:25:49

Leitmann:

To this day, the consequences are terrific. I've stayed in touch with all who have survived this far. Some haven't. Skowronski died at sixty-three. Both Blaquière and he—this is really something to think about—stayed with us the month before they died. Blaquière was here, and I was planning a meeting for his seventieth birthday, and on the way to the meeting, he had a heart attack. Skowronski, whom Nancy loved, because he was such a bubbly person and so kind, and loved good food and good drink, he stayed with us a few days, and on his flight back to Australia, he had a stroke when the plane landed in Sydney.

07-01:26:36

Burnett:

Good God. But it speaks to the frequency of contact between these people.

07-01:26:41

Leitmann:

Oh, yes. We were very close. We were very close.

07-01:26:45

Burnett:

That's extraordinary. These are multiple research tracks. You're looking at the geometry of optimization processes. You're also getting into the work on games. Does the work on games just take over in the late 1960s? Are you really—

07-01:27:15

Leitmann:

Yeah, it took over, because anything you can now want from now on for the old-time optimal solutions are just a special case with one player. In fact, some of the results, later on, when I became interested in extending the theory, I simply said, "Now we don't have to worry about the one integral kind of thing, because that's a special case." The differential game is much more

difficult, of course, but once you have conditions, they will also hold for a one-player game. A one-player game is much simpler, so you can have additional things that are allowed, that might not be allowed. In both cases, what you use as information to make your decision—in the classic calculation of variations, it's only the time. Independent variable doesn't have to be time, but we call it time. As soon as you get to the game, it becomes much more interesting, but difficult, and therefore, very often, we go back to what are called open-loop controls, or strategy, because it's the fact that you can have much more information. You can say not only what time is it, but where am I. Am I in the state space? What geographic location, what velocity am I going at, all those other things. That immediately gives you a much richer problem, if you want to put it that way.

I remember visiting IIASA [International Institute for Applied Systems Analysis]. It is in a castle outside of Vienna. It was the emperor's hunting castle, just half an hour outside of Vienna, in a town called Laxenburg, a little village. I was walking with [Aleksandr] Letov. He was very famous. We were walking in the park, the imperial park, and we were talking about what makes more sense, that you only say what time it is, or where I am. He gave an example. He said, "Now, in our five-year plans, we don't care where we are. We just measure the time. Now it's this time, and so we do that. Or we can say, what the hell have we been doing to the economy? Let's look at where we are." So that kind of thing came in much more with games, of course. You have the whole idea of having either nice people who are willing to give something up, others who say, "I don't care. I won't cooperate with you." It's interesting, and so much more has been done since then, of course.

When human beings are involved, these original games are really not terribly useful in a descriptive way, because people aren't rational. Much of game theory was based on rational expectations, and they ain't fulfilled. [Reinhard] Selten, who was a Nobel laureate at the University of Bonn, was really one of the first who did major work in introducing psychology into games. Nash, to some extent, had already had a little bit of that and so, if you're in a dictatorship, those equilibrium games are interesting, because that's the best way to do it, and I can tell you that you're going to do that. But in a so-called free society, they're not descriptive. The more you can build into the model, of course, from that uncertainty because of the unpredictability of human players, the more the game becomes descriptive.

07-01:31:48
Burnett:

You have said before that your dissertation work involved computing, and was difficult, and it was with punch cards, and it was tedious, and you wanted to stay as far away from it as possible. Where is computing with respect to the field? If the games are becoming more complex and there're more variables or dimensions or spaces, and they have relationships that change in a dynamic sense, with respect to each other, is increasing computing power a part of the

story? You can indulge in more complex games, because you could plug in numbers and have a computer solve it.

07-01:32:46

Leitmann:

Then you draw conclusions from examples, of course, or you can test sensitivity, for example, that way, and say what's important, what's more important, in a decision-making process. Yes, that's absolutely true, and a lot of work is being done precisely because you can't prove enough analytically, and then you increase the definition of proof, sort of, by saying every example we've looked at gives you this result, the better the chances—that also leaves the possibility, of course, that you'll make terrible mistakes, because you haven't thought of something. You have to do these things with care, but it's certainly true that computational capabilities give you a lot of information you don't get analytically.

07-01:33:39

Burnett:

Yeah, and it's one set of the conferences that you begin to attend, as we move through your career. There's this International Federation on Information Processing Conference on optimization techniques, and that started in 1968. It's right at the moment when you have Department of Computer Science and Electrical Engineering at Cal, at your home institution, 1968. That begins. Even if you weren't doing it—

07-01:34:15

Leitmann:

No, and I'm completely ignorant in that field.

07-01:34:18

Burnett:

But it's part of the mix, this technology—

07-01:34:21

Leitmann:

Yeah, and it has changed both the research fields and the teaching fields, absolutely.

07-01:34:33

Burnett:

Can you tell me a bit more about the work with Blaquière, with respect to games? How does the monograph that you work on together come into being? Obviously through these intense collaborations.

07-01:34:55

Leitmann:

Yeah, although both for him, maybe more so than for me, it was really the theory that was more important, because he felt that you should be able to prove things. If you do it numerically, usually you can't prove a damn thing, except you have confidence that this is the right answer. So it's a different view of the world, and I think you need them both, as long as you are aware of the fact that when you do it numerically, things can happen that you haven't foreseen. I'm thinking of Chernobyl, for example. We talked about this before. You leave something out. It takes a lot of imagination to think of all the possibilities, and so you have to be aware. There has to be a caveat saying, this is what we know so far, and we can be pretty sure this is very close to

optimal, because numerically, not much changes from that solution. Then somebody comes along and shows you that some disaster happens that you haven't thought of, but then people will say those are very rare. But then you have to multiply the result of that happening by the rarity, and that's maybe a very big number, because the result is so catastrophic. That's another way of looking at—I just found fiddling around with the equations and that kind of thing more fun, let me put it that way.

07-01:36:40

Burnett:

Is there an overlap with Casti when it comes to the X-events? Is that him? Is he the X-events person?

07-01:36:49

Leitmann:

Yeah, he's one of the important ones—in fact, they just funded a big institute for him in Vienna, and then he works for some outfit in Germany, as a consultant on that. There was an economist [Lebanese-American Nassim Nicholas Taleb], mathematical economist, who wrote the book *Black Swan* who was extremely acerbic. For example, he says that people who use statistical methods are morons. It's sort of the watchword of his book. Because the statistical methods do not account for outliers of that importance. He calls them also “normal curve morons,” or something like that. It's an interesting book to read, but it's very much different from Casti's, which came later, a year or two later, because he gives you—this book was not meant for specialists, but simply somebody who wants to know what's an X-event. He gives you a lot of actual examples, those that happened already, those that can happen, and he gives you a very good idea what an X-event is. It's very difficult, of course, to plan for those, because they are so rare, but there's a lot to be done along those lines, because of the collapse of the stock market, a terrorist attack like the one on the Twin Towers, that kind of thing. Rare events, but very catastrophic, and so it's a subject which is fascinating. It has to do with, essentially, how complex the situation is, so that no single normal person can envision all the connections. That happens as more and more connections happen between things. It's a little bit related to chaos theory, where the butterfly in Siberia changes the weather here. The actual examples are really stunning, it's a fascinating subject.

07-01:39:22

Burnett:

Catastrophes are just that, and some of them are civilization-ending.

07-01:39:29

Leitmann:

That, again, has not to do with stochasticity. In other words, catastrophe happens because, in these systems, very small changes have huge effects [as in so-called chaos]. I think the first example of that in the literature is actually a meteorological system. The system equation has to be at least third order. First- and second-order equations can't have catastrophes in that sense. An old friend of Nancy's at MIT gave the first example of that [Edward Lorenz]. Anyway, he had a third order example in a weather forecasting system, and it shows that you get something that looks random, but really isn't. It's just that

the changes are so small, but it gives you big results that it looks like crazy things happening, because a small change has been—in a parameter, or an initial condition has brought this about. Those are fascinating. Then the question arises, would you be better off modeling systems like that statistically, and then use statistics? There are, again, two schools about this. There are a lot of these fascinating subjects that are beyond me, really, and I wish I had more time.

07-01:41:16

Burnett:

Oh, of course. Talking about the author of *Black Swan*, who was complaining about statisticians, or those who are slaves to statistics, there are complaints within disciplines about the excessive reverence for statistical analysis, and an insufficient attention to the mathematical logic. That speaks to what you're talking about.

07-01:41:45

Leitmann:

I'll give you an immediate example from where I live. I live at a very fancy senior home, and they have an alarm system, presumably. They have these little boxes in all the rooms. It says, "If you have a problem, pull." Now, your problem might be that you have difficulties putting your socks on or that you're in the midst of a heart attack. In that fancy place, there is no way of letting people at the other end know, unless you can converse with them, whether you have a serious or just a trivial problem. If you can't put your socks on for fifteen minutes, no problem. In this discussion, which is currently going on between some of us and the administration, you get pushback. They say, "Oh, but no matter what the problem is, our average time of response is only so little. Twelve minutes, fifteen minutes." I point out to them, to the person who's dying of a heart attack, the fact that the average is fifteen minutes might mean an hour if he's farther out on the curve. It's a statement which is completely meaningless. What you want to analyze when you decide what kind of system to put in, is to look at the worst case, not the average case, because the worst case is what concerns the individual who can suffer the worst case. I can't convince them of that. I can't convince them of that. We're in the middle of the debate now, so—

07-01:43:44

Burnett:

It's not over yet.

07-01:43:46

Leitmann:

No, no. But it's interesting that, here, the people who are, of course, ignorant in statistics use this thing, the average, as saying this is a good system, on the average. This is very good if you produce nails. On the average, every tenth nail fails. No problem, right? For that kind of system, average and median, is perfectly good. Then if you're risk-averse, you will say, well, if the average is too high, we have to do something. But when human beings are involved, and particularly their lives, it's a completely ridiculous statement, that you get this—it was really, again, more obvious to me, because I'm risk-averse, on one hand, and I think really looking at stabilizing systems—we'll talk about

that later on—under the worst allowable conditions. It's really interesting how you run into this in real life.

07-01:45:02

Burnett:

On one hand, these conferences that we've been talking about, it seems fairly abstract, and if you go through the monographs and the papers, it's a lot of mathematics, and it can seem fairly abstract. But it does point to very, very critical applications that you have been deeply involved in. Does a rocket explode, or does a rocket make it to its destination? Does a system—if it fails, does it fail well? These kinds of—

07-01:45:43

Leitmann:

Yeah, the type of catastrophe. It's maybe Chernobyl, and maybe just a very hot room.

07-01:45:50

Burnett:

Right, exactly, exactly. In 1968, Austin Blaqui re and you published *Jeux quantitatifs*, and it's translated into English, and then you build on that, and there's a collaboration that comes out of that kind of work. Can you talk a little bit about putting together that work with Blaqui re? Or is it kind of continuous for that period?

07-01:46:30

Leitmann:

No, it's continuous. It's already something we talked about, that we always had a shot of cognac when we thought we had proved something. It's that kind of thing, so we drank a lot. [laughter]

07-01:46:47

Burnett:

That's wonderful. That's a wonderful description of academic success.

07-01:46:51

Leitmann:

Or something.

07-01:46:52

Burnett:

Or something, yeah, something to celebrate. As you've mentioned before, there's so much travel. I mean, it's extraordinary. You are going to Hawaii almost every year. There's an annual conference there, the Conference on System Science.

07-01:47:11

Leitmann:

Yeah, it was in January. I went, really, many more times privately, of course. It's now almost eighty times. I got to Hawaii because I stopped for refueling on a flight back from Tokyo in 1961, and said, gee, this is a great place, I'm going to stay a few days. It led to decades of happiness.

07-01:47:40

Burnett:

You couple this with, if you had a conference you were going to, you had friends in that country, typically, and in many neighboring countries. I can see from your travel plans that you would arrange, and sometimes Nancy would come with you the whole time, sometimes just part of the time. Can you talk

about some of the most memorable travel that you did, and how that was integrated or not into your research work?

07-01:48:12

Leitmann:

I'll give you an early example, again, a US-Soviet interaction. I think it was the early sixties again. It may have been towards the end of the sixties, in fact. There's probably a paper on this. The University of Liege, in Belgium, organized a meeting just between US and Soviet scientists, again, in, essentially, the optimization rocket field. I recall, just to make life more interesting, an incident. Angelo Miele was one of the main people there. I don't remember offhand, but there were some important Russians there. This was maybe a meeting of thirty or forty people, that's all. A symposium, four or five days. Since it was a US-Soviet symposium, English and Russian were the working languages, and you could present your paper in one. It would be simultaneously translated into the other. When Angelo Miele was going to give his paper, he said, however, that he cannot be interrupted by the simultaneous translation, and he would like to have his talk translated, so there can be a discussion, after he finishes it. Talks were forty-five minutes or so, not a big deal. The Russians didn't like that. In his then inimitable way—he wasn't the greatest diplomat in the world then—he said—the Russians were having some problems in the United Nations—they were giving the Russians a hard time. He said, "Believe me, you will get a better deal from me doing this than you're getting in the United Nations." All the Russians turned around, gave him their backs, and started reading *Pravda*, or *Izvestia*—anyway, their newspapers. I was then important in healing that, by giving a party, a joint party. The Russians had brought a lot of vodka and black bread with them in their suitcases. Suitcases were all made of cardboard in those days. We drank an amazing—we were marching down the streets of Liege, through the red light district, singing loudly at two o'clock in the morning, and it was the one time I had a terrible hangover, really bad. So that's outcome of an international meeting, which, however, in the end, everybody got along, at least in principle.

These meetings very often had really two aspects—many aspects, and two of them were, if they were in Hawaii, everybody wanted to go. Now, that meeting started out as a small meeting, it was AFOSR-supported when I started going there [Air Force Office of Scientific Research]. It may have been a hundred people, something like that. I remember a colleague from the math department approaching me and saying, "I see you on the program committee. I'd really appreciate it if you invited me to give a paper. I'd really like to go." Being a nice guy—he was a good mathematician—I arranged a session in his field. You know that there were people, they came only to the session where their paper was, and some only went to give their paper, and left afterwards. Didn't come until their paper was presented and left right afterwards.

07-01:52:36

Burnett:

That's the danger.

07-01:52:37

Leitmann:

That only lasted a couple of years. The Air Force found out about that. It's now back to the small meeting. People screwed themselves. There is that aspect to these huge meetings. Many of the professional meetings, the AMA [American Medical Association] meetings, for example, usually on cruises, that kind of thing. The best thing is that you are well enough known so that you become a plenary speaker in one of those, and then everything is paid for.

07-01:53:12

Burnett:

But I think your point is that smaller meetings, it's a bit more specialized. It offers opportunities for contact, genuine contact, between scholars.

07-01:53:22

Leitmann:

Yeah. There were meetings, and they're still going on, in Vienna. My good friend, Gustav Feichtinger, who was one of the first people that I met when I started returning to Vienna—it was the early sixties—and I think he probably introduced me to John Casti, who already decided to live in Vienna. They have a yearly meeting, called Optimization in Economics and Operations Research, that kind of thing, and I went to quite a few of those. Those are small. There are maybe thirty, forty people, that kind of thing. There are no simultaneous sessions, first of all. The meetings with simultaneous sessions are always interesting for young people who want to make as many contacts as possible, but if you really want to know what's going on—first of all, the papers are very short, and secondly, you sort of have to figure out which session should I jump to now, because there maybe thirty, forty simultaneous sessions in some of these big meetings. So I have my doubts about those.

07-01:54:45

Burnett:

This is at the happy stage of the beginnings of the general field of control optimization, and all of these subfields that are associated with various applications and various theories, and the marriage between the two. It sounds like it was an exciting time, and a wonderful kind of cultural exchange, too, between different groups of people.

07-01:55:12

Leitmann:

First of all, it made for friendships. If there are a lot of simultaneous sessions, most of the people whose paper you go to, you'll never see again. It just looks as if what they want to talk about is of some interest to you. That's number one. The other thing is it becomes a circus. For example, I've gone to meetings in Italy, in vineyards. One of the most recent ones, maybe, was ten years ago. It was in an old monastery. They had their own vineyards. The lunches went from twelve to two, with a lot of wine. Three o'clock, you reconvened until about five, five thirty. Then you have a lot of time, A, to have a really nice time, and B, to talk more closely to people, not necessarily at the meeting, but afterwards, or say, "Let's stay in touch."

07-01:56:16

Burnett:

It sounds like it was a very heady time. I think, for this session, let's pause, and we can continue next time talking about another evolution in the 1970s in

your work in robust control, and also get caught up in some of the service work, because you were still, of course—it seems like—

07-01:56:43

Leitmann: I thought we were going to do the service separately, as a track.

07-01:56:47

Burnett: Yeah, we can. I think, decadally, it might be helpful to just track that a little bit, but we can completely decide that.

07-01:56:56

Leitmann: We can mix it in a little bit. It won't all get recorded. Nancy and I wake up in the middle of the night, and we say, "Have we really been to all those places?"

07-01:57:11

Burnett: It's amazing to see.

07-01:57:13

Leitmann: No, it really is. I still can't believe it.

07-01:57:17

Burnett: Well, let's continue talking next time.

07-01:57:19

Leitmann: Okay, thank you.

Interview 8: November 30, 2018

08-00:00:18

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History Series, and this is our eighth session, and it's November 30, 2018, and we are here in the Berkeley Hills, and in our last session, we were talking about your research in the 1960s, and your trips to many different conferences, some of which were inaugural conferences of new questions, new subdisciplines, in your field, and in a Cold War context, and so, we talked a little bit about meetings in the Soviet Union and Pontryagin. We talked about the controversy between Rufus Isaacs and Richard Bellman. We talked about risk, and the assessment of risk, and we talked about your collaborations with Austin Blaqui re, and I think we still haven't finished with the sixties, because this is a really important, seminal decade for you, in establishing multiple strands of research. So, I'm wondering if we could return to the 1960s, and if we could talk a little bit more about optimal control in some different areas, and optimal control in mechanics is something that you were interested in. Can you talk a little bit about the work that you did in that area?

08-00:02:08

Leitmann:

Now, I'm not really quite sure whether you're talking about applications in mechanics, or—

08-00:02:15

Burnett:

I think the actual—

08-00:02:17

Leitmann:

—maybe you have a particular paper in mind.

08-00:02:18

Burnett:

You published a paper in 1963 in the *Journal of Applied Mechanics*, "Some Remarks on Hamilton's Principle," and this ends up being important for your later research, and I'm wondering if you could talk a little bit about that, and why it's so important.

08-00:02:37

Leitmann:

This is really one of those little papers, five or six pages long, in which I thought I had an interesting idea, but for a number of reasons, I didn't pursue anything beyond that, in the hopes that at a later time, something will arise, either another idea or a collaborator or whatever, so that we can carry on, and it's one of the two papers primarily where this happened. The other one is the paper on the transformation-based idea of getting equivalent systems for which the extrema, minimum or maximum, essentially, obtainable by inspection, and then by retransforming to apply that to the actual problem, allowing one to meet boundary conditions, which is usually not possible in the untransformed problem because the solution is a constant.

Hamilton's principle applies in a number of areas in science, in physics, and other parts of mechanics. The mechanics I was interested in is Newtonian,

with Leibniz, of course, being very important. I think this was in the middle 1800s that Hamilton was one of the people who had the idea of having a single principle from which one could derive all the features of the problem applicable to the basic setting. Those principles which date back to the eighteenth century, when people thought that God was a very powerful and all-knowing optimizer, and so people started putting forward minimum principles, or maximum principles, depending on whether you want to minimize or maximize something. It turns out that many of these principles, and in particular, the ones in classical mechanics, can be minimum principles under circumstances, but in general are not. They are certain principles, the solutions of which are necessary conditions for a minimum or maximum, but only necessary conditions and therefore may not lead to a minimum or maximum, so they're called stationarity principles—

08-00:05:38

Burnett:

I'm sorry, what was that?

08-00:05:39

Leitmann:

Stationarity principles, because—

08-00:05:40

Burnett:

Stationarity.

08-00:05:41

Leitmann:

[Content redacted by narrator]

[Narrator Addendum: In a little note “Some Remarks on Hamilton’s Principle” I address the following question: Suppose I have the equations of motion of a mechanical system and the solution of these equations; can I determine the forces resulting in a solution which satisfies stationarity of an integral. In particular, this means determining integrands of such integrals. This is the so-called inverse problem because in applying Hamilton’s Principle with appropriate integrals, obtaining the equations rendering stationarity renders the equations of motion. The corresponding forces can then be found via the equations of motion.

The great 19th-century mathematician Oskar Bolza considered this problem for single variable linear systems and found a partial differential equation, the solution of which allows an answer to the inverse problem. I employed Bolza’s methodology to find solutions to Bolza’s partial differential equation for a class of integration functions. I then illustrated this result on hand of example for which the forces are dissipative as well as non-linear, both of which are ruled out in the basic Hamilton’s Principle. Bolza had already found the dissipative forces case.

This was a remark which lay dormant for more than four decades except for an application given in “A Minimum Principle for a Population Equation,” namely the so-called Volterra-Verhulst population equation.

About ten years ago, Firdaus Udwadia, with one of his students, and more recently Fai Ma, a colleague in the ME department, and students, re-addressed inverse problems for vector-state variables, albeit solely for linear systems.]

[End content rewritten by narrator]

08-00:14:26

Burnett:

Well, we were talking the other day off camera about this shift that happens. Depending on the discipline, it happens at different moments, but in the 1960s, just for one example, take economics, where you're moving from these relatively static analyses to these really dynamic ones, and using, incorporating partial differential equations, which will give you this change over time that you're seeking, right, when you're looking at something like markets?

08-00:14:59

Leitmann:

Yeah, well of course, in our case, we were still able to work with ordinary differential equations because when I did use the equivalent-system method, the great mathematician who first came up with this in the 1935 era, was a German/Greek mathematician, Constantin Carathéodory.

08-00:15:29

Leitmann:

He was very famous. I didn't know his work because it appeared in German in the 1930's, and the first translation of it was just about the time when I was looking at the equivalent problem, he was really the father of the equivalent problem, but he ended up with partial differential equations, and because of my transformation method, I ended up with ordinary differential equations, so that pleased me. [laughter]

08-00:16:02

Burnett:

So, in the 1960s, you are traveling around the world, and you are giving seminars or short courses, you are, of course, giving papers, and just attending conferences in general, and we talked a little bit about that, and we also talked about the Cold-War largesse a little bit, that there was a lot of support for this kind of work, and we've talked about that to some degree, but I think you had some examples when you were talking about, there was a course at the Air Force Academy in Colorado Springs, I think, and you said that they were able to fly people out, and can you talk a little—

08-00:16:55

Leitmann:

Actually, and that one wasn't at the Air Force Academy; it was in Colorado Springs. The Air Force Academy came in when they asked me to give a course there, but it was Air Force-supported because the AFOSR [Air Force Office of Scientific Research] actually was the vehicle for this. Well, I thought what I might do, because I tend to wander, is write down notes, for the 1960 decade, with your help and to some extent, the list of meetings that I could recall, either with your help or by myself, and I'll go through and stop saying more if it's one that's not germane.

There was the London meeting in '59 first, where I got tangled up with a famous person and thought he was going to ask me a nasty question but he didn't, then the next meeting was the one in Stockholm, which was the Eleventh International Astronautical Congress. The Astronautical Congress is probably one of the first one that came up, because, of course, the Space Age started much earlier. Then there was a meeting in '61 in Tokyo, which was on space technology and science, and by that time, I had become a consultant with Lockheed so they not only paid for it all, but I got treated extremely well because they were trying to get a contract. (The Olympics were to be in Tokyo) for building a monorail, and so they had a PR company, and the PR company took care of me too; that was very nice. When I came down the steps from the plane there was a sedan waiting for me. I wasn't a vegetarian yet so I got fed a lot of Kobe beef. I was taken to a Geisha house, which was perfectly okay, that kind of thing. So that was Tokyo.

Then in 1964, I went to the Second All-Union Congress on Theoretical and Applied Mechanics, in Moscow, and with a side trip to Leningrad where I met a very famous Soviet mathematician—I'll speak about that a little bit later—and also met Vyacheslav—Slava—Isaev, with whom I had started to correspond, because he had published a paper on a topic that I had written about using the classical calculus of variations—I think it appeared in '57—and he solved exactly the same one, not knowing about this paper of course, by the maximum principle. We started to correspond, and he was really my host, in a sense. Again there's a little one liner, funny story: We were driving in from the airport. It was the year when there were these terrible bog fires around Moscow. In fact, they appear in Smith's book, *Gorky Park*. It's that very summer that *Gorky Park* takes place historically, was the summer I was there.

08-00:20:54

Burnett:

There were bonfires, or—

08-00:20:56

Leitmann:

No, bog, bog fires.

08-00:20:57

Burnett:

Oh, right, okay.

08-00:20:58

Leitmann:

Yeah, and they still, they have them every year, because the whole city is surrounded by these fields, and once they start burning—of course, they're used for fuel anyway, and that was really, really bad. What I said to him: "Oh, this is just like in Los Angeles," I said, "with the smog," and he said to me, "Oh no, it is not smog. It's atmospheric condition." [laughter] I'll never forget that phrase.

By the way, just one more line: It was very hot. It was a terribly hot summer, and those bog fires, and he said, "Let's get outside of the city to Moscow

woods, and have a picnic, and we'll pick up some beer on the way." We went through about five or six *gastronomes*—*gastronom* were the food stores in the Soviet Union—because there was no beer—people had bought it all—and so we had almost given up when we got to the place where we're going to settle down and have a picnic, and one of those babushkas, old ladies with the kerchief, was sitting by the side of the highway, and she had a bucket of beer. It was warm beer, but she had a bucket of beer. We bought that. [laughter]

The next meeting was also in '64, and that was at the University of Michigan, I think, yeah. That was an important one because that was Pontryagin's first visit together with [Revaz] Gamkrelidze, his number-one graduate student, with whom I became very friendly, and Rufus Isaacs had shown up at that same meeting bringing the manuscript for his *Differential Games* book with him to show people, to prove to them that this is all old stuff. Dynamic programming was born from this. That was '64, and the book came out in '65, I think, his book. Yeah, so he had the proofs with him. So that was an important meeting, and Pontryagin then on that same trip came to Berkeley, and I don't know if I told you this little story about—

08-00:23:27

Burnett:

Yeah, we did.

08-00:23:29

Leitmann:

Being a blind man, he always had a couple of graduate students with him, even on these trips, and Nancy stood next to him at a cocktail party. He was by himself, and so she—he had fairly good English—started speaking with him, and after about three minutes, he said to her, "Now I am getting tired. You may leave." [laughter]

08-00:23:55

Burnett:

Very direct.

08-00:23:56

Leitmann:

Well, his English wasn't that good. So then, there was the Michigan meeting, which was important because it brought together not only the people who were still trained in the calculus of variations with the exponent, the major exponent of the maximum principle, and the next meeting was in Colorado Springs, actually. That was the one that had the wonderful support from, I'm pretty sure it was AFOSR—it may have been ONR but I don't think so—and it was a meeting, essentially, on the optimal control. They invited people, and they said to the people they invited, "If you have other candidates, let us know," and this was really going to be sort of a post-doctoral school. Everybody who was invited and paid for gave two lectures. It was a ten-day meeting, and it was so posh that first of all, everybody was flown first class, and I invited Blaquièrè. He came with his wife and a graduate student. They were all paid for, and we got \$2,000 for those two lectures, \$2,000 apiece.

08-00:25:35

Burnett: Yeah, in 1965 dollars.

08-00:25:40

Leitmann: Yeah, and—and—they were so well organized that, for example, they had a separate program for the kids. A lot of people brought their kids of course, this was so posh, and you never saw your kids all day and they had a separate program for them—they had an ice-cream truck—and it was fantastic. And this only lasted maybe ten years.

08-00:26:08

Burnett: But that was the space race. This research was going to be central to technological advance writ large.

08-00:26:20

Leitmann: Yeah, and it was the space age, and, the rocket age, in the sense that we were going to have more and better rockets than the Russians, and this was not an international meeting, other than by invitation, and Blaquièr came from France, but I don't think there were any Russians there. I'm sure there weren't. [laughter]

08-00:26:43

Burnett: Well, now you organized that, didn't you?

08-00:26:45

Leitmann: No, I was a member of the organizing committee. I didn't organize that meeting myself.

08-00:26:51

Burnett: Oh, I remember in your papers, it looks like you did a lot of work, because you're sending letters, you're inviting all of these people, and then there's a, yeah, there's a publication around that time, isn't it, that has some papers that come out? I don't know if it comes out of that directly. I may be lost in my own notes here, but at any rate, you had done this, and this shows the tremendous support and connections with the organizations in the United States that were deeply invested in this, not only the Air Force, but also the Air Force's chief research facility at the time, RAND, even though it had spun off at that point.

08-00:27:38

Leitmann: It was Air Force and Navy, and actually, the NSF came a little later, maybe in the seventies.

08-00:27:46

Burnett: Well ONR [Office of Naval Research] was the huge supporter at—

08-00:27:48

Leitmann: Yeah, particularly math. Yeah, they had a fantastic math division, and it was a lady mathematician who took me on a gigolo to Moscow. I wish I could remember her name. She was a wonderful woman.

08-00:28:03

Burnett: I think you mentioned her name in a previous session—

08-00:28:06

Leitmann: Maybe I did.

08-00:28:07

Burnett: There were also ONR-funded meetings, at least your travel to some of these meetings into the seventies, and some of them to the Soviet Union. So, can you reflect on the purpose of, if during the Cold War, the Soviet Union is the enemy, and the area of research that you work on is very closely tied to the ultimate weapons that are the subject of the standoff between the two countries, between the two powers, can you reflect on the purpose of bringing Soviet—

08-00:28:50

Leitmann: I think—

08-00:28:50

Burnett: —and American scientists together?

08-00:28:50

Leitmann: Well, I think that both sides felt that if they could talk to each other, they would learn something, and it was true, as a matter of fact, of course, and, you got to remember that when Nixon was president, *détente* was coming up, early seventies, when he was still president, and it was really—I'm jumping ahead a little bit to 1972, as a matter of fact—there were two things, two events or initiatives that came about from the *détente*. One of them was a joint space flight—you may remember that we had a joint space flight—and the other one was the establishment of an institute to bring together only American and Soviet scientists, and that was going to be and did come about actually in Austria. The Austrians offered the two parties the emperor's hunting castle near Vienna in a little village called Laxenburg, and that's IIASA, International Institute for Applied Systems Analysis, which is in existence now, but after years, maybe ten years, it became a completely international organization, and it's still headquartered in this castle, and that's also where I met a number of very interesting people, and many of them still with me in a way.

08-00:30:34

Burnett: Right, right. But it's science diplomacy, in a way. It's some way of bringing—

08-00:30:40

Leitmann: I'm not sure how well that was thought out. Of course, the *détente* was diplomacy, but the idea to bring just those two together, it really was a hope on both sides that they would learn something. It wasn't love for each other, that's, I think, I'm pretty sure. The original constitution of IIASA specified that the president of that institute would always be an American, and the technical director would always be a Soviet citizen, and for example, one of my best friends to this day, just had dinner with him and his family a week

ago, is Alexander Kurzhanski, and he, to this day—he's seventy-nine years old—still commutes between Berkeley, because all his family is over here, and Moscow to teach, because they can't find really highly qualified people to teach the basic math and science. Everybody wants to go to business school. It's the thing. And it's also interesting that one of his sons, the younger one, is still, even though he essentially grew up and got his PhD at Berkeley, sort of impressed with Putin because he's really there to make Russia great again, and the father simply can't understand that, just as he can't understand his older son who became a capitalist, because he grew up as a socialist.

You see, so you get these interesting interactions, and we have wonderful discussions all the time. They came to our place where I live for dinner a little over a week ago, and then he goes on for hours because you know—

08-00:32:51

Burnett:

So much to tell.

08-00:32:52

Leitmann:

No, it's very interesting. So, I became peripherally attached to IIASA, partly I gave a few lectures there, but primarily through the people I met there, and of course, I went to Vienna a lot of times, so every time I went to Vienna, I took the side trip. It's about a half-hour bus ride from the center of Vienna to Laxenburg. It's a small village with very nice houses, and fantastic gardens, the emperor's gardens, and also a very good beauty parlor. Nancy liked to go there to have her hair done. [laughter]

08-00:33:38

Burnett:

So there's a number of conferences that you're attending in the 1960s. It's astonishing, and I think I've remarked on this before, and I'm wondering about, by the end of the sixties, if there's a shift in your thinking or your approach to—you know, we've talked a little bit about you working on control optimization, and this shift to work on games. We've talked about Blaquièrre a bit, and your work with him. In terms of the work, how do you make the decision to move into that field? Is it through these encounters with scholars that you're—

08-00:34:52

Leitmann:

Only in part, I think. I think it has a lot more to do with the people, like Blaquièrre, with whom you work very directly, so I think we sort of, I don't want to say inspired each other, but sort of pushed each other in this direction. It was already in the middle sixties when the emphasis went from optimal control to games, so it was the beginning of people talking about differential games, maybe five, six years into that era, and we recognized, and I think there's not great recognition, that if you can go to game theory, then you've already subsumed optimal control, because optimal control is just a single player game, much simpler, therefore, and you can get much more easily results, but nonetheless, we've simply gone into a generalization, and it also

opens up a larger field of application, for example, economics, among others, you see.

So that, I think, made an interest, and some of the people that I came into contact with—there was a man, a very good friend of mine now, Alain Haurie in Canada, when he retired or half retired—he taught at the University of Geneva until recently—who had also gone into this move, so, and there were others who sort of got transported into it. Almost all the people that eventually became either friends or collaborators or both, most of those have gone into some aspects of game theory, or at least as part of their interest, and there is Bill Schmitendorf, for example, whom I met when I gave a lecture at Northwestern University in Evanston, and that must have been in the late sixties, and eventually he moved to UC Irvine and became a professor there and retired a few years ago, and we began to look at one of the interesting things to do, both cooperative and non-cooperative games, in the—again, we’re jumping maybe a few years ahead, but in my association with another organization, in fact, earlier, was what the Italians call CISM.

08-00:37:55

Leitmann:

International Centre for Mechanical Sciences. It is a UNESCO- initiated and supported operation in Udine, a town in Northern Italy, near the Austrian border, in fact. Possibly through my contact with Italians in general, and being conceived in Italy, maybe that helped, I became interested in contact with CISM in Udine in the later part of the '60s, and I was appointed to the Scientific Council of CISM almost at the beginning. I was I think the only non-voting member of that council because the United States never supported it—I don't know why—but I was on that council maybe fifteen years, twelve years, something like that, and that got me to Italy and Udine, in particular, practically every year, and that was terrific, of course. Then in 1973, I gave a short course there, and by that time, we had gone from two-player games to n -player games. So now of course, you have to drop the idea of zero-sum, because there is no zero-sum, so it's a more complicated [problem]—and then in 1974, my lecture notes for that course were published by Springer Vienna. They just took the typed notes I had and essentially cleaned them up and published them in a monograph, a paperback monograph of some seventy-five pages and that was only four or five years out of the sixties, you know.

08-00:39:59

Burnett:

Right. I'm thinking about the field of economics, and game theory starts to make its way into that field. I'm not so sure about differential games. It's not until the early seventies, I think—

08-00:40:19

Leitmann:

Yeah.

08-00:40:19

Burnett:

—and so that would be coterminous with that move that you and your colleagues make. I think we talked a little bit about this off camera too, that

there is this work on control optimization that is strongly supported by the military, for example, but also by the NSF, and there are all these conferences and all these contacts are made, and then, at the end of the sixties, you have a number of things happening: one, man on the moon, so that the moon shot is achieved—there's that success—but also tremendous economic strains globally—the United States is facing a problem with the Bretton Woods system, the rising costs of Vietnam, the rising costs of the Great Society programs—and there's a real focus on economics and economic problems, and I'm wondering if that's maybe the larger context in which—

08-00:41:39

Leitmann:

Well, I'm not so sure. I'll tell you why. Again, I'm not a specialist or anything in that area, but it seemed to me that I'm not sure that there was any great interest in mathematical economics, I recall, on the government's side, and the whole idea, which of course permeates the politics of the system even today, is, there are these Nobel Prize laureates and whatever, who say one thing, but my gut tells me that that's not the right thing. I heard the president say that exactly the other day. And so I think there's always been a certain amount of—I wouldn't call it fear—essentially, an adversarial state between the academic economists and those who were working for the government, say, in the labor department. I don't think that many of those were mathematical economists. The whole trickle-down idea, right, under Reagan, certainly didn't have a mathematical theory.

So I think that there is, maybe even to this day, well even more so to this day perhaps, a certain schism between, for want of a better name, academic economists, and sort of working economists, running either departments or divisions of government departments, whether it's labor or—you know. I have a feeling that if you, and there are, of course, exceptions, but if you look at who really attained any kind of high position in the government part of it, most of them are not academics. I think there may have been more during the Kennedy administration, simply because he leaned more heavily particularly on Harvard, of course, a big school of economics. But generally, I think most presidents didn't cotton to that at all, and obviously still don't. And consequently, I think the support from the government was certainly much smaller for that research effort.

08-00:44:29

Burnett:

Oh, absolutely. So what I'm suggesting is that there is the state support of the research that you and others are doing in this whole world, and across all these countries, United States and the governments of these other countries as well, and then there's this shift in the early seventies towards other questions that are germane and that are in the public interest. Economic problems, ecological problems, these things become more important to a number of stakeholders. I'm not suggesting that the state then shifts money from rockets to economics, necessarily, but that the scholars are pursuing these questions, and if they don't get funded, they don't get funded. They're at universities, they get a

salary, so they're fine. So you begin to see in the list of conferences that you have, by the end of the sixties—there's the first conference on differential games and economics, for example, and papers in your list of publications with students and postdocs and colleagues on ecology, for example, and so these other questions begin to take over gradually in that time. So I wonder if you had any reflections on that, or if that's just something that was really organic for you at the time.

08-00:46:07

Leitmann:

I think it was organic, and it probably was of less interest and maybe consequence even, that since I was not at that time in a laboratory-type field, that it was essentially all theory, all paper, and the most money I needed was to support students, and maybe the computer center once in a while. So the amount of money that I needed and often got was peanuts to what people got, for example, in very applied or very expensive equipment fields. Talk about particle physics, for example, huge amounts of money going to that, and even that's surprising, because that's not of terrible interest to the politicians, but at least you have that. And so, I really never worried too much about that, whether I was going to get support or not, and I was very fortunate. Most of my postdocs were self-supported or supported by fellowships, so I put very little money into it. Just as long as we're talking about postdocs, I got a German postdoc in 1969, Wolfram Stadler, call him "Wolf," who became a very close personal friend. He and my mother adopted each other because they both knew Europe, and he was an interesting guy. He joined the US Air Force as a German in the late fifties, early sixties, and went to school under the GI Bill, [laughs] but he was a German citizen.

08-00:48:07

Burnett:

Why, I didn't know that that was possible.

08-00:48:08

Leitmann:

Oh yeah, I think it was possible, yeah, and he eventually I think did become an American citizen, and after three years, he was three years with me as a postdocs then eventually got a teaching job. He became a professor at San Francisco State University. So, I'm probably not as knowledgeable about why people went into particular fields, I think sometimes sort of just slip into them, and if they are amenable to you and you meet the people you want to work with, I think then you don't really worry too much about the impact your work has on things, and I—

08-00:48:54

Burnett:

But these are interesting questions, and the students came with research questions and begin to pull you in these interesting directions. Is that part of what happens?

08-00:49:05

Leitmann:

Well, in a sense, yes, I think students wanted to work in something that looked interesting and that they could contribute to, and I was very fortunate that I

had students. There were some students less helpful in that respect, but some of them outstanding when we go to talk about sufficiency theorems, or robust control, which we will do pretty soon. You will see that the students became extremely important because by that time, they had had a lot of the background training, but not necessarily the ideas, and so, that, it was fortuitous in a way. As I say, I can't repeat often enough that I think I was just very lucky, fortunate, whatever you want to call it.

08-00:49:56

Burnett:

Well maybe that's a good time to talk about the sufficiency theorem, and I think we talked, I think in an early session, in like session two, we talked about sufficiency and optimality. This is something that gets going at the end of the sixties, and maybe a way in—I'd like to get an understanding of sufficiency conditions and explain the difference between sufficient conditions and necessary conditions for optimality.

08-00:50:44

Leitmann:

Well, first of all, it's the nomenclature that gives you that. If you have a result, theoretical result, for example, well let's think optimal control. "Optimal," first of all, is a misnomer, and most people, almost to this day, and I learned that pretty quickly—you don't want to call a control that satisfies the necessary conditions "optimal," so in my books, I call them "extremal." And so I make a distinction between feasible to satisfy the constraints, and admissible if they satisfy the boundary conditions in addition, and extremal if they satisfy, so what some people call first-order conditions, are really not optimal until you have proved they're optimal, really, and that's very difficult in a way, but very simple if you make enough assumptions; and if you make enough assumptions, then probably you will not apply it to a lot of things. So it's a problem.

So, I became interested maybe for a couple of reasons. First of all, I came to that point where I was very careful to distinguish between extremal and optimal. I had already mentioned, in my first little book, there was a little section on when you can call things optimal, and it's interesting because if you satisfy the necessary conditions, and you can prove there is only one solution to this problem, you've done it, because if it satisfies the conditions of optimality and there are no other candidates, that's a fortuitous thing. It doesn't happen very often because proving uniqueness in mathematics is very difficult. So then that's when I began to think about, okay, sufficiency. Then I had a colleague—I'm not going to mention any names—and friend, who used to give me a hard time, because he said, "You know, if I satisfy those necessary conditions, if those aren't optimal," he said, "you've got a very strange problem. It's an outlier." And so, I said, "That doesn't make me happy. I want to know more, okay?" And there were a number of people, quite a few people, very good mathematicians who had, in looking at sufficiency conditions—it wasn't my invention. If you go to the literature and look under "sufficiency conditions," you'll find reams of papers.

08-00:53:43

Burnett:

But all around that time, all around the late sixties?

08-00:53:45

Leitmann:

Yeah, in the sixties. I mean, sufficiency conditions have been looked at, even in the classical calculus variations, but usually local sufficiency, which means, it's definitely a minimizer in some neighborhood of the optimal solution, but not what I call "globally," for all the possible solutions. That was really not looked at very much, and so that was probably about the middle sixties or something like that, and—

08-00:54:18

Burnett:

So this is just, to really put it in my lay understanding, when your critics were saying, "if they satisfy the conditions for the problem that you set for it, we can call that 'optimal' because of how we've defined it, and if there's something outside of that consideration, it's rare, or it's a complete outlier, so don't worry about it," effectively.

08-00:54:46

Leitmann:

Well, you can do better things with your time, in their thinking, and some of that didn't make me happy, and since I had raised that question in the first book in the sixties, when I wrote my second textbook which was '80 or '81, I tried to be very careful about that, to A), distinguish among these all different kinds of possible solutions, and B), in particular, not use the word "optimal" when it might not be optimal. And so sometimes when I gave an example, I could prove optimality; in other case, I said, "Well, I'm stuck with just having extremal solutions." That's very important, because it says that you can rule out a lot of candidates for what you're trying to do, if they don't satisfy the necessary conditions. So it's essentially an eliminator of candidates, and so that's the difference between necessary conditions and sufficient ones is, necessary conditions are necessary for what you are trying to have naming an optimal solution, and the sufficiency conditions are just that, they're sufficient to assure optimality—so, obviously, they satisfy the necessary conditions, of course, and then the question was to come up with reasonably straightforward sufficiency conditions. So I was very fortunate. I had Harold Stalford—I may have mentioned his name before. There were three or four graduate students who joined me towards the end of the sixties.

08-00:56:37

Burnett:

Was Stalford, H. L. Stalford—

08-00:56:38

Leitmann:

Stalford, yes, Harold Stalford came to me. He was from Oklahoma, and very nice fellow and we're still very close—in fact, we correspond—and then I had a student, a Chinese student, Wang, W-a-n-g, who worked with me on sufficiency conditions and so there may be one or two papers with him, and then, I had another student who came to me about that time from Belgium, François Litt—we are very close friends now, and have been for a long time—who came to me from the University of Liège, in—

08-00:57:22

Burnett: Belgium.

08-00:57:23

Leitmann: Yeah, in Belgium, and he was interested in dealing with constraints and in particular, up till that time, most of the constraints we had dealt with were constraints on the control, but sometimes there are constraints on the state, the actual variable describing the system. For example, you want to avoid a certain area, so you say, "That's a forbidden area," or, you want to make sure that they don't stray too far away from what you can achieve, so, you put constraints on it, and he did some of this work and then his dissertation was really on state constraints, as a basic topic.

08-00:58:25

Burnett: And [William] Schmitendorf too, right?

08-00:58:27

Leitmann: Yeah, well Schmitendorf wasn't a student. Schmitendorf was, he was a professor, and eventually came, as I said, to Irvine. I wasn't really until the seventies that some additional students came in, and they were more interested in application. I had one student who worked on cooperative and non-cooperative games, Saul Rocklin. Then another student who, by that time, we had started to talk about avoidance control—in other words, how to avoid collisions, that kind of thing—and he worked on that. Agarwal, he was an Indian student, and at the same time, I got my first Israeli student, Shaul Gutman. He came to me just around 1970, and his main interest was to protect Israel from its enemies. So he was interested in control problems, and particularly games, pursuit evasion games, for that reason, and so he started to work with me on that. He was very good. These are all people with whom I'm in close contact. In fact, people like Gutman and Litt, I see almost every year, and some of them come to visit me here. Gutman comes every year. And so it's also on a—

08-01:00:06

Burnett: It's like a family.

08-01:00:06

Leitmann: —yeah, on a personal basis. This is a terrific plus, of course.

08-01:00:13

Burnett: And so they're—where am I here? Let's see.

08-01:00:19

Leitmann: Yeah, we were still on sufficiency condition.

08-01:00:21

Burnett: We were still on sufficiency, yeah, and so they're kind of expanding the number of constraints, so the number of things that can be considered constraints. So the state was unconstrained in the older model, right?

08-01:00:42

Leitmann:

Well, it's dynamic now, yeah, and so it's a function of time. We always say "time" but it could be something else. It could be distance. And the constraints say that the candidates you have for what you're interested in, are in some way constrained, usually by size, or by sort of constraints that say they can't wander into certain regions. You know, if you want to avoid a region, you have state constraints that say, "They must be"—for example, "greater, less, or follow some contour in terms of where they can be," that's sort of thing. Those are very interesting problems, and they are much more difficult than the control constraints, much more difficult. Even from a theoretical point of view, still unsolved problems in that area, because, well, control constraints, you have control. State constraints, you don't have control. They're constraints that you can't impose if you can't bring them about, but with control, you simply say, "The controls that I'm going to use to control something, I will pick out of that set. That's up to me."

08-01:02:14

Burnett:

So on—

08-01:02:14

Leitmann:

And the state constraints are very often not up to you.

08-01:02:18

Burnett:

Some of this, in game theory, really takes off to deal with conditions of uncertainty, right? They want to understand—

08-01:02:30

Leitmann:

It could be, yeah. As a matter of fact, when Gutman and I started looking at the business of uncertainty, which just about that time came up, and classically, sometimes, there were two or three other people who looked at these situations—well, first of all, you want to divide the realm of the kind of math you want to use to deal with uncertainty. Are you going to talk about deterministic, or random uncertainty? So are you going to deal with statistics? And that, of course, means that the kind of distributions you have for your uncertainty might be normal.

People very often say, "Okay, it's a bell-shaped curve, with the outliers. First of all, I'm not a statistician. I can deal with situations where I can say that I can impose the bounds of the uncertainty by simply saying, "How much of it can I stand under the worst of circumstances?" So I, say, in earthquakes, I'm not going to get myself a situation where I can take care of every earthquake, but let's say I'm not interested in doing it beyond, I don't know, nine on the Richter scale, or ten on the Richter scale, okay? So then you can say, "I can assure you that if I have a theory which you trust, I can assure that we are controlling against this, taking care of it, up to a certain point. Beyond that, all bets are off."

So these X-events that we mentioned briefly don't fall into this category at all. You could look at it another way. I could look at it statistically by saying, "If you have just magnitude constraints, you're dealing still statistically with a distribution which is simply a rectangle."

08-01:05:01

Burnett:

You're dealing with it.

08-01:05:01

Leitmann:

—you start dealing with it, and then it's also distribution, but it's one that you impose, and it's very often very conservative, if you really want to deal with situations. So then—I think we touched on that briefly—if you are risk-averse, you are more apt to do that kind of thing, namely, dealing with the worst situations, and it depends on what you can risk.

08-01:05:35

Burnett:

Well, this is highly abstract and mathematical, but if we're talking about the field of engineering, say, civil engineering, we're talking about designing to thresholds. So something is built for a purpose, and the purpose is set. It's conceived, and the things that could happen to this structure are decided in advance, so it's protected. A bridge is meant to extend—

08-01:06:06

Leitmann:

Yeah, and it changes with time. For example, building codes are of that class, and they're constantly being, one hopes, being made stricter, as we gain more knowledge about what can happen. I guess I mentioned the Chernobyl disaster, and the sort of, where you deal the statistics, and you don't think of something that might happen, so it's not included in the distribution. But if you say, "That's the limit, and I know that if I can control to that limit, I'm not controlling beyond that." Obviously, I can't give you a guarantee, but at least you know, given that your theory describes the system well enough, that you've assured, in quotation marks, that you're okay, and you pay a price for that, and of course, and the price is more costly, more constrained. So I think in my own case—I've thought about this more now that we've raised the whole question—that I'm generally more sympathetic to worst-case designs simply because I'm trying, and I'm always—I said this before—over insured.

08-01:07:38

Burnett:

Well I think we've said it before that you have lived worst-case scenarios, in a way, right?

08-01:07:44

Leitmann:

Yeah, certainly, I've learned by history. I've learned by temperament, I don't know what, and lots of things go into what you do, but you want to avoid certain situations simply because the consequences are so disastrous.

08-01:07:59

Burnett:

And your concrete experience even as a child with the certainty of those in authority that, in your own family, and in the community, that, "this is going

to blow over. We've been in this situation before. It's not normal, but it's within the bounds of what we understand," and it went so much further than that, and I can't help but think that that shaped you in how you think about things.

08-01:08:29

Leitmann: Yeah, and it makes you leery.

08-01:08:31

Burnett: Yes, absolutely, that—

08-01:08:33

Leitmann: Yeah, no question about it. So, these things don't come out of the ether, and I haven't given it a lot of thoughts. I've thought about it, of course, over the years, but I didn't really, I don't want to say waste time on this kind of thing, [laughter] no, but I have a little bit more time to think now, and I'm pretty certain that uncertainty was an important part of the sort of things that I looked at, yes.

08-01:09:05

Burnett: So across these areas of inquiry, and techniques, and subjects, there is an effort to account for uncertainty or bring uncertainty into the equation, if I can describe it in that way, and this is—I hate to jump around from. We were talking about sufficiency before, but when you were talking about games, you had said earlier off camera that Lotfi Zadeh had helped you think about fuzziness in games, so instead of rationality, you have membership functions. So, is that another example of this kind of moving from this control, and if we're talking about optimization, or these sort of step-wise decisions in game theory, to something more unpredictable, something more complex?

08-01:10:16

Leitmann: Yeah. Well, I think, and so I've penetrated into fuzziness, it's certainly an awareness and a way of dealing with uncertainty, and dealing with uncertainty in a fuzzy way, even though people are wrong if they say, "It's just statistics," because distributions and membership functions look alike, very often, but again, I think in both cases, and Lotfi and I talked about this a lot, what is the applicability? Now, it's very nice and important and a beautiful theory, for example, for language, you say, "What is the class of beauty?" So you can look at a person and say, "He's a number two in beauty, that's not very beautiful," or, "She's a ten," that kind of thing, but to me, and he agreed with me really, for that kind of linguistic uncertainty, both theories are really prescriptive, because who is going to make up the membership function, or the distribution in the statistical case? Now, in the linguistic area, just take beauty as an example, the attribute, his feeling was that you can survey, do this by survey. You can ask people, "Here is a picture of a beautiful woman or less beautiful woman, ugly woman. What number would you give her?" But that's sort of makes it—again, it's a little bit like survey-based medicine. It's not—

08-01:12:40

Burnett: Or behavioral economics.

08-01:12:41

Leitmann: Yeah, it depends on whom you ask, and statisticians always feel they have certainty plus or minus something. I don't really believe that, because their samples are still selected, and they're sometimes not very large or representative, so this is why we have not predicted outcomes of elections extremely well, I believe. So, for example, Lotfi's became extremely famous, of course, deservedly, all over the world, and the Japanese particularly like the—you will see ads to this day that say, "This vacuum cleaner is designed using fuzzy set theory—"

08-01:13:24

Burnett: Right, yes, yeah, that's right.

08-01:13:26

Leitmann: Well, then somebody—and this a question of setting a thermostat, what number do you put on thermostat if you wanted to cool, less cool, pleasant, warm, warmer, hot, that kind of thing? Well, you can then, as a designer of thermostats, simply arbitrarily put that as a membership function, and then you can design the thermostat—

08-01:13:54

Burnett: And that's fuzzy logic.

08-01:13:56

Leitmann: Well it's fuzzy logic because the person will see on the thing, the do it warm or cold or whatever, and they will set a given number, but I don't think that has anything to do with the actual situation, because it's essentially a linguistic problem, made numerical.

08-01:14:18

Burnett: Right, right. It's normative.

08-01:14:21

Leitmann: Yeah, so it's not descriptive. In other words, it won't tell me to get a warm room or a cold room because you haven't even decided yet what's warm and cold, so, no. So it's that dichotomy, and it's true in game theory, because rationality doesn't really exist in the real world, and so, what you assume in a game to model the real world, I think, is very, very iffy. Now, if you are, even a bureaucrat, I'm not even talking about dictatorships, but a bureaucrat who will say, "How do we allot money to do certain things, and I think this is a good thing that's a bad thing," well, you already essentially made up the membership function in your head, and you've made up actually the whole idea. The classical rationality-based game theory may be prescriptive, because it says, "If people are rational," or whoever, the players, "then this is the best thing to do," but as soon as you deal with a real situation, you can't say, "I'm describing what's going on."

And so I think there's a very big dichotomy between whether a theory is descriptive or prescriptive, or what has to be put more into the theory to make it descriptive, and people have done that of course. Now [Reinhard] Selten and people after him, such as Richard Thaler, have tried to put psychology into the process, and that makes it much more useful as a descriptive tool, but until that point, when we just say, "Okay, we'll get a Nash equilibrium that's optimal, if—" And so, I think that, and I'm not really sure how much people think about that, particularly in the political arena, or even in the operational part of the government. This is why I think what is so attractive to people in decision making is that, you know, drawing a curve like that and it's visual, but it's—

08-01:16:54

Burnett:

It *seems* utilitarian.

08-01:16:55

Leitmann:

Yeah, but it's very dangerous, I think, the trickle-down theory of economics is sort of based on that, and then when you add how what you got makes you make a decision, then you are really in trouble, and you see this, I think, every day. It proves to me how important the meaning of words is, and how they are misinterpreted or misused, either on purpose or not, and how people really don't take irrationality into account. They think that these people know what they're doing, and there's a lot to be done yet.

08-01:17:47

Burnett:

Yes. Well you're working; at this time in the late 1960s, you're trying to make abstract models of a problem that requires some solution towards optimality, and we've already talked about how we have to be careful about how we define optimality, and you and others are trying to introduce both more precision and more caution in terms of what is being defined, and whether or not this can bear any relation to something in the outside world. If you're going to let something unfold in time, then you have to use different techniques to reflect a continuous change in how these variables change in relation to one another, and then you've moved from what is an enormously complex system, a rocket, and within that, you've looked at the depletion of the mass as the rocket spends its fuel—that's an enormously complicated math problem—and you moved into something like economics, or something like ecology, where you've got interdependent relationships among all of these variables, and that's something that's happening across the board. You're trying to develop new tools and take old tools and repurpose them.

08-01:19:23

Leitmann:

Well I think a lot of it has to do with the people I worked with, when, for example, in economics, I was very lucky to work with and become friends with, who was then a professor at UC Davis, Henry Wan, and then his wife is also an economist, different name, Simone Clemhout, and he eventually became a professor at Cornell. That's where he is now, emeritus. I can't say he taught me economics, but at least he made it palatable to me, and he would

provide primarily that part of what we did and I would try to do the control part, but he learned that also quickly, so that was fine, and I'm just looking at, well, applications, for example. Yeah, there it is. So, I took economics first because we started early with that, so with Henry Wan, I'll just name the title of the paper: "Differential Games of Duopoly," "A Differential Model of Oligopoly." [phone rings. break in audio]

08-01:21:06

Burnett:

So, we had some interruptions there, but, so, we were talking about the collaborations with Simone Clemhout and Henry Wan, and a number of papers you did in the early seventies: "Differential Games: a Model of Duopoly," "A Differential Game Model of Oligopoly," "Equilibrium Patterns for Bargaining Under Strike: a Differential Game Model," and we were talking off camera about this collaboration as something that is illustrative of a shift in your approach from studying optimality to thinking about stability, and stability theory. Can you talk a little bit about the difference between optimizing something and rendering something stable using stability theory?

08-01:22:05

Leitmann:

Sure. Well, I think in the kind of optimality that we're talking about, and this is particularly in the control part, maybe less so in the game part, there's usually some quantity one wants to maximize or minimize with constraints. I think with stability what you want to do is control the system, and this, again, could be in a game sense or in a control sense, so as to do some specific thing in terms of the evolution of the system, for example—

08-01:22:49

Burnett:

To grow something, right?

08-01:22:51

Leitmann:

Or to approach a certain—or even more importantly, to stay within some bounded region, whatever the variables you are. So maybe you want to control the system to keep unemployment below a certain level, and if you now use game theory, then there may be a number of things that you want to control that way, and they may be in conflict with each other, and then you immediately have the game. You can say, "I cooperate," or "I don't cooperate," and there are many variations of those games, or you have a Stackelberg game that we just barely mentioned, where there's a leader and a follower.

So, the stability seemed to me more important, if you want to go to importance, because lack of stability, particularly in some physical systems, but it's, of course, obviously true in economics too, leads to real disasters, whereas as soon as you have more than one goal in mind, then you are immediately going to, of course, a game, and then you can look at multi-criteria optimization where you have a vector-valued cost function—you want to maximize profit and minimize pollution, etcetera—and those things can't happen simultaneously. So you have, in addition to the game and the ordinary

control problem, you have multi-criteria optimization, or control, multi-criteria control, where just by the fact that the different controllers, or the different interests you have to control are not compatible, and so, you have some kind of interchange, and that's where game theory comes in as well, of course.

08-01:25:10

So it isn't necessarily that you have conflicting players. You may have conflicting goals, and mathematically, these things very often can be handled the same way, although in effect, they don't deal with the same situation. And so, it just became more interesting, since we're going back to, why did I go the other way, because when we first looked at it, we looked at uncertainty in a system really as a game against nature, allowing for your opponent doing the worst to you. Again, with worst-case design, you're trying to do one thing and uncertainty, under the worst circumstances, will be a player that plays against you. I turned out that the theory actually is much simpler if you deal, and I'm leaving statistics out now, with the kind of thing that I very soon came to: namely, worst-case situations where I put bounds on what I can live with—you know, an earthquake up to Richter ten scale, or whatever—and sometimes these bounds are imposed not by what I can accept but because they're just there.

So, it wasn't a planned thing, that's really what I'm saying. It just sort of grew, and having the right people to work with had a lot to do with it. I want to say, right at the outset, when we got to stability, probably my main student was Martin Corless. Martin Corless, extremely bright, now he's a professor at Purdue, and with Stalford, who is very bright. It is important when writing a paper, to make it clear who was the senior author, right? My name came first in the alphabet but if his name appeared first on the paper, I wanted to make sure that people understood there's a reason for that. With Corless, that wasn't so clear, because his name just happens to be [laughs] lower on the scale, and so, you don't know. Is it because his name came first alphabetically? I want to make very clear that there are few papers where my name comes first. In all the other papers, when his name stays first; there's a reason for it, because I really feel that—

08-01:28:27

Burnett:

First author.

08-01:28:28

Leitmann:

Yeah, how big the differences was is a question of measure, but I wanted to make sure that people understood that his name is there first because I consider him to be the leading author, and that may be the majority of the papers we wrote together. So, I think that's important to state in writing history.

08-01:28:52

Burnett:

Right. No, absolutely. Well, you're deeply invested in your students, throughout your life. It seems most of the people, to the extent that they were in good health and alive, they've maintained contact with you. You see them on a regular basis. It's quite rare, I think, for someone to have that many former students—

08-01:29:17

Leitmann:

Well thank heaven for email. [laughs]

08-01:29:19

Burnett:

Right, right, communications technologies have advanced and assisted in this, but that's also, I think, a testament to your commitment to them, and their fondness for you. So I think we were talking about how I was struggling with stability theory in economics versus a control problem or an optimization problem, and I think you explained really clearly that, if you have a certain objective in your system, that would be stability theory, where you would need to maintain unemployment within a certain range, and everything else is built around that, whereas optimization is setting a maximum or a minimum for different constraints, and, well, I'll let you clarify that.

08-01:30:35

Leitmann:

Well, what you want to do, in optimization, is to have some goal in mind. It may be numerical, or it may be just, as in, for example, the pursuit- evasion game. One wants to get away, the other one wants to catch the one that gets away, so we have opposing aims. That makes it, in that case, a zero-sum game, of course, because what one loses, the other one gains. The one gets away, wants to maximize the distance; the other one wants to minimize it. That's different from a stability problem where you say, "I want to control, to keep the system either within bounds, or go towards a certain point," and you can do it under uncertainty. This is why people have thought, at some time, that uncertainty is your opposing player, if you want to be risk-averse, and so these things are not completely separated, of course, and there are some people who do stability of optimized systems, that kind of thing, and I'll just give you one example that has to do with something we haven't talked about at all, is the information that you use, and that's not my forte, but the variables that you rely on to give you information about the control that you want to do is, for example, classically, the calculus variations. It was just time, or the independent variable. I'll call it "time," okay?

Then, people became more sophisticated and said, "Why not base it on more information than time?" And I think I may have given you the little anecdote with Professor Letov, a very famous mathematician, Soviet mathematician. I was walking with him in the Imperial Garden at Laxenburg where IIASA is, and we were talking about what's called open- and closed-loop control. Open-loop control, you just say, "It's three o'clock, I do this; at five o'clock, I do that"; and closed-loop control is, you look at what comes from the system, the state of the system, and you use that also as information. So you can say, "The

variables that the control depends on is time and state,” and then you can go beyond that and say, “only part of the state, because I may not be able to measure all of the state components, or the state is very uncertain,” and those are refinements. He said, “Well you know, as an example of open-loop control where we only look at time, take our five-year plans,” he says, “in the Soviet Union it doesn’t matter what’s really going on; everybody says, ‘Okay, it’s five o’clock, I do this, and tomorrow, Saturday, I do that.’”

08-01:33:52

Burnett:

Yeah, give me a million nails in the next year.

08-01:33:56

Leitmann:

So, that’s an important aspect of course, but the example I was going to give you is, it was always sort of accepted that the more information you have, the better. The given system you have modeled mathematically, now if you could use time as the input, that’s fine, but you can also use, say, the current state of the system as inputs. More information, that should give you a better thing, and I came across somebody else’s example, but I came across it and it’s in my book, the second book. Suppose you say, “Okay, I’m going to use more information, time, and the state of the system. Then using that information, I want to optimize the system.” For example, in that example, you want to drive the state to zero in minimum time, so in a sense, you have also introduced stability, but that’s your goal. And it’s interesting: you get an infinite number of solutions, and they all say, “You do this, and then you jump to doing that,” so there are discontinuity surfaces that you cross to go from doing the maximum to doing the minimum, or conversely. It’s as in the rocket: you put the thrust to maximum thrust, and then you let the rocket coast for a while, that kind of thing.

Suppose the system has just a little bit of uncertainty. In other words, there’s a delay. You can’t measure time exactly, or the state exactly, so in fact, I think I’m still on this side of the switching surface, but in fact, I’ve slipped to the other side of it because there’s a little perturbation in the system, and suddenly from minimizing the time to get to where you want to go, you make it infinite, you maximize it. Why? Because the system will go across and suddenly realize it’s gone too far, and now it’ll go back and again have that little delay. It just dithers back and forth—

08-01:36:33

Burnett:

So it’s kind of a feedback system.

08-01:36:35

Leitmann:

Yeah, well that’s right, and so instead of going nicely and then switching again and switching getting to zero eventually, it just stays at the point where you’re switching from one type of control to the other, say from maximum to minimum, but you have a little error that you actually have slipped into the wrong region, and now you dither back and forth. It means you never get away from that point. So in fact, you’ve maximized the time. You’ve, in fact,

made it infinite because you never get to where you want to go. And so you have to be aware of that kind of thing, and that's a stability problem, you see, because it's kind of functional instability. It's the system, by allowing uncertainty, to tell you that you have got a good system, or it is not a stable system, because a stable system means, "If there's some perturbation, you can deal with it," and in this particular example, no matter how small the perturbation, as long as it's finite, you'll run into this difficulty, of dithering back and forth.

08-01:37:56

Burnett: So that's an inherently unstable system.

08-01:37:58

Leitmann: It's essentially an unstable system, or, and you can, if it's simple enough, you can decide when that's a possibility. Not every solution that you have will have that difficulty, but when you look at the particular solution you are looking at, you want to be aware of this kind of possibility of, in fact, going completely crazy. If the system were perfect, that couldn't happen.

08-01:38:32

Burnett: A well-designed system, we were talking about this earlier with respect to Chernobyl, but a well-designed nuclear reactor can have something like a negative feedback system where, if the system starts to overheat, it reaches a certain heat threshold, and it trips an automated shutdown. So it'll automatically shut down the reactor in a safe manner, but in the case of Chernobyl, the system was designed to work automatically, and they took it off automated control and introduced a human controller—

08-01:39:08

Leitmann: Yeah, and that was the little delay, you see, we're talking about.

08-01:39:11

Burnett: Right, right. Yeah, so that's clear, so, but you could have an optimization problem and a stability problem with respect to the economic, for example. Let's say, your stability system is, you want 2 percent economic growth per year. So organize your system so that that is the stability, but there's also maximum and minimum constraints, so you want to set a maximum threshold for unemployment, and a maximum threshold for inflation, and there's a relationship between them, and you have other variables, right? So that can be an optimization—

08-01:39:54

Leitmann: Yeah, well that's a very complicated system, of course.

08-01:39:56

Burnett: [laughs] Yeah, so, a very complicated system.

08-01:39:58

Leitmann: And in addition, of course, you have uncertainty built into the system because you're dealing with people, you see. So that's a system that you can never

think of as a perfect system. So any theory that is based on perfect information is suspect, and you have to be careful. See, I think that's the lesson, and so you have to depend on your gut. [laughs]

08-01:40:26

Burnett:

Well we seem to have a mixed system. We have a market society which, and we treat the market as an information processor, but we have these other, crude mechanisms of governance where we have a money supply, we have an interest rate, and we have a rate of inflation, and those are fairly crude things that we manage through a central bank, or a federal reserve, or something like that.

08-01:40:51

Leitmann:

But you see, it's very hard to determine in a system of that kind, how far you can go, because of uncertainty, and then you have the human uncertainty, and then you have the historical uncertainty of events happening that you don't have any control over, whether it be a terrorist attack or global—

08-01:41:15

Burnett:

Supply of oil.

08-01:41:16

Leitmann:

—or global warming. We really don't know what this is all going to—because we can't really say how bad it could be. It's just what you do know as a worst-case designer is, that you'd rather do something than say, "Maybe that's not what's going to happen," because again, it's one of these things where the consequences are just too great to take the risk. So we have to be risk-averse, is what I'm saying, and of course, the people who are against that say, "No, what's important is that unemployment is low." You know, it's—

08-01:41:55

Burnett:

Yeah, quite literally—

08-01:41:56

Leitmann:

And anyway, there are also people who live in the present not the future. "Hundred years from now, who knows?" they say, "who knows?" They say, "Technology will take care of it." Maybe it will. That's certainly possible. The question is, can you take the chance?

08-01:42:14

Burnett:

Yeah, and fix things in time.

08-01:42:16

Leitmann:

Well, that's right.

08-01:42:19

Burnett:

Right, now we're getting into the early seventies, so before we leave that entirely, there are some—we talked about the density of travel that you undertook in the sixties. You were taking multiple trips per year around the world, going to conferences and meeting folks, working on these problems,

and it accounts in part, I think, for the rapid migration of your interests and switching gears from one set of techniques to another and one field to another. But you also, you did have some time for some personal travel, and I'm wondering if you could talk about some of these trips that you did, some side trips.

08-01:43:09

Leitmann:

Well, I can take a couple of particularly interesting examples, for example—for example, examples—there was the meeting in Akademgorodok. I think it was in '68.

08-01:43:25

Burnett:

Yeah, and we did talk about that, I think, didn't we, and you took a side trip from there?

08-01:43:29

Leitmann:

Well, before, Blaqui re and I had papers at that meeting, and we decided we were going to take a trip to Uzbekistan, because that's sort of on the way, and it was interesting because travel, it was very controlled in the Soviet Union, so, whatever travel you booked had to go through an agency called Intourist, which would book the hotels for you. They would make sure that you had the flights and all that. So they knew exactly where you were, I think it really had to do a lot with that. And so it's very surprising. We decided first of all to go to Uzbekistan which, even then in '68, was still a very remote area, and so we made all the arrangements. There was no email in those days, so Blaqui re spent a lot of time on the phone, talking to our travel agents, got in touch with Intourist, and we decided we were going to go for about ten days, a pretty long trip, before going to Novosibirsk, which is the airport next to Akademgorodok. So we met in Tashkent, the capital of Uzbekistan. He came from Paris, I came from Berkeley, and it was the year when they had the big student riots in Paris—

08-01:45:05

Burnett:

Mm-hmm, '68.

08-01:45:05

Leitmann:

—'68, yeah, so he was leery about doing this, but we had arranged it all, and we met in Tashkent and got the same hotel, in fact, a room together, and he was immediately going to try and call Paris to make sure everything was okay. First, there was no telephone in our room, but I noticed there was a telephone outlet, two little screws, and so we got in touch with the management, and they said, okay, they can install a telephone for us, but it was a woman installer actually, who didn't have a screwdriver. So when she attached the little wires for the telephone—this wasn't the plug-in switch, this was just the two eyes—she used her fingernail, and she, [laughter] she tightened the screw. Fine. Anyway, the lines were completely tied up. There was no way to get to Paris, so that was just a worry for the whole trip. So then from Tashkent, we

went to Samarkand, and then to Bukhara, and then back to Tashkent. So we spent three or four days in each of these cities, which was really terrific, and—

08-01:46:29

Burnett: What were the striking differences for you? Was it—

08-01:46:34

Leitmann: You mean in the—

08-01:46:35

Burnett: In the city, in the streets, and you've got—

08-01:46:37

Leitmann: Well, first of all, it's a culture which is not really European. The religion is, of course, Islam—just absolutely wonderful buildings—Islamic art is, of course, very geometric—and tile work, and just beautiful stuff, and we were able to visit the grave of Tamerlane, called “Timur” in Uzbek—I have a friend now who came from Uzbekistan who is a professor at University of Hawaii, but that's another story—and it was fascinating.

So I think it was in Bukhara where there were a lot of people from Mongolia. There were oil and gas fields in that area, and most of these workers were in from those fields. One evening, when we went to the dining room in the hotel, it was completely full with these workers from the oil fields and the gas fields, and we found a table where there were two of them, and there were four seats. So they sat opposite each other, and Blaquièr and I sat opposite to each other, and they had a bottle, it wasn't vodka, it was cognac, sitting on the table. There was only about this much left in the bottle, and they just, each took a swig about every couple of minutes, and suddenly, one balled his fist, and just hit the other one straight on the nose. That started a riot all over the place. We were the only Westerners, but the waitresses were terrific. They were these really strong Soviet ladies. Within ten minutes, they had thrown all the people out of the place, but that was a huge fight. So there's this guy here that then somebody else hits somebody, and everybody was drunk.

08-01:48:39

Burnett: And it was like a movie, [laughs] this seems like.

08-01:48:41

Leitmann: Sort of. On the other hand, in Samarkand, we were going around town; it was wonderful—we didn't have any guides—absolutely, I must tell you. Nobody followed us. We were completely free to do what we wanted to do, except we had to stay in that hotel, of course. So, we got to our hotel, and it was just around noon, and we went into the dining room. It was beautiful. All the tables were set up, and there was the flatbread there, and there was the mineral water standing there. So we sat down, and started nibbling on the bread and having a little mineral water, waiting for a waiter to come—that's another big story in the Soviet Union—and suddenly, a bus pulled up in front of the hotel,

or the dining room, and I looked out, and it was a tourist bus from East Germany.

So they were tourists, East German tourists. We had not met any other tourists. We had met one American. And so, the leader of the group marched up to our table—we were nibbling on their bread, I guess—and he clicked his heels together, as they do in Germany, like that, you know, and he looked down and he just said, “Here sit Germans,” in English, and I had this wonderful experience that I had before, you know, in Épinal. I looked up at him and I said, in German, “I’m afraid,” I said, “I don’t understand a word you’re saying.” He left us alone. We had our table to ourselves and we nibbled on their bread and drank their mineral water. [laughter] So, it was a great trip.

It was really terrific, but then, flying from Tashkent—we met again and took the plane from Taskhent to Novosibirsk. It was trimotor, very much like the Ford Trimotor, almost the same plane, and you sat across the people, and there was a little table, even with a lamp on it. It was wonderful. Well, sitting across from me was a lady from Siberia, and she had a baby, and the baby was crying before takeoff, and so I made faces at the baby and stuff. She handed me the baby, and for the next four and a half hours flying, I had this baby with me. [laughs]

08-01:51:19

Burnett: For your sins. [laughs]

08-01:51:20

Leitmann: Just one other one is, once in Georgia, I flew—

08-01:51:26

Burnett: As in Tbilisi, Georgia.

08-01:51:27

Leitmann: Yeah, Tbilisi, that’s another trip that’s coming up, those trips, but we were flying from Tbilisi to Moscow, and in those days, internal flights in the Soviet Union were very inexpensive. The food situation was very bad. The distribution, the organization wasn’t good, and so peasants actually found it worthwhile to take their produce by plane to Moscow. And I sat next to a Georgian farmer, peasant, and on those planes, the food was minimal. You got a little bag, and it had black bread, and a pickle or gherkin in it, and that’s about it. But—

08-01:52:26

Burnett: For four hours, right, or—

08-01:52:28

Leitmann: Yeah, so, but he had a gherkin and it was minimal—you know, I had a nice gherkin, so I gave him my gherkin. I was a friend for life! On the other hand, these peasants were trying to get on the plane to be able to take their produce

to Moscow, and then the plane was full and they just closed the door. These guys were knocking from the outside trying to get in from—

08-01:52:56

Burnett: Wow.

08-01:52:57

Leitmann: And one other one: I once flew from Moscow to Leningrad on an internal flight again, and they had overbooked. So instead of keeping people out, they took the seat divider out and put a third person in there. [laughs] So, it was a little uncomfortable. Anyway, so—

08-01:53:21

Burnett: Incredible adventures.

08-01:53:23

Leitmann: I'll think of other things later.

08-01:53:24

Burnett: Yeah, yeah. Well, we should perhaps pause now, but make sure to tell us stories. Let's continue with the travel motif—

08-01:53:34

Leitmann: A little bit, yeah—

08-01:53:35

Burnett: Those are fascinating stories.

08-01:53:37

Leitmann: Yeah, well, one of the really most wonderful things is just the number of side trips, and then after a while, see, my mother retired and came to Berkeley in '69. We had her for twenty-five years after that at Berkeley. It was wonderful. So, Nancy was no longer insisting on staying home when we took trips, and from then on, for most trips, she came with me.

08-01:54:15

Burnett: Wonderful.

08-01:54:18

Leitmann: Sometimes when it was an exploratory trip, then on the next trip she would come along, and she never went to the Soviet Union. And we talk about this, Nancy and I, just how it's just absolutely fantastic, every time we see something on movies, we say, "Oh, we've been there," [laughter] or on TV. Yeah, it was really amazing.

08-01:54:44

Burnett: Wonderful. Well let's continue next time.

Interview 9: December 13, 2018

09-00:00:15

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History Series. This is session number nine, and it is December 13, 2018, and we're here in the Berkeley Hills once again, and we were in the middle of beginning to talk about stability theory, and this move from looking at optimality to looking at stability under conditions of uncertainty. So, when we were talking off camera, you talked about this domain of research being about the deterministic control of uncertain systems, and I'd like you to set the context for that a little bit by talking about, by contrasting it with other domains of inquiry, other ways of gathering and formulating knowledge. What kind of knowledge is this, and how is it set apart from other forms of inquiry?

09-00:01:23

Leitmann:

Well, there are, I think we started to talk about this, essentially two, and more recently, three main avenues to go to modeling, and then handling the information, in this case using control, to account for uncertainty in a system. Now the uncertainty could be either because you just don't know all the details, and in fact, any mathematical model, it's uncertain because you can't model everything—first of all, you may not think of all the things that occur in your system, and secondly because it's just mathematically unwieldy to do that, and you don't have enough information, in fact, for lots of systems. Now for systems for which there is a lot of information, for example, in manufacturing or product—you're making nails, and we've done that for a long time, or automobiles for that matter, or whatever—statistical methods, which is one way of dealing with uncertainty. How many units will fail, for example, to use statistics has been obviously wonderful, and fairly simple systems usually have some kind of normal distribution which is the basis on which you take care of probability of certain events happening at a given value of the independent variable, whatever it is, maybe time, maybe something else, and they are obviously very useful and they've been often normal, and so, the mathematics becomes fairly easy. So it's probability theory that underlies that approach.

There is also another way of handling uncertainty or unknown elements in the system, and that's fuzzy logic, which has, at least on the surface, certain similarities with statistics. So instead of distributions, there are membership functions, but they're basically a different idea, and then there are deterministic ways of handling this, and you can look at that statistically if you want too; I think I mentioned that before. You say that you have a certain level of uncertainty, say in the parameters, and then you go from the engineering point of view that says, "I will simply design this system up to a certain maximum uncertainty." We do this with housing codes, dealing with earthquake engineering, where we say, "We can stand an earthquake of size ten on the Richter scale." Doesn't mean that it will never happen that you get a twelve, but the probabilities are very small.

09-00:04:47

So, that leads immediately to the idea that all you do, simply, with the different variables that you deal with and that you want to take uncertainty into account, by simply putting an upper and a lower limit on those variables. In the case of control theory, we started out by putting limits on the minimum and maximum values of the control variables. Say, in case of a rocket, where you have the thrust on or off. These constraints, but they're imposed by you. This is what happens in fuzzy logic for prescriptive problems. It's a little bit less successful if you want to describe what actually happens, and that, of course, happens with statistics because what happened in the past really doesn't predict the future, particularly in today's world.

So it seemed like a safe thing to, for me, for the other reason is that I'm not a statistician, to try to handle it that way.

[Narrator deletion: 09-00:06:16-09-00:15:15, rewritten as addendum below]

09-00:15:15

Burnett:

So what was Lyapunov's—what did he solve, or what did this give you that wasn't there before?

[Narrator Addendum: We consider a system stable if its state $x(t)$ “reaches” an equilibrium state, say $x = x^* = 0$, i.e., any given neighborhood in finite time. For us $x(t)$ is a solution of the state equation

$$\frac{dx(t)}{dt} = f(x(t))$$

To discuss this question, stable or not, we choose to employ the method of Lyapunov which is based on the use of Lyapunov functions, say $V(x)$. Given certain conditions, it can be shown that

$$\frac{dV(x(t))}{dt} < 0$$

for all solutions, assures stability. E.g., consider the simple system

$$\frac{dx(t)}{dt} = -x(t)$$

$$V(x(t)) = x(t)^2$$

so that

$$\frac{dV(x(t))}{dt} = \frac{dV(x)}{dx} \frac{dx(t)}{dt} = (2x(t))(-x(t)) = -2x(t)^2$$

Thus

$$\frac{dV(x(t))}{dt} < 0 \text{ for all } x(t) \text{ (except at equilibrium } x^* = 0).$$

Now let us consider an uncertain system, i.e. containing uncertain elements as mentioned earlier. The problem is to find control which assures stability in the sense of Lyapunov allowing the worst case, namely, maximum uncertainty. For a controlled system with uncertainty

$$\frac{dx(t)}{dt} = f(x(t), u, v)$$

where u is control and v is uncertainty, we seek u which assures stability under worst uncertainty. This is done by choosing a control u which minimizes

$$\frac{dV(x(t))}{dt} \text{ when uncertainty } v \text{ maximizes it.}$$

This was already considered in 1974 in the paper “On Stabilizing a Linear System with Bounded State Uncertainty” and in 1976 in “Stabilizing Control for Linear Systems with Bounded Parameter and Input Uncertainty” co-authored with [Shaul] Gutman. The theory was greatly advanced by results of [Martin] Corless’s thesis and reported in many co-authored publications such as “Continuous State Feedback Guaranteeing Uniform Ultimate Boundedness for Uncertain Dynamic Systems” in 1981. The following years saw a plethora of papers, many co-authored with Corless, Barmish, Ryan, Chen, Garofalo, Xiang, and others. Perhaps the best exposure to the underlying thesis may be found in three chapters in the book “Deterministic Control of Uncertain Systems” edited by A. Zinober and published by Peregrinus, London, 1990.

09-00:16:00

Burnett:

So I read in one textbook that talks about the late-twentieth-century descendants of Lyapunov functions in order to explain to young students: think of all the circumstances under which you can’t actually experiment, you can’t gather information. The information’s not available. You have to build an entire plant, and you might build it upside down. So you need to actually be able to design without information, and that’s what this deterministic control under uncertain systems means, and so in the absence of that, can you and reason your way through to something that can be a beginning?

09-00:16:54

Leitmann:

Yeah, well there’s a caveat here, it is that it’s clearly a conservative way of dealing with uncertainty, and it’s the main criticism, in fact, that exists, and it depends whether it’s justified. It may be perfectly justified, or it may in fact be the only thing you can do under this—for example, if you know past

information, of a nature of many, many samples, but you can—for example, in an earthquake, you really have very little statistical information except, in history, there've been earthquakes up to a certain size, and under this approach, you don't have to know any more than that there's never been an earthquake greater than a ten, or whatever, and this has the additional advantage that, in designing a system, you can say, "I will risk anything up to a certain point," which in no way says that "it can't happen," and with what we call X events, which are sort of outliers in the distributions, there's nothing you can say, other than that, "How can I prepare for something I know very little about?" And you know, for example, global warming or climate change is one of these disciplines—

09-00:18:23

Burnett:

Climate science, yeah.

09-00:18:24

Leitmann:

—yeah, which says that "we're not saying that certain things will happen, but just from the observations we have," that "certain things *may* happen," and we again look at past changes in climate and so on, and how this changed when the Industrial Revolution started, how human behavior is involved. And there are natural catastrophes, comets striking the earth. They happened and that's how the Ice Age happened and other events where animals just died out, whole populations.

So, it's, for somebody like me, about avoiding the worst that can happen, and maybe it'll cost a little bit more, or a lot more. It depends, as I say, how risk-averse you are, and that's maybe a major element, is that you want to be more or less risky. That's one thing, and the other thing is also a certain amount of simplicity. If you can do this, changing the system so that you are Lyapunov stable, then that may be—for example, with a rocket, that may be costly in the sense that you may have to use full thrust more often than in the other case. That kind of thing can happen, certainly, and then you just have to make a decision.

Now, I think that in situations where you have very little information, for example, landing on Mars, or later on, on other planets, there's almost no information. We have more information now on Mars and we'll presumably get more before we try to land somebody, but then you can say, "What is the worst thing that we saw in terms of solar winds and whatever can happen, and we have a piece of equipment that cost a billion dollars, can I risk losing it?" So it's again, a sort of a bet.

09-00:20:51

Burnett:

Yeah, yeah, and you're trying to assess risk in advance, without knowing all the parameters, and trying to design for that.

09-00:21:02

Leitmann:

Yeah, actually, you never do know all the parameters, so it's a question of how well you think you know everything that can happen, and we talked about Chernobyl as an example, where things happened that simply nobody thought would happen, the human stupidity kind of thing.

09-00:21:24

Burnett:

Right, the controller worked pretty well, and—

09-00:21:26

Leitmann:

Oh yeah, so—

09-00:21:27

Burnett:

—it's when they took it off—

09-00:21:28

Leitmann:

It's just not fast enough to put it in human hands, or observant enough; in addition, what you sense is perhaps a lot less than what the equipment senses. So, it's just a decision you can make, and it was attractive to me. I guess I mentioned it, I'm basically very risk-averse, over insured, and sometimes, that's very good; sometimes it costs more money and effort and whatever, and that, of course, in turn, has effects on other things, for example, in terms of the current situation where the president feels that it's a worse thing to affect industry by putting regulations on industry than to risk having, what he doesn't believe anyway, an event or events that—you know, and so it's a balance kind of thing. So, my wanting, and going into this that way, no doubt even subconsciously, was influenced by that. I certainly didn't think of it in those terms until recently when we started talking about it. I didn't bring myself in as being risk-averse. It just was, it was attractive to me.

09-00:22:48

Burnett:

Yeah, well—

09-00:22:49

Leitmann:

And, it was something which I could handle, and eventually handle with help from my students, my colleagues, coworkers and so on, better, and so, the particular approach we took is similar but different from what other people have taken for deterministic control. There's something called H-infinity control, any number of other approaches, and they come and go; some stay.

09-00:23:26

Burnett:

Well, there's two things worth mentioning, I think, that are happening in the larger culture, in the larger economy, and one is, increasing efforts to address risk, to look at the distribution of risk, and to figure out how to minimize risk, and a lot of that stuff is done probabilistically, but the other piece of it is, increasing automation. So, there are literal controllers more and more in various things. There are controllers in buildings that automate—

09-00:24:00

Leitmann:

Right, sensors in particular that can tell you what the state of the system is, because that's needed, of course, to use these controllers. In an ideal system, you have perfect knowledge about the system's state. There a lot of work has been done and is being done where you have less information about the state. For example, there are only certain things you can measure with more and more precision now, so that the whole idea of thinking of everything that can happen in a system depends on the system, and the more complex systems become, and that, presumably, the people that I know who work on X events, think that the arrival of an X event is very much tied to the complexity of the system, because you just can't handle it. You know, Renaissance men are very few now. Used to be a time when there were five mathematicians in the world, that kind of thing. So that's all changing. There's certainly no way of predicting what it will be like ten years from now.

09-00:25:13

Burnett:

Right. But it is just to say that you can see the demand for exploration of these kinds of questions mathematically, so you are attending conferences on, I can't remember the name, but the Society for Information Processing, for example.

09-00:25:31

Leitmann:

Yeah, IFIP [International Federation for Information Processing].

09-00:25:33

Burnett:

And so, there're organizations, there're institutions such as the US military, governments, and universities, and engineering schools of course, all interested in trying to theorize the dimensions or the domains of control, techniques of control, techniques of automation or quasi-automation, and that is the world that you're growing into. You're starting with rockets, but you're growing into these other domains where people are thinking about ecosystems as systems of control, or amenable to control, or management.

09-00:26:18

Leitmann:

Yeah, and I think the management in that case is not to do certain things, rather than an active kind of control, or active control is that you handle the bad things: emissions, pollution...

09-00:26:32

Burnett:

Right, terrorism—

09-00:26:33

Leitmann:

Yeah, well, among other things, sure.

09-00:26:37

Burnett:

And so, why don't you talk a little bit about some of the students that you had and colleagues you had who were involved in this type of deterministic control of uncertain systems research?

09-00:26:55

Leitmann:

Okay. We've talked about a few students so far, I think Ken Saunders and Gordon Mon, primarily, and they were really just beginning to get into control theory and game theory, a generalization of control theory, but not instead of optimizing things, which is what you do in optimal control and in differential games, to go into what I began to feel for a long time: it's perhaps more important to be safe than to be best, so to speak. And so, so it's the middle at the end of the sixties; I was able to attract two or three very good students: There was a fellow from Oklahoma by the name of Harold Stalford, and we're still in close touch; a student from Liège in Belgium, François Litt, with whom I'm in weekly touch; and Saul Rocklin, and I'm not quite sure where he came from, but he was a very good student. Unfortunately, he died very young. There was Aggarwal, an Indian student who came about that time, early seventies; and an Israeli student who came in, I think, 1968, yeah, Shaul Gutman, with whom I'm in touch, in fact, he comes to visit me; and then towards the middle seventies, maybe the late seventies, I was very fortunate to attract a really outstanding student, Martin Corless, from Ireland.

So, these people all have done very well later. For example, François Litt is an emeritus professor already for some years, from the University of Liège—I think he was emerited seven or eight years ago; he's twenty years younger than I am, but [laughter]—and, Shaul Gutman became a professor at the Technion in Haifa, and also a member of the Israeli parliament, the Knesset, for five years, and as I said, Harold Stalford is a professor at the University of Oklahoma. He had a couple of other jobs. I think he was at the Georgia Tech for a while, but has been for a long time at the University of Oklahoma, and again, we're in touch. Gordon Wang, I don't know, as I lost track of him.

So, these are really the main students that came to me in the late sixties up through the late seventies, and of those, the ones who became primarily involved with Lyapunov controllers were Shaul Gutman and Martin Corless, and later on, other students, of course, but they already were sliding into this, so maybe we could just quickly mention what the students did who didn't go primarily into deterministic control, were the Belgian student, François Litt, did his thesis on control with state constraints. We talked about that a little bit the other day, and it's a topic that I dealt in one chapter in my little 1969 text already, and so when he came, I had already thought about that and lots of other people had thought about it, of course.

09-00:31:23

Burnett:

Is that the *Introduction to Optimal Control*?

09-00:31:25

Leitmann:

Right, yeah, but it just occurred to me as an aside how few books there were in that field at that time. I don't know what organizations put out a book—I'll give you the page from that book that rated systems and control texts throughout, up to the current time, which was maybe the early 2000s. It was given to me as a present from the Automatic Control Council when I got the

Bellman Prize, and I found, to my astonishment, in optimal control, there are only four books that were mentioned as being what they call classics. One was a book by two professors from MIT. Athans and Falb. They dealt primarily with linear systems in great detail, and produced a 900-page book to do that. When they first submitted it, they had over a thousand pages. And the book by my colleague [Arthur E.] Bryson from Stanford and his coauthor [Yu-Chi] Ho, from Harvard, called *Optimal Processes*, and then they mentioned my little text, '69 text, as one of the ones that can be considered classics in the field, and if you look at it now, anything but a classic. It's simpleminded in a way, and certainly didn't use fancy mathematics, really fancy mathematics, but, it shows that the field just exploded. Really, that's what I'm trying to say.

09-00:33:38

Burnett:

No, absolutely. You can see in the stacks in the engineering school—

09-00:33:42

Leitmann:

Yeah, or go to Amazon even.

09-00:33:43

Burnett:

—texts on robust control—

09-00:33:45

Leitmann:

Well, and—

09-00:33:45

Burnett:

—in the eighties, nineties—

09-00:33:45

Leitmann:

—you go to Amazon, see how many books there are still, and some of them, that little '69 text is still available.

09-00:33:56

Burnett:

I'm wondering, is it '66 that, what we—

09-00:33:58

Leitmann:

Maybe it's '66.

09-00:33:59

Burnett:

—because the—

09-00:34:01

Leitmann:

It was the late—

09-00:34:01

Burnett:

—*Introduction to Optimal Control* is '66, I think. 'Sixty-nine might be a second edition, or a revision of it, I'm not sure.

09-00:34:08

Leitmann:

I don't know. I can look it up. I have the list of book—

09-00:34:11

Burnett:

Okay, good. What—

09-00:34:13

Leitmann: Anyway, it was the second part of the sixties.

09-00:34:15

Burnett: Okay, yeah. And so, what's interesting about this is that a lot of these edited volumes are put out by the association, they're just very lightly edited, and—

09-00:34:36

Leitmann: Yeah, well a lot of them come from meetings and conferences.

09-00:34:38

Burnett: Proceedings, basically.

09-00:34:39

Leitmann: Yeah, proceedings.

09-00:34:40

Burnett: Yeah, and so you can get a sense at that moment, it's a very raw, new area of inquiry, and the community must have been quite small worldwide. You knew most of them, I would think.

09-00:34:56

Leitmann: Well, meetings got to be bigger and bigger, with the space age when a lot of people went into this thing. It's sort of interesting, among other things, that the real risks—for example, [Revaz] Gamkrelidze, who was the main coauthor of Pontryagin's—in their book, *Optimization of Processes* or something like that, which came out in Russian I think about 1962 and then an English addition in about '65, he told me that they were very unhappy, including, Pontryagin claims, that they felt, looking back, that they had not really taken great care in doing this book I found out from Gamkrelidze, the reason for that was that Pontryagin, who was already a very big wheel of course in mathematics, he was told that they would receive the Lenin Prize if they could put out this book within a year. And so, they took papers that they had written and sort of put them together and tried to clean them up, and that kind of thing was happening.

09-00:36:26

Burnett: Yeah, there was a lot of haste. I mean, it was a space race, but it was also a science race, and the scientists were racing against one another.

09-00:36:33

Leitmann: Yeah, well, in some cases, government supported that. In the Soviet Union, it was the government, of course, which ran things, but it's interesting that Pontryagin told me that he did not accept any royalties, his three coauthors get the royalties because he wasn't that happy with the book. I don't know if that's true, but that's what he said.

09-00:37:01

Burnett: Interesting.

09-00:37:01
Leitmann:

So, that's an interesting sideline. Yeah—

09-00:37:04
Burnett:

So, these figures—I mean, your students go on to publish. Martin Corless is publishing on *Deterministic Control of Uncertain Systems*. You're co-publishing with him into the eighties and 1990.

09-00:37:19
Leitmann:

Well, there's even one that I just looked at in 2000. It dealt with control of endemic diseases. I think that was 2000, yeah, and as I said, he was truly outstanding. If his name comes first in a paper, then he deserves the credit primarily. I mean, there's always a matter of percentage, but I certainly felt that way and I'm sure that he did, and I think in total—I didn't try add them up—I think we co-published about twenty papers over that period of time—

09-00:38:01
Burnett:

Yeah, a great collaboration.

09-00:38:09
Burnett:

Well, can we talk about advances in the theory, talking about Lyapunov control, and as a way to then talk about how this gets applied? There are application papers, as you said. Whole domains open up around this topic. So, at a certain point, it becomes robust control. Around the mid-eighties, people start using that word. Is that basically interchangeable with—

09-00:38:37
Leitmann:

Pretty much, yeah, because the feeling was, it's robust in the sense that you've taken care, if you didn't do it statistically, but deterministically, you've sort of taken care of either the worst case or at least something that approaches it, where you have a lot of information about the uncertainty so that you can feel that it's pretty safe, but, one has to be careful because this name sometimes implies more than meant. Just as an aside, for example, you talk about chaos theory, but there's absolutely nothing chaotic about chaos theory, because in fact, it's a wholly deterministic approach. It's simply that the systems that are subject to what is called chaos in this sense, is that they are extremely sensitive. If there are very small changes in the parameters or initial conditions, or perturbations, they can have very large effects, and when you plot all that, it looks like a random response, and I think from looking like a random response, it got the name chaos theory, but it's got nothing to do with—you know. So it's, these things just get their own little names, and—

09-00:40:06
Burnett:

So robust control is not the opposite of weak control, [laughs] right?

09-00:40:10
Leitmann:

No, yeah, right, in fact, it says that you're taking care of the worst so that's robust, you see, in that sense, in that sense. Maybe you're overdoing it, but you know, it—and again, there are no guarantees, of course. You have to always put this kind of caveat in that what you consider the worst case, clearly

almost surely is not the worst case, but it may be so rare that you can take that chance, and this is where the newer field of X events comes in, which deals with these events that don't fall under either the statistical or the robust rubric, because the upper limit is—well, it's not enough to talk about the event. It's not the rarity of the event; it's the kind of catastrophe that it can encompass, that really is, and how to get a number or measure to take care of rareness and terrible result. You can say, "It's so rare I don't have to worry about it," but the way you say, what? Maybe a little bit like climate change falls into this kind of thing, you know?

09-00:41:32

Burnett:

Well it's mathematical eschatology. You're studying these kind of world-ending events that—a complete collapse.

09-00:41:41

Leitmann:

Potentially, then you can say, "Well, I don't worry about the rest of the world except up to my grandchildren," kind of thing. [laughter] Now I have to do great-grandchildren so I have to worry even more, so, well let me look. You were kind enough to look at this, at my bibliography mostly, and well, one of the earlier papers, yes, it's a paper given at an IFIP [International Federation for Information Processing] conference, as a matter of fact, in 1976, and it's "Stabilizing Feedback Control for Dynamical Systems with Bounded Uncertainty," and that's a paper I wrote with my Israeli student, Shaul Gutman. He started working on this seriously maybe from 1970 onward. Just again, an aside, in 1973, he was well into his thesis, but the war broke out, you remember, 1973.

09-00:43:06

Burnett:

The Yom Ki—

09-00:43:06

Leitmann:

—the Yom Kippur, yeah, I think it was '72—yeah, I may be off—and he actually disappeared for six weeks. He was a captain in the IDF, tank commander, as a matter of fact, and he was maybe twenty-four years old, and he just went, and came back when the war was won, that took only about that amount of time, but I somewhere lost it—I treasure the postcard he sent to me from the suburbs of Cairo, that's how far they got, and he went to the postal clerk, the post office, and made them mail it in front of him, because he thought they might tear it up and throw it away, and that's how I got it. Yeah, it had the postmark. I don't remember what the suburb was, but you know, so—I thought we'd, I'll mention what some of the other students who didn't go into this did.

09-00:44:20

Burnett:

Sure.

09-00:44:21

Leitmann:

For example, let's see, we said that François Litt went into problems with state constraints.

09-00:44:31

Burnett:

Yeah, you mentioned Ken Saunders, Gordon Mon, and Gordon Wang.

09-00:44:34

Leitmann:

Yes, and then Saul Rocklin a little later, and then Shaul Gutman. So Shaul Gutman went into Lyapunov control, deterministic control, for a very good reason, by the way, which I will mention in a minute, and even Stalford, because he was already very much into sufficiency conditions for optimal control, expanded that into sufficient conditions for games, and that in turn led into the Lyapunov control of course, and I had a student by the name of Aggarwal who worked on avoidance control but not from that point of view.

09-00:45:29

Burnett:

Not from robust control.

09-00:45:30

Leitmann:

Yes, right, and then of course, as I say, in the late seventies, Martin Corless went to robust control, and then students later went into at either the theory or the theory at least in a very applied problem, like labor management and that kind of thing. So, looking at some of the other papers, there was “Avoidance Control,” which was based on Lyapunov control, because Skowronski, of course, was really a predecessor. Then another colleague of mine really, did something that’s called “Ultimate Boundedness,” but it was really Martin Corless who did not come up with the idea, but used it to improve the control.

See, if you do Lyapunov control, then very often, the control function, which is the function, of the state, u of x , has discontinuities, and the theory, as it exists for ordinary differential equations, can’t handle that, so if you have, for example, \dot{x} equals f of x , usually assumes f of x is a continuous function that then satisfies some other conditions, and the Russians particularly were able come up with a theory, at that time a little early, already in the thirties, which handled, by extending conditions, discontinuous right-hand sides, because usually, you assume that the control is at least piecewise continuous, okay, so that, the control would allow that, but the equations don’t allow really sharp discontinuity. So if you go from a faucet running at full speed, that may be the upper limit of the control, turning off where it’s zero, the idea came—first of all, you get much simpler mathematics, that is—

09-00:48:04

Burnett:

It’s on or off.

09-00:48:05

Leitmann:

—classical mathematics will handle it, and secondly, it would actually be a better model, because if you apply a control, there’s no real system that can do it in zero time. May be very fast, but that’s a relative term, so mathematically, they don’t care how big the derivative becomes as long as it isn’t a jump.

09-00:48:29

Burnett:

Right, right. Interesting.

09-00:48:31

Leitmann:

And so we extended the theory to allow for that kind of control and that led from allowing the system to go eventually to zero, to where you want to go, to an arbitrarily close neighborhood. So the faster you make the jump in the controls, it doesn't matter. The slope of the curve that goes continuously up or down, because you don't turn a faucet, you know, or you start up the engines and they go and all that, doesn't matter, because mathematically, that's still a continuous control, continuous in the state, and that was called ultimate-boundedness control. In other words, the ultimate region to which you drive the system is bounded. It doesn't have to be zero, but it's bounded, and then, you can show that the faster you make the jump—it isn't the jump, now the faster you make the change, continuously, the smaller you can make that region.

So, from a practical point of view, that's really a much better model. Mathematicians like what they call asymptotic stability, which is a process where you go exactly to where you want to go, but from an engineering point of view, ultimate boundedness is actually very realistic, and so that's where we went. So, many of the later papers dealt primarily with ultimate boundedness, but we took more and more systems into account for which we could get a controller, and so, many of the papers deal with that and then some applications.

09-00:50:42

Burnett:

So that permits you to use the—

09-00:50:45

Leitmann:

Existing.

09-00:50:46

Burnett:

—existing theory for ordinary differential equations, right?

09-00:50:49

Leitmann:

Right—

09-00:50:49

Burnett:

And you can't—

09-00:50:50

Leitmann:

—because the right-hand side will be continuous in x , right?

09-00:50:55

Burnett:

Right. And you can do it in partial? You can do that, you can use continuous in a partial differential equation? Is that the difference?

09-00:51:03

Leitmann:

I really didn't deal with partial differential equations. Again, you have problems if you—and the smoother you can make it, the easier the theory is, there's no question about that. That much I know, yeah. So, there was a symposium in Tainan, if you remember that. It's an interesting city in

Southern Taiwan, a big shipbuilding city, and few foreigners ever go there. I was one of like three people in the street. Little kids used to run after me. It's a big city—there are about million people or more—and that was the International Symposium in Engineering Sciences and Mechanics, and I gave a paper there on stabilizing uncertain systems. Then, there's more confusion on nomenclature. People began to call, instead of going asymptotically to going into this region, ultimate boundedness region, they called it practical stability, but other people mean something else by that, "practical." So that, we dropped doing that. Then we had a paper in talking about what kind of systems we dealt with, with Professor Kelly and another student of mine, Soldatos, from Greece. He got lost somewhere in the list, I think.

09-00:52:52

Burnett:

Yeah. "Robust Control in Base-Isolated Structures under Earthquake Excitation?"

09-00:52:57

Leitmann:

Right.

09-00:52:58

Burnett:

It's '87, with [J.M.] Kelly—

09-00:52:59

Leitmann:

Right, that was much later, of course, but there are lots of papers. If you go to the bibliography—

09-00:53:07

Burnett:

Hundreds.

09-00:53:07

Leitmann:

—there are 320 items, and we just picked out a few, just to give us an idea.

09-00:53:13

Burnett:

Yeah, and along this area, I mean, there's other areas; we should let the listeners know—

09-00:53:18

Leitmann:

Well, there—

09-00:53:19

Burnett:

—that there were many different areas.

09-00:53:20

Leitmann:

There were some other interesting ones, but again, in the eighties, because I think Martin finished his dissertation in '82. He became a professor at Purdue, which he is to this day, of course, I began to also work with people in Italy, and one called "Robustness of Uncertain Dynamical Systems with Delay and in the Absence of Uncertain Condition," so, sometimes you may have delay in either the measurement of the state or delay in application of the control. Martin Corless gave a paper in '88, "Controller Design for Uncertain Systems

via Lyapunov Functions,” at American Control Conference in Atlanta; that was ’88.

[Narrator Addendum: Another PhD student with me was Y.H. Chen who did his thesis on robust control relaxing assumptions such as what we call “matching.” He also worked on interesting applications, often with Cho Seng Lee]

Then I had a visiting scholar in the eighties from the University of Helsinki, Veijo Kaitala, and we had six or eight papers together, all of them on the fisheries industry. So just to give you a couple of names, one is called “Stabilizing Management of Fishery Resources in a Fluctuating Environment,” that was with Veijo and me, and it was in a journal called *Dynamics and Stability of Systems*. There was a paper in the—well, now, there were lots of them actually, but anyway, if you look up Kaitala, you’ll see there must be six or eight papers, in the second part of the eighties. He became interested in another problem later and we did a couple of papers together.

So, in vibration reduction, I attracted a—well, there was a German postdoc who came to me around 1969, Wolfram Stadler. He was interested primarily in dynamics per se, and dealing with controls wasn’t really his major thing, but he was with me three years, and then eventually became a professor at San Francisco State University. Then there were interesting problems, of course, in robotics, and so there’s, for example, “Tracking Control of Robot Manipulators Using a Robust Deterministic Controller,” and that was done actually with a colleague, Roberto Horowitz. In fact, he’s the chairman of mechanical engineering at Berkeley, and one of its students. The German postdoc that came to me around 1989 or ’90 was Eduard Reithmeier. All these people became very close friends. In fact, Reithmeier, I just sent him email. He came from Hanover. Well, no, actually, that’s where he ended up. He became a professor at the University of Hannover, Leibniz University.

So he was interested primarily in vibration control because he became interested in medical instruments, and in fact, his first job after he got his doctorate was with a medical instrument company, before he went to—in Germany, it’s good to have industrial experience to become a professor, otherwise you have to start at the bottom. And so, for example, we have a paper together. It’s fairly recent: “Robust Constraint Control for Vibration Suppression of Mismatched Systems.” That was 1996 already, so we already apparently corresponded, because—well, he came to me in 1990, so that was already after he left, yeah.

09-00:58:28
Burnett:

Vibration control is very important for instrumentation, electronics. You need to isolate them, otherwise that begins to introduce distortions in the measurements and so you can have controllers that compensate because the vibration is somewhat consistent—

09-00:58:51

Leitmann:

Well, for example, he worked, and then I did, with someone who came originally from Turkey. His name is Küçükay. He was the head of the automotive department at another university, and they were interested in noise suppression, which is a vibration, of course, and we have a couple of papers on “ANC,” active noise—

09-00:59:18

Burnett:

Control.

09-00:59:19

Leitmann:

—control, yeah, so that’s in there. And then, I got a visiting scholar, who spent a whole year with me four times, or better part of a year, and that was Lee. Cho Seng Lee came first from the University of Malaysia, and since the Chinese apparently don’t do too well in Malaysia, he moved to Singapore, and he was there for a long time, and was emerited probably seven or eight years ago. He visited me a couple of years ago, a fantastic guy. He had all these cute little problems. For example, what was this called? Yeah, well we did one called “One Avenue to Robust Control of Nonlinear Systems and an Application to Social Systems.” We did “Optimal Learning Policy for a Lazy Student,” [laughter] to guarantee that he will get a certain grade kind of thing, but we had a lot of these papers and then he became interested in ecological problems. Controlling pollution in river systems, we have a number of papers on that. It must be fifteen papers with him. [Narrator Addendum: He also co-authored papers with Ye Hwa Chen who became a professor in Georgia Tech University]

09-01:01:07

Burnett:

And there was another former—a Malaysian as well: Bean San Goh, Goh, G-o-h.

09-01:01:15

Leitmann:

Yeah, well he’s of course a much more senior person. He came as an RA, research assistant, to work with me probably in the early sixties, yeah, and we did one of the—then I have a colleague, of course, and dear friend who died very early too and certainly tragically, Tom Vincent from the University of Arizona. I spent a number of summer semesters there working with him, or giving lectures there and for example, we have a paper together with Bean San Goh. I’m trying to remember what it was.

09-01:02:06

Burnett:

It was in seventy—

09-01:02:08

Leitmann:

Yeah, it was “Optimal Control”—

09-01:02:10

Burnett:

Yeah, “Optimal Control of the Prey-Predator System” with Tom Vincent, at the Fourteenth International Congress of Entomology, and it was in the

Journal of Math Biosciences in '74. I read that paper. It's interesting. It's really interesting.

09-01:02:25

Leitmann: Well, we also deal with not using pesticides.

09-01:02:30

Burnett: Right. So it's about integrated pest management, and it has some counterintuitive conclusions that have to do with managing the predator population, not the prey population, which is—

09-01:02:44

Leitmann: Yeah, well that's the control—

09-01:02:45

Burnett: —unusual.

09-01:02:45

Leitmann: —variable, of course.

09-01:02:46

Burnett: Right, right, but it actually runs counter to the conventional integrated pest management wisdom, which is a wonderful provocation, to say that this model is somewhat counterintuitive, but it takes into account differences in the life cycles of the organisms, and it plots it mathematically, and it's wonderful.

09-01:03:09

Leitmann: Well, and it's still referenced, and this is fairly early, '74 or something like that.

09-01:03:17

Burnett: Well, G. S. Goh claims that “it is the first ever application of optimal control to manage an ecosystem.”

09-01:03:25

Leitmann: Well that's probably true, yeah. He knows that much better. By the way, he was also very active. He was a student of Derek Lawden's.

09-01:03:38

Burnett: Oh! Right.

09-01:03:39

Leitmann: Yeah, he was a student of Derek Lawden's, and he became very interested in what Lawden—we're straying here—remember, made a little mistake, and seemed to show that control with the rocket thrust will either be on or off in transfer trajectories in the solar system, and so that so-called singular solutions were ruled out. Those were the solutions which vary with time between the minimum, maximum value. It turns out that all solutions in classical, the calculus of variations, are singular solutions, because they did not deal with bounds, right? I had at the time written to Lawden to send him some

counterexamples to his result, because from then on, I've always felt that the best thing to do is to communicate with people when you think you found something wrong, or continue talking to them rather than—

09-01:05:09

Burnett: Publishing a criticism.

09-01:05:11

Leitmann: —saying “so and so’s result is wrong,” and it always worked out beautifully. Goh mentions that because he’s written quite a bit, and then other people picked it up from him, about the early history of singular solutions, and what I showed was that at least in the necessary conditions, which are the first thing that you look at, those solutions are possible, they can’t be ruled out, and Lawden had ruled them out using the necessary conditions, and of course, he was very gracious about that, and then he wrote a paper, in fact, showing such a solution, known as the Lawden Spiral, which is sometimes optimal and sometimes not.

And so this is all something that I feel very grateful for, because it made friends in addition, and Bean San and I have been in touch ever since. He spent time with Vincent and me, in fact. Vincent was on sabbatical here, I think, and he was essentially a postdoc of mine. So, all these wonderful things happened that I never really thought about until I was faced happily with these interviews.

So, you wanted some notion of what people did in the theory, and we just touched the surface, of course.

09-01:06:49

Burnett: Sure. Well, I’ll follow your lead, but is there one more evolution of your research career in the theoretical, pure theory domain that we haven’t addressed?

09-01:07:11

Leitmann: Well, I went back recently to 2000 onward, to two of these little notes that I had written: one based on the equivalent problem approach, which was a note in the middle sixties—

09-01:07:31

Burnett: Yeah, the Hamilton’s principle.

09-01:07:31

Leitmann: —and the other one, Hamilton’s principle, which was also a little note, which I just let go, and as I had maybe mentioned, on the equivalent problem thing, it was a colleague and friend, Efim Galperin originally from the Soviet Union who immigrated to Canada, and is a professor at the University of Quebec at Montreal. He felt in 1999 that much more could be done with the equivalent problem approach, and we corresponded for about six months, but decided

that we were looking at things in different ways. He was very helpful, making me go back to it.

So that actually was picked up when I, at his behest, wrote a little paper about 1999 or 2000, in which I expanded that considerably from just minimizing or maximizing an integral to dealing essentially with optimal control, at least for nice systems, where I wanted to minimize or maximize an integral, but subject to state equations, and that appeared in *JOTA*, I think, about 2000, and, what happened was that a mathematician from the University of Cleveland—[coughs] excuse me—wrote a paper in the same journal, *JOTA*, the next year. He did not get in touch with me, but he didn't criticize me. He simply showed that the equivalent problem approach was essentially what Carathéodory used in the middle thirties, and how that differed from what I did, because I allowed a much larger class of equivalent systems than he did. He simply looked at different integrands, and I looked first at transformed variables, and then at different integrands. On the basis of what Carlson, Dean Carlson, said, namely that these two, they're different in the approach, but they're identical in the basic idea of dealing with equivalent systems, and that mine, in the end, when I took account of also different integrands, of course, allowed for a larger class of equivalent problems, and I ended up with ordinary differential equations rather than partial differential equations, which Carathéodory did.

And we wrote thereafter about a dozen papers together, from the late nineties through 2010 or something like that. Again, he really was the most senior author, not only because of name but also because he was a mathematician—I would furnish an idea and we'd talk about it, and then he was able to do the grunt work. So that worked out beautifully, and we're still in touch. We write to each other. He's now, has been for some years, associate editor of the *Mathematical Reviews*, and he writes maybe one or two papers a year, maybe even fewer than that because that's a very busy job, but he's a great guy. And then of course, I have a colleague who came as a visiting scholar, Gene Ryan from the University of Bath, about that time in the eighties, and we are coauthors on a number of papers as well. So, I've had a lot of help.

09-01:11:40

Burnett:

Yeah, and a lot of—

09-01:11:42

Lietmann:

No, really.

09-01:11:42

Burnett:

—a lot of collaborations.

09-01:11:43

Lietmann:

No, I call it help.

09-01:11:47

Burnett:

Well, it's hard to—I mean, the range of subjects that you have touched on in your career is quite remarkable, possibly unwieldy. Even though we've had

twenty hours' worth of conversation about your career, we've still scratched the surface in terms of the kinds of applications of research. So, I was just looking at conference proceedings from the late eighties. There was a series of workshops at the University of Southern California on control mechanics, and the third one was in honor of George Leitmann. It was for your sixty-fifth birthday, so this is already thirty years ago.

09-01:12:35

Leitmann: Yeah, right. [laughs] Almost thirty years.

09-01:12:38

Burnett: And so people were singing your praises back then as someone who had—

09-01:12:43

Leitmann: Well, people were very nice to me, all along. If you look at what they did for my seventieth, eightieth, and ninetieth birthday, it's sort of overwhelming. It really is.

09-01:12:53

Burnett: It is. Well, we'll talk about that in our final session, I think, but I think the story is one of being one of the founding people who developed a set of subdisciplines—

09-01:13:10

Leitmann: You're being very kind. [laughs]

09-01:13:11

Burnett: —but, what was exciting to me was going through the—and clearly, there are papers by junior professors that are part of the Festschrift, right, and, because I didn't recognize the names immediately among—they're not part of your—

09-01:13:29

Leitmann: So these are Festschrifts—

09-01:13:31

Burnett: Yeah, well—

09-01:13:33

Leitmann: —and thereabout—

09-01:13:33

Burnett: —it's the third year of that conference—

09-01:13:35

Leitmann: Okay, but—

09-01:13:35

Burnett: —the 1990—

09-01:13:36

Leitmann: —but then, since then, there've been six or seven of just journal issues that people put papers together.

- 09-01:13:43
Burnett: Well this is where papers that were part of this conference, and—
- 09-01:13:47
Leitmann: That's the one that Skowronski started.
- 09-01:13:49
Burnett: Right, right. So, the papers are on optimization, but there are papers that deal with robots and robotics, industrial robots; optimization of disk drives in computers; the optimization of food chain systems, so thinking about populations and population biology; and of course, that old chestnut of missile guidance; but you get a sense—
- 09-01:14:16
Leitmann: Because it was in Southern California, remember? [laughs]
- 09-01:14:17
Burnett: Right, fair enough, fair enough, but that's just, that's not even all, because it goes, as we said, we've talked: There's papers on ecology. There's papers on pollution control. There's papers on terrorism modeling. There's papers on labor relations and when to strike, when not to strike. There's papers on oligopoly and competition, so, economics. So, any field that you can describe phenomena with measurable criteria, measurable variables, is amenable to this kind of exploration, and all of these areas were robust, but it seems that the entry of this way of thinking can shed light on areas that are hitherto—
- 09-01:15:20
Leitmann: Yeah, and other people picked up—
- 09-01:15:20
Burnett: —just empirical, right? And so it's a wonderful insight that you bring to these different fields.
- 09-01:15:27
Leitmann: For example, I didn't—Walter Breinl was a postdoc with me, must have been in the early middle eighties. He became the CEO of the largest locomotive company in Germany, and we did a little paper together first in Germany, then a couple of years later, an English edition with some small changes, Breinl and I, and it dealt with the control, a very simple model of the suspension system for magnetic—what do they call it? Magnetic—
- 09-01:16:19
Burnett: Maglev [magnetic levitation trains]?
- 09-01:16:19
Leitmann: Yeah, maglev, essentially—
- 09-01:16:20
Burnett: Maglev trains.

09-01:16:21

Leitmann:

—levitation, magnetic levitation vehicle, and this is a system where you cannot measure all the variables. So, for example, they use a feedback control. You have to deal with only some variables. The other one's just not measurable, or at least not well enough, and that's of course a completely different part of control theory, namely, filtering using statistical methods. We did not use statistics. We did it by robust control of systems where you cannot base your control on the state but only what's called output, and the output may be a combination of state variables, or just some components of the state variables. Maybe you can measure velocity, but not position, that kind of thing, and we had a little paper on that—that's middle eighties—Breinl and I.

09-01:17:28

So, so much of, both in theory and certainly more in application, came really with the people I worked with. It was just in retrospect, I marvel at all of the—we didn't even touch a lot of them so far. Manela, for example, was another Israeli student that came to me in the late seventies, Jonas Manela. He came from the armament industry in Israel. It's one reason why we didn't write a paper together; he went back right away, like the next day. He was interested in describing the systems discretely, in other words, as difference equations rather than differential equations, and I guess I mentioned to you last time that somebody in the late sixties wrote a two-volume book, two-volume treatise one on differential equations and the other one on difference equations, and in the difference equations, he left out a condition because he got an optimal control maximum principle which looked the same way as the other one except in discrete variables, but it turns out, it's not a necessary condition. There has to be one other condition satisfied and it's called directional convexity, and that was brought out, when I was in touch with a man at UC San Diego, who even became the chair of the math department eventually. His name is Hubert Halkin, and he was able to show, in fact, as soon as this book came out, he was able to show, gave a counterexample showing that necessary conditions were not satisfied, but the example was such where you can just, by inspection, you can get the answer, you see. So it was one of these things that you can't say you don't have an optimal solution, because it's so clear. And so, that two-volume book disappeared within three months. You can eternalize a mistake. It's not so terrible when you do that in a paper, but when you do it in a book, it's a little bit more embarrassing.

09-01:20:04

Burnett:

I think there was another case in economics, again in the early seventies, where there was a confusion broadly in the profession between difference and differential equations, so they used differential equations when they needed to be using difference equations, and that leads to a real problem, so that's a separate—

09-01:20:28

Leitmann:

Well, I'll give you another example, because since I became well acquainted and friends, not close friends, with both a Russian in Leningrad—well, Saint

Petersburg, eventually, who came originally I think from Armenia—and a professor at not Singapore, but maybe Hong Kong, David Young, and we were taking a walk the year I got the Bellman Award. By the way, Y. C. Ho and I—we were taking a walk, and he told me this and I later actually checked on it: There were two people who shared the Nobel Prize in economics just a couple of years before then, who made a very interesting mistake, and this is where we were going to talk about “time consistency”, okay?

So, very often, economists can either do open-loop control, which means depending only on time, or closed-loop control, and by the way, feedback in closed loop is the same for optimal control, but it’s not necessarily the same for games because of the linkage of the different players. So the other players’ states come into your own equation of motion, and what happens is that economists very much like to get systems where the feedback control is a constant with respect to the state. So it depends only on time, okay? This is sort of a justification for having I guess a five-year plan, and these guys got their Nobel Prize working in that field, but it turns out that you have to have two things: your state and your adjoint equations have to be separable. In other words, they are sometimes called the costate or adjoint variables (those are the equations that come out of the necessary conditions) can depend on the state as well, because they are coupled equations for most systems. So if you want time consistency, you need those to be decoupled, but you also need the boundary conditions decoupled, otherwise the systems are not independent of each other. But you may have initial conditions that depend on both those variables, in other words, that are not decoupled, and they missed that point because they were interested primarily in the application, of course. So in fact, their theory, from a mathematics point of view, is incorrect.

09-01:23:52

Burnett:

Which economist, do you remember? What year did they, were they—

09-01:23:55

Leitmann:

No, and I don’t even want to mention the names, because Young and I decided at once, we were going to write a little note on that, then we said, “Oh, it’s not the first time that somebody made a mistake and got the Nobel Prize. It’s not the first time.” So, but it’s interesting, so that you get these little things, and also, one thing that is very much done by economists primarily is to deal with the steady state of the system.

So first of all, so, this may not occur for a long time in a real system, the steady state, and of course, if the system gets perturbed and then of course it never happens, but that’s for—much of the mathematical economy theory is really primarily on the steady state solutions. And so with the people I worked with, for example, in Vienna, Feichtinger, and some other people, such as Dockner, who was actually at the university, with whom I wrote a couple of papers using the equivalent problem approach for some papers—he died a couple of years ago—they very much dealt primarily with the steady state of

the system, and also only with what they call the first-order conditions, which are essentially the basic conditions that you get from the first variation, the necessary conditions which need not assure optimality. They deal primarily with those not proving optimality, let me put it this way, and that's simply because it's much easier to do that. Very often, sufficiency conditions have too many conditions before they can be used for real systems.

09-01:26:15

Burnett: So, what is a steady state solution as opposed to the kinds of work that you—

09-01:26:21

Leitmann: The steady state solution is when $\frac{dx}{dt}=f(x,t)=0$ for all t .

09-01:26:58

Burnett: So they're looking for equilibrium, basically, in that—

09-01:27:18

Burnett: Okay. Well that would make sense. The entire stock-in-trade of economics is time, right? That's what they're interested in. They want to understand how something unfolds in time, how a system unfolds.

09-01:27:34

Leitmann: Yeah, and if you can stay on $f(x,t)=0$, it's an equilibrium. That's very nice. Very often you don't start at such a point, so then you have to go and get trajectories, again by control, I suppose, which approach steady state, and hopefully get to it in finite time which is not necessarily required, but nonetheless, they very often deal with those kinds of problems, and I have some joint papers with them because they're interesting problems. Yeah, we did mention other recent applications either game or optimal control applications for terrorism, but then you also get—and this is where we've talked of my colleague and friend, Udwadia, at USC. He and I, and Lambertini, whom, by the way, we haven't mentioned yet, in Italy—

09-01:28:41

Burnett: Yeah, Luca Lambertini.

09-01:28:42

Leitmann: Yeah, Lucca, a very dear friend, we wrote a paper together, and again, it's one where I had some basic ideas, and then the really heavy mathematics, Udwadia did, so he's the senior author.

09-01:28:57

Burnett: This is Firdaus, Firdaus?

09-01:28:59

Leitmann: Firdaus, yeah, Firdaus.

09-01:29:00

Burnett: Firdaus, Firdaus Udwadia. What's what nationality is he, Firdaus Udwadia?

09-01:29:06

Leitmann:

He's from the Middle East. He's a Rosicrucian, by the way, so I'm not sure, Persian maybe, yeah, maybe, awfully nice guy, has a wonderful wife too. He thought he would not want to deal with terrorism as a game, but he would like to understand what is involved in planning acting against terrorism, what may be interesting ways of doing that, or better, some better than others, and so we took a very simple subsystem of that. We looked at the idea of dividing a population into three parts: those people who are pacifists and will never become terrorists, those who can be recruited to become terrorists, and those who are terrorists, and that's a very nice system. First of all, I suggested that we don't deal with populations but percentages of populations because—

09-01:30:14

Burnett:

Yeah, proportions.

09-01:30:15

Leitmann:

—we don't have to know what the population is; we want to know what percentage of the current population. So that was a very simple suggestion I made when he first started to write it up, he dealt with, and the other one was, so we considered I think essentially two ways to fight terrorism: one is military action, police action, and the other one is persuasion through education or whatever. So those would be the two main ways, and it's difficult to say which one is more important. So my idea was, let's give each one of these a coefficient, and then look at what happens when one has a bigger coefficient than the other—so in other words, which one is more important—and then you can say, “What is the result of having one more important than the other?” And that's what he worked on, and that's a very heavy mathematical problem, actually. So that was a very interesting one because it didn't involve control at all. I mean, it involved control in terms of the decisions you make, of action, but it was control in planning rather than it executing a plan.

09-01:31:41

Burnett:

So that's, the first one was “A Dynamical Model of Terrorism,” that's with Luca Lambertini and Firdaus Udwadia, and that's in *Discrete Dynamics in Nature and Society*, 2006.

09-01:31:54

Leitmann:

Yeah, it was accepted very quickly for some reason, I think within a month or six weeks.

09-01:32:00

Burnett:

I think there were a lot of resources being devoted to counterterrorism and research fields.

09-01:32:05

Leitmann:

And so that's very different from some of the later ones I did with the people in Vienna—

09-01:32:13
Burnett: Feichtinger?

09-01:32:14
Leitmann: —Feichtinger and—

09-01:32:15
Burnett: And Novak, right?

09-01:32:16
Leitmann: Yeah—

09-01:32:17
Burnett: A. J. Novak.

09-01:32:18
Leitmann: Let's see.

09-01:32:20
Burnett: That's "A Differential Game Related to Terrorism: Nash and Stackelberg Strategies." It's the *Journal of Optimization Theory and Applications*.

09-01:32:26
Leitmann: Yeah, and that was maybe ten years ago, or so—

09-01:32:30
Burnett: Yeah, 2010, yeah.

09-01:32:32
Leitmann: —not even ten years ago, eight years ago, but there was a more recent one that—here, it's in here—may be even be the last paper I did with him: oh, "On the Optimal Trade-off Between Fire Power and Intelligence in a Lanchester Model"—Lanchester model is a particular economic model—and it really says what is the balance between military action and intelligence, the more you know about the other side, and what was involved there is if you know a lot about the other side, also killing innocent people is diminished, and that's a very important attraction. So that was with Novak and Feichtinger, Novak, a very good guy there too, and it appeared in a book called *Dynamic Modeling and Econometrics, Economics and Finance*. It was edited by Feichtinger, that helped. [laughter] It's a Springer International Publishers' book. That was 2016.

09-01:33:45
Burnett: Oh! Wow.

09-01:33:47
Leitmann: Right.

09-01:33:49
Burnett: Well this is ripped right from the headlines, and just the other day, Google has announced that its pulling out of its cooperation with the US military in

developing artificial intelligence for military [applications], and some of it had to do with identifying civilians. When you're talking about drones, how do you automatically identify a civilian versus a terrorist combatant?

09-01:34:11

Leitmann:

Yeah, right. So those, that one was 2016. Lambertini is a guy I've published with for quite a long time, maybe the early nineties or maybe even earlier. You've mentioned some of those with him, and that was in the ecological group, and then, on some mathematical ideas, "Hamiltonian Potential Functions for Differential Games," that was with Lambertini and two other Italians, in *Automatica*. Then there's a paper, "R. and D. for Green Technologies in a Dynamic Oligopoly," mentioning also "Schumpeter, Arrow and inverted U's," [laughter] which was in the *European Journal of Operations Research* in 2016.

09-01:35:38

Burnett:

So right up to the present, essentially, you are still—

09-01:35:41

Leitmann:

Well, people are very nice to me. We have another one, which was in 2017, and so far it's only come out as a working paper, but it's under review for *Automatica*, called "On the Attainment of the Maximum Sustainable Yield in the Verhulst-Lotka-Volterra Model," and that's a University of Bologna working paper in 2017. As I say, it's under review now.

09-01:36:18

Burnett:

But this Volterra model is what was also at stake, not necessarily exactly, but I believe it was at stake in the Bean San Goh paper from 1974, the Volterra—

09-01:36:30

Leitmann:

Well it's a classical population equation, and yeah, that's right, it comes up all the time, and in fact, I had a little note on a minimum principle for that population equation, and it's something that Volterra did as well in the thirties. He wrote a paper and he got stationarity principle, you know, like Hamilton's principle, for his equation, but it had an integrand that makes absolutely no sense. It has about ten terms in it, and then it's sort of looking at the calculus of variations and trying to fiddle around, and I did something which still has no explanation to me and I've tried to get an explanation. I wrote that equation in inverse variables, instead of population, I wrote the equation for the inverse, I mean, one over the population as the dependent variable.

09-01:37:34

Burnett:

Mm-hmm, and why did you do that?

09-01:37:36

Leitmann:

Because when I did that, that nonlinear equation became a linear equation, [laughter] and for that, I was able to apply that remark on Hamilton's

principle. I could apply that to get an integrand in that inverse variable, which is potential plus kinetic energy.

09-01:37:59

Burnett: And did you write that paper? Did you publish it?

09-01:38:01

Leitmann: Oh yeah. It's—

09-01:38:05

Burnett: That's fascinating.

09-01:38:06

Leitmann: Yeah, it's—maybe I—

09-01:38:13

Burnett: So it is, you know—

09-01:38:14

Leitmann: We can find it. It's in the bibliography.

09-01:38:16

Burnett: I encounter this a lot in economics when I interview economists, because a lot of the work that the important economists have done has been to take the conventional wisdom, a given formula, and poke at it and see, does this really work, and sometimes they'll run, sort of as best they can, they'll run some tests using natural, basic, time series of data. Is that what you were doing with something like the Volterra equation? You're just like, wait a minute, that doesn't make sense.

09-01:38:51

Leitmann: No, I looked at it, and how I picked the inverse, I really can't tell you. From a population dynamics point of view, why that should be the case, I have no idea. I've talked to Feichtinger about it who is a population dynamicist—that's what he got his degree in, University of Bonn, and I talked to a couple other people. They sort of lost interest. They didn't think that was so exciting, so I'm still pondering how come that happened.

09-01:39:28

Burnett: Yeah, it's a really interesting question.

09-01:39:30

Leitmann: Yeah, and it's—

09-01:39:32

Burnett: Can I ask you something, a very naïve question, and it might help outsiders, people who don't know this kind of math, about fluency in the language of mathematics? So, for example, in preparation, I was quote/quote reading these papers, which meant I would read the English parts of it, and then I would come to the equations and I'd be like, I can see that that is calculus of variations, but I don't know really what's happening in this, and it's therefore

this, and I can't tell you where you got from one equation to another. As a reader of mathematics, do things slow down incredibly when you come to that equation? Do you pore over that, to look, to see whether it makes sense?

09-01:40:27

Leitmann:

Well, I think there's a basic reason for that. When you write it up, it all seems to flow logically. Getting to it isn't that flow, very often, a completely different process, yeah, but writing it up, it makes it easier to read in a sense. You don't have to go through all the thought processes; you just prove one thing from another, that's essentially the result. So, in many cases, the actual process of getting to that flow—this is why, you remember, I mentioned to you when I was working with Blaqui re, and we wrote, I think it was our chapter in *Topics in Optimization*, and even part of the book, every time we came to a point where we thought we had proved something, we would have a drink of cognac, and the next day, we've thought about it during the night, and it turned out we hadn't proved it. So it's that kind of hit-and-miss kind of approach. You think you have it and sometimes, if you're lucky, it works.

09-01:41:44

Burnett:

So, for example, in some of these papers, is it that there is a conventional wisdom about here and you present it? I think this was true of some of your rocket research papers and actual—you would illustrate the conventional wisdom, but your contribution was in fact perhaps just an equation, or a short sequence at the end of it. Is that true, or is that—

09-01:42:17

Leitmann:

Yeah, that's—

09-01:42:17

Burnett:

—common? Is that common?

09-01:42:18

Leitmann:

Well, I think that's certainly not unusual. I don't think that people think in this, this follows from that and that follows from this. Very often the path is much more crooked than appears when you write down the final result, that you've tried to clean it up, obviously. Why take people through the agony, unless you're writing a history of—but, in terms of the subject matter, that that's completely irrelevant really, but if you want to do philosophy of science, that's something else again, or psychology of science or—

09-01:42:58

Burnett:

It's just, I think to help people access the work practice, and the—because we see the complete papers, and we don't understand what went into the hit and miss, as you say, or the false starts, the agony of being stuck, because you're often stuck at a certain point. One mathematician I spoke with talked about a lot of his breakthroughs come from not sitting down with a pen and paper working something out. It's doing all that work and then going away and going to the symphony or something, and then it snaps into place. Cognitively, does that resonate with you?

09-01:43:41

Leitmann:

Yeah, that happens. I don't think that happened to me a lot, frankly, because my mathematics was never that sophisticated. Take, for example, how did Einstein get to such a simple equation, E equals $m c$ squared, for such an incredibly difficult—and again, you can say, with many of these theories, that “they really have no application because they really don't deal with the real world.” But unfortunately, Einstein did deal with the real world, right? They didn't make the atom bomb out of just a pipe dream. So, it's an interesting process, and until I got to doing this the last few months, I never thought about it. No, I never said, “How did I get from here to there,” because—

09-01:44:51

Burnett:

You were so immersed in it.

09-01:44:52

Leitmann:

Well, and I had this crazy way of operating. When I had an idea and I got stuck, I just said, “Okay, I'll write up what I got so far, and then somebody else can go alone or join me and do something,” and both of these things happened, and I'm sure it also didn't happen in many cases, but, so that whatever progress was made, I think, really came from the willingness not to insist that I have to get all the answers right now.

09-01:45:27

Burnett:

Right. I think that's an important component for—

09-01:45:30

Leitmann:

Well, it was in my case, not for everybody. Einstein never got over not getting a field theory. The last twenty years of his life, he was really stuck there. It got to—

09-01:45:42

Burnett:

On the unified field theory?

09-01:45:43

Leitmann:

Yeah, it got to the point that he had to go to the *Journal of The Franklin Institute* to publish. People wouldn't even take his papers. I'm sure that people have done a lot of work in psychology about thinking processes, and what leads to new ideas. First of all, there are very few new ideas. I'm convinced of that because no matter what we do, somebody else did it first, sometimes better and sometimes not as good. It's what I talked on in the thank-you remarks when I got the Bellman Award, in about 2009 and they always ask you to talk for about fifteen minutes. Usually, people talk about their work, and I picked the topic, “Who Did What First?” And, my conclusion was that it's really not that important, because if you try to prove that you did it first, as Isaacs did, vis-à-vis Bellman—he wasted fifteen, twenty years of his life. He actually could have produced a lot more. He was very, very smart, but he spent the last years of his life just trying to prove he did it first, and I think that's too bad.

09-01:47:17

Burnett:

Oh, there's the story of Armstrong, right, who developed a version of the vacuum tube? Now I'm talking off the top of my head in my history of technology, but he got into a patent dispute with RCA over it, and it killed him. I think he committed suicide, and it completely ruined him, and other people learned from that, and other inventors, when they were getting ripped off by companies that were infringing on their patents, they decided not to pursue it because it wasn't worth it.

09-01:47:52

Leitmann:

Well that, there at least, there is a monetary incentive. For a lot of people, it's just pride, and I'm always surprised. If it appears that I thought of it first, I can't think of a thing I did that didn't have somebody whose shoulders I was standing on, and only a little bit. So, I'm convinced that—it was even true with Einstein, of course. There were people that got very close to relativity theory, which is another misconception of people. They think that these geniuses, the thought of this suddenly popped up, and when you look at what they did, you can see that there were very often predecessors, and some of them very close, so the genius sort of puts the crowning conclusion to the subject, but—

09-01:48:58

Burnett:

And the position they have already, sometimes it's a Charles Darwin, for example, he was in the right position to be considered a gentleman scientist, whereas Alfred Russel Wallace was not. Fortunately, Darwin shared credit, and that was very nice of him, but all this to say that pride is a sin in science, as in other things. [laughs]

09-01:49:21

Leitmann:

No, but it's a waste of time. It's a waste of time. You can be proud after you've done something of what you did. That's a sort of different kind of pride, but too, it's all right. The sun is, yeah, the sun moved these days—no, actually, the earth moved, and—[laughter] no, it's very interesting.

09-01:49:44

Burnett:

You spoke earlier about—you said your math isn't very good, but you spoke about your ability to be a critical eye to others, when I asked about, why are you involved in this, and you said people are honoring you. That's one thing, but you have a critical eye. You're a good sounding board, it seems. Is that fair?

09-01:50:06

Leitmann:

Yeah, and maybe partly, maybe mostly, because of my ignorance, I ask questions: "How did you get from here to there?" Sometimes they can tell me. Sometimes they say, "You know, I don't know," and then they say, "Well maybe it isn't right, if you don't know how you got from here to there," and all that has happened.

09-01:50:28

Burnett:

How does that happen that someone comes up, “Here, I’ve got a new approach to this theory, here’s my evidence,” and there’s a chunk of it where they’re not sure, does some of the previous accumulated knowledge come in chunks, and you can just, “Well we know that this is this,” and you put this chunk of the equation in?

09-01:50:48

Leitmann:

Well, there’s another aspect to this, which I think is much more serious. Nobody in his right mind, now—you know, during the seventeen, 1800s, there were few people who worked in these fields, so you could actually go back and try to re-derive their results. If you tried to do this, there are certain things that are known, theorems that are known, but how do you know that the people who derived them didn’t make a mistake? And of course, there are instances where they did. So everything that’s based on that thereafter is probably wrong, and that is the serious thing, you see, and this gets worse and worse because the amount of knowledge that’s produced is growing exponentially. And so now, when I used to talk to younger people and I’d say, “Well so and so did this,” they say, “I don’t have time for this. I just don’t have time,” and it’s true. They would do nothing if they went back to all the references and made sure that they were correct.

09-01:51:58

Burnett:

So there’s this portrait that, I mean, historians of science have to deal with this all the time. There’s the Kuhnian notion of scientific revolution, as that really only applies to physics and for only part of its history, but this notion that there are these paradigm shifts, and that during, there is a period of normal science where there’s this gradual accumulation—people add to the existing edifice of knowledge, and everything gets incorporated—and then it becomes unstable and unwieldy, becomes too large, like a big molecule or something, or an isotope, and so it falls apart, and then it’s reconstituted with a new way of explaining that is incommensurate with the old way. But what you’re talking about is that things are discovered, and forgotten, and rediscovered. There’s Mendelian genetics and then there’s the independent rediscovery of Mendelian genetics in the beginning of the twentieth century, and you’re talking about numerous examples in your own lifetime, in your own career, in your own field, where the march of science is so rapid, and there’s such pressure that there is incremental innovation based on a convention of existing accumulated knowledge, that may or may not reflect actual accumulated knowledge.

09-01:53:24

Leitmann:

Yeah, well, first of all, a very large percentage of what is published is very specific, it’s not fundamental, and that adds to knowledge of course, but not necessarily to basic knowledge or understanding. It may be useful result, it may be of great importance at that time, but it isn’t really sort of at the basis of science and mathematics. So the people who do very basic things, for which very often nobody knows what it’s good for until a hundred years later, those

are very rare, so most people sort of peck around, pick a particular problem or a sub problem or—I told you I had a colleague who had a lot of graduate students, but the theses all were just small variations, just a little change in the model or what was wanted.

09-01:54:43

Burnett:

Right, so they're miniature versions of the senior professor.

09-01:54:47

Leitmann:

In a way, in a way, yeah, and that makes life easier, and it may even be useful because that little tweaking may be important, but it's not something that sort of adds to the basic knowledge of the field, so, but everybody has to go to heaven in his own handbasket, so—

09-01:55:16

Burnett:

[laughs] Well, it's maybe a plug for doing some kind of historical research in one's own field, right? What makes the field a field? And it's you, it's other people I've interviewed, other notable scholars, scholars who are known for blazing a new path have often had the humility to look at how a path may or may not be new, so they're looking at the past. They're looking at Lyapunov. They're looking at Euler. They're looking at these—

09-01:55:51

Leitmann:

Well, this is why I was very grateful when it was pointed out to me, and actually, Dean Carlson wasn't the first. We had a meeting in Corsica, I don't know, anyway, but it was for my seventy-fifth birthday, I guess. It wasn't that long ago. It was a little symposium, and the French have a wonderful sort of meeting center but it isn't really a center, in a fishing village in Corsica.

09-01:56:27

Burnett:

Oh my goodness.

09-01:56:29

Leitmann:

And it was at that little fishing village, and a number of the people I've mentioned were there, and there's an important French mathematician—anyway, he was, in fact, the technical director of INRIA, which is the French NSF, and he was at that meeting. We knew each other. We'd met before and all that, and he also—I gave a paper on the equivalent problem thing, and it must have been after 2001. Maybe I just published that first paper extending my earlier note to systems rather than just minimizing an integral, and he said, “You know, that sounds very familiar.” He said, “Carathéodory did something like that, in his book in 1935,” and he was partially right, of course, that he used the same basic philosophy of proving something on the basis of—but he—of course, the difference, as we just said a while ago, was only really different that I got my equivalent systems in a larger class by allowing the variables to be transformed. In addition to what he did in the integrand, and that kind of thing, you have to say, first of all, “Let me see if it's so,” and secondly, I'm not insulted. In fact, in a way, it's a compliment that somebody even took the trouble to think about it.

So, I think it's the only way to do, that otherwise, you make yourself truly miserable. I can't think of a thing that I did where I could say I did it the first time. Really, I can't. The same thing with Hamilton's principle: Bolza in the 1800s did what is called the inverse problem. He asked the same question about integrands, and I went one step further and said, "Well what are the forces?" So if you say, you know, he got what are called Lagrangians now, the integrand of that integral, and if you apply now the necessary conditions of the calculus of variations to it, it gives you the equation, okay? And to this day—by the way, there's a paper in here, the last paper I think on the list, which I have a colleague in the department, Fai Ma, M-a, and we have a paper.

09-01:59:30

Yeah, here it is. Yeah, it's the last paper on the list, in fact, "The Inverse Problem of Linear Lagrangian Dynamics," and it's by a couple of students and Fai Ma, *Journal of Applied Mechanics*, and put there on purpose because my little notes appeared in the *Journal of Applied Mechanics*. So, it was a continuation of the same problem, but for systems, not just for one single variable, and in fact, but to this day, nobody's got beyond proving that beyond linear systems, and he was able to prove it by essentially getting conditions that will assure that the equations of the system get decoupled, again, a transformation of variables, and then you have, if they're invertible, you have n systems, and each one of them is a system in which you have only your own state equations and your own integral. So you have as many of the kinds of systems that I looked at, you know, for a single variable.

09-02:00:51

Burnett:

It's important, I think, for people to recognize the unfinished business of science. We'd like to think of things, "oh yeah, we figured that out, we figured this out."

09-02:01:04

Leitmann:

Well, this has to do, of course, with the fact that there are two types of people, both in the lay population and smaller number, smaller percentage in the science population, who believe that there is an explanation for everything. I think religion, of course, is one answer, okay. That's an explanation, but it's a different meaning in science. Explanation means that you have a methodology that will say, "If I'm here now, I can predict where I'll be tomorrow," kind of thing, and that prediction via the equations of the system. That is taking God, whatever that means to different people, is really that mechanism, right? So, but the difference is, there are some sub branches of religion, I guess, which allow for that to be an evolutionary process, the notion of God, but in most, it's really a set thing.

I am convinced that we will never know the answer, and yet there are a few scientists who say, "No, no, now that I have sixteen variables and blah, blah, blah, that's the explanation," and I'm convinced that—I mean, Hawking, we talked about that when he sat next to me at the Franklin Institute event where he was winner of the Franklin Institute Medal, and he, I think, began to feel

that way, that it's that. But the whole thing that became string theory, for example, that was it. You don't have to go beyond that, and I'm convinced that we will never be able to say, "We can't go beyond that," for a number of reasons, because the verification of your result will become a much more sophisticated process as instrumentation and everything gets better. And so, leaning on having shown that your theory is right because it works in the laboratory, is a very dangerous thing.

09-02:03:35

Burnett: It's turtles all the way down.

09-02:03:37

Leitmann: Yeah, well, it's great if people are enthusiastic about what they did, and feel very good about it, and say that "it's a great result." Very often true, but to claim that this is it—

09-02:03:51

Burnett: Yeah, a naïve positive is a—

09-02:03:53

Leitmann: Well, I think this is why the difference between explanation, in the usual sense, and the explanation in science is not the same thing, and so explanation in science is sort of predicated on the fact that it's only explained now, may not be explained tomorrow, and I think that's something that most people miss, and once you're sort of—this is why I'm an agnostic, not an atheist, because atheists also have a religion: they know something. Agnostics say, "I don't know anything," and I think there's a big difference.

09-02:04:39

Burnett: No, no, absolutely.

09-02:04:40

Leitmann: It's very interesting.

09-02:04:43

Burnett: Well, perhaps we should pause for now, and next time, we're going to talk about some elements of teaching, and also your long career in the administration of the University of California, Berkeley, and—

09-02:04:57

Leitmann: Well I think that wasn't so terrific, but I think I'm interested in service, and that can be any number of things.

09-02:05:10

Burnett: Right. Well let's be more open about it then as we—

09-02:05:13

Leitmann: Yeah, and then we'll finally get to the personal things, because there are some people that I just have to mention as being it, in my life.

09-02:05:25

Burnett: Yeah, absolutely, the “without which not,” right?

09-02:05:29

Leitmann: Very good.

09-02:05:30

Burnett: Very good.

Interview 10: January 14, 2019

10-00:00:24

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History Series, and this is our tenth session, and it's January 14, 2019, and we're here in the Berkeley Hills, and last session, we were talking about robust control, and the wide range of research projects that you had undertaken, and we can't really mention, apart from just mentioning, all of the postdocs and all of the graduate students you had. You have significant relationships with most if not all of them, and they have had these illustrious careers, in academia, in industry, and so on. So, we are being somewhat selective, I think, in choosing to mention figures. It's not just about importance; it's about something unique or special about them that's worth talking about, and so, I was wondering if you could talk a little bit about Sandeep Pandey, and he was an interesting student of yours, and noteworthy in a particular respect. I'm wondering if you'd talk to me a little bit about him.

10-00:02:01

Leitmann:

Sure. He was actually my last doctoral student, must have been 1992. I had a couple of Indian students before that: Aggarwal, for example. Sandeep, first name, was, I don't want to say somewhat unique, because that's not right. He was unique, because he had already established at least an intellectual life while he was with me, doing his thesis research. He started an ashram and it turned out, of course, that this was not a surprise to him, because he was already very involved as a social activist, as was his wife, who had a PhD also, not from engineering, and so he started this organization, foundation, whatever you want to call it, to support education for very poor people—loads of those in India, unfortunately—and this grew over the years, very quickly, into quite a large organization.

So, one of the things he did, first of all, which may be somewhat different from other students, was that he immediately returned to India, as soon as he got his degree, and he got a job as an assistant professor in one of the IITs [Indian Institutes of Technology], the important engineering schools—I don't know which one offhand—and he immediately got into trouble. The teaching assistants were on strike, and he joined them in their cause, which made the university unhappy, and the other thing he did almost immediately is get in touch with a colleague in Pakistan, who was a physicist. They, an engineer and a physicist, were able to interpret the budget of the Indian government, the published budget, which said nuclear research, but was primarily to improve the atomic bomb which they had. That also didn't endear him to the Indian government, and in fact, he did end up in jail.

10-00:04:50

Burnett:

It was to expose the extent to which they were working on—

10-00:04:52

Leitmann:

Yeah, to which that budget really was primarily or largely military, rather than, as it stated in the budget, that this was for doing the nuclear research. So that didn't sit too well, and, this was also an attempt to work for peace between these two countries. Both his colleagues there and he worked on that, and so they were in the India-Pakistan peace movement which is of course, again, not beloved by the Indian government, or at least some of the parties, the major parties. He became sort of a holy man. He came to see me maybe a month after September 11 going to a peace meeting at Princeton University, and he was in white robes, and he had a beard, and it was not the time for that kind of person to travel, but he didn't have any problem, actually, no, not in the United States.

So, to this day, he's involved with this organization, and again, the peace movement, he supported. There was a peace march late last year. I forget now offhand who was the Indian leader of that, but he was one of the people on the committee that supported him and so on. So he's still, now that he's getting older and maybe a little bit more subdued, I don't want to say conservative. He again went into teaching. So he does that, and his activist activities to this day, and he came to see me just about two years ago, and in fact, brought me a present that's lying over there, and we've stayed in touch in terms of the involvement of his interests with social activism, not on engineering problems. That essentially took really a back seat, and, the thing that helped him, maybe kept him from being re-arrested, was the fact that about ten years ago, he won the Magsaysay Prize, which is a prize the Philippine government gives. I understand this was actually supported by the Rockefeller Foundation. I looked into it, the money involved, and it is given for really a number of things, as the Nobel Prize does. This is more like the Nobel Peace Prize and that, of course, created a lot of, well, first of all, publicity, and secondly, a brake on being too overt in trying to put him down. So that helped him a lot, of course, and he did give all the money to that ashram.

10-00:08:11

Burnett:

This is this organization, Asha for Education.

10-00:08:13

Leitmann:

That's the one.

10-00:08:14

Burnett:

Yeah, and that has a large profile.

10-00:08:16

Leitmann:

Yeah, and that started here in Berkeley, and there are chapters now all over the place, yeah, so—

10-00:08:23

Burnett:

So—

10-00:08:23

Leitmann: —I'm very proud of him. I think that that's better than doing engineering, frankly. [laughter]

10-00:08:31

Burnett: But you come into contact with extraordinary people doing interesting work, and when you're young, you become educated, but you also become exposed, and that can be a new path for you. It doesn't necessarily follow that you go—and you wouldn't discourage him from doing it. I mean that's—

10-00:08:50

Leitmann: No, no, I mean, I was very happy that he did that, and we talked about it, not ad infinitum, but we talked about it, and so I was very pleased with that outcome.

10-00:09:05

Burnett: There's Maj Mirmirani, too.

10-00:09:07

Leitmann: Mirmirani, yeah, Maj Mirmirani. I used to call him Dean. I don't know. Here he was known as Dean, or maybe when he became a dean, he didn't want to be called—

10-00:09:16

Burnett: Dean Dean, yes. [laughs]

10-00:09:18

Leitmann: Anyway, Maj, M-a-j, he was one of two Iranian students who came to me about the same time, must have been the middle eighties, something like that. The other one, his family name was Azgarzadeh, came from an extremely wealthy family. You know, maybe it was the late seventies. Yeah, I think so, because what happened was—

10-00:09:45

Burnett: For Mirmirani?

10-00:09:45

Leitmann: —the revolution happened, and Azgarzadeh's family was of course in big trouble because they were owners of plants, industrial plants and so on, and Azgarzadeh was going to work with me, and it was just about the time, so I know it's just about the time I got extremely busy with what you would call service. So, we shared him, George Oster and I. George Oster was a really outstanding person in his field. He was interested in more biological problems, population, biological populations, and also medical aspects that could be treated as an operations research subject, and he was very happy to have him, so we co-chaired his doctoral committee. No, I think I'm thinking of Mirmirani. I've got this all screwed up.

10-00:10:58

Burnett: Yeah, well Mir—

- 10-00:10:58
Leitmann: No, it's Mirmirani, Mirmirani, because he did—
- 10-00:11:01
Burnett: He did control theory.
- 10-00:11:01
Leitmann: —the population work, yeah, right—
- 10-00:11:03
Burnett: Applied to human immune response.
- 10-00:11:03
Leitmann: No, Azgarzadeh actually wanted to do—now I know what. He wanted to do engineering of walking, skeletal engineering and that kind of thing—
- 10-00:11:12
Burnett: Oh, cool.
- 10-00:11:14
Leitmann: —and he—yeah, but it turned out that in the end, he decided he'd rather do traffic engineering, I guess maybe just influence from his family, and so, I asked him to do that in civil engineering where that subject is one of the major subjects. So then he moved to, Azgarzadeh to civil engineering, and Mirmirani is the one who was co-chaired by me and George Oster, who, by the way, just died a very short time ago.
- 10-00:11:50
Burnett: Yeah, George Oster did.
- 10-00:11:51
Leitmann: Yeah, I think, in the spring of last year.
- 10-00:11:56
Burnett: Yeah, it's very recently.
- 10-00:11:57
Leitmann: Very tragic, because he was outstanding. He was a MacArthur Fellow, well deserved, and really extremely bright, about twenty years younger than I am, and he died very early.
- 10-00:12:13
Burnett: So in the eighties, just broadly speaking, there is a turn, isn't there, towards the life sciences, because there's a lot of federal resources being devoted to research in that area and if you can turn your ship towards that, it makes things a bit better.
- 10-00:12:32
Leitmann: I had a visiting researcher, I think we talked about him already — Veijo Kaitala, from the University of Helsinki, who was for obvious reasons, given that Finland is strong in the fishing industry, interested in applications, and we

did about six or seven papers just on the fishing industry, maybe more, a couple of them with Hilden who was a colleague there but really had nothing to do with me personally, and those were very interesting to me, robust control particularly seemed to be a possible tool to deal with that, and that ranged all from allocation of fishing rights to employment of fishermen, the whole economic problem, pricing and that sort of thing, and those are very interesting problems. Veijo also got a little bit interested later on in some of the work that Pandey went into having to do with aircraft flight in downdraft conditions. He co-authored a couple of papers in that, but that was quite a bit later. I have not been in touch with him since then, so he is probably one of the few that dropped out. He's still around. I check on him occasionally, but we haven't kept up a correspondence as I did with almost all the other ones.

10-00:14:07

Burnett:

And so there's this powerful tool that can be applied to so many different—

10-00:14:11

Leitmann:

Well—

10-00:14:12

Burnett:

—problems—

10-00:14:12

Leitmann:

—it's, yeah, it's—

10-00:14:14

Burnett:

—or a set of tools.

10-00:14:14

Leitmann:

—it's one tool, of course. People who go for other tools will say, "That's the tool." There are many tools for doing this and that happens to be the one that I got involved in.

10-00:14:27

Burnett:

And there was one with respect to earthquake engineering. This is an ongoing area of research here at Berkeley for obvious reasons.

10-00:14:36

Leitmann:

Yeah, Professor Kelly in civil engineering was really the primary person in base isolation of buildings. There's only one building on campus that is fully base isolated and that's Hearst Mining Building. It was actually lifted off the ground to put that in. The whole building was jacked up with pneumatic jacks for quite a long time, and they put the springs—this is a system essentially of supporting the building up and down, and very little in the—

10-00:15:14

Burnett:

Lateral movement?

10-00:15:15

Leitmann:

—in the lateral movement so that very little lateral forces would come from an earthquake. In practice, of course, there has to be some transmission of forces, but the main support of the building is really in the vertical direction, and so if the building stays where it is, and goes up and down, and then the ground moves under it and there's very little force, one hopes, transmitted. They found out that Hearst Mining, was by far the unsafest building on the whole campus because, when it was built, they built two walls, and filled the inside between them with the rubble, and there was no other structure. They put the structure in, of course, when they put the base isolation in.

One of the students who worked on this, one of my doctoral students, was a Greek student, Argiris Soldatos, who became also very close in a sense. He sort of anointed himself as a member of the family, and I'm still in touch with him. It's a very sad story, because his father worked for NATO, and so his father, as a Greek, was stationed in Turkey at one of the air bases, Agiris, and was and is a patriot—I mean, he told me stories about how Turkish planes are always over flying Athens, that kind of thing. We saw him a couple of times in Greece and he took us on trips. There was a meeting in Athens—another person is involved in that—was an international meeting on what used to be called cybernetics, but anyway, this must have been probably in the late eighties, maybe early nineties.

At any rate, he then took us for a full week's trip through Greece, and Nancy had to eat a lot of fish. I think it must have been after I became a vegetarian already, so this was probably maybe in the nineties, and he went into the kitchen and made sure that all this fish came out in buckets, you know—poor Nancy, every day she ate like three pounds of fish. Anyway, he's an extremely well-meaning guy. He had a Cadillac, an old Cadillac with big fins. He left that behind as a present for Nancy. Well we kept it in the garage for a while. Then finally, when he came back on one of his trips, he sold it. [laughter] So it was that kind of relationship, and it still is. In fact, I heard from him at Christmastime, and he said he's still expecting us to see him in Athens again.

10-00:18:36

Burnett:

Oh, wonderful.

10-00:18:38

Leitmann:

He's really a teaching assistant still, because they have a terrible system in places like Greece where there's one professor, and until he or she dies, they have five or six people who are vying for this one job, and they really exploit them. They do all the teaching, essentially, for practically nothing in the hopes that they are going to be the replacement, and Argiris never made that. He had a brother who was mentally deficient. He died last year. He had a sister who married a German engineer from Munich, and he wanted to move back to Munich, but she wouldn't fly. Once she went on the plane with him and they had to stop the plane from taking off; she had a fit, and I think they finally got

divorced. So he's one of these people with a black cloud hovering over you. So—

10-00:19:42

Burnett: So he's at the National Technical University—

10-00:19:44

Leitmann: Yeah, that's where he is—

10-00:19:45

Burnett: —in Athens.

10-00:19:45

Leitmann: —still an assistant, yeah.

10-00:19:46

Burnett: Okay. And, Mirmirani is now the Dean of the College of Engineering at Embry-Riddle Aeronautical University.

10-00:19:54

Leitmann: Right. He started to teach at, it was Long Beach State, I think. Anyway, it's either Long Beach or South—

10-00:20:08

Burnett: CSULA, I think, isn't it?

10-00:20:10

Leitmann: It was—

10-00:20:10

Burnett: Los Angeles?

10-00:20:11

Leitmann: No, it was—yeah, it may have been one of the state colleges. I don't think it was Long Beach. I think it was Southern Los Angeles, and yeah, one of those.

10-00:20:25

Burnett: That's right.

10-00:20:26

Leitmann: And he taught there, and he became a chair of the ME department there, and then when he took early retirement from there—that must have been ten years ago or so, or maybe a little longer—he accepted the job at Embry-Riddle in Florida. They have a campus also in Kansas, I think. I think it's Kansas, I was there once. For a while he was also the acting vice president of the place because one of the vice presidents retired, but he got out of that finally. Clearly because of that, I got appointed to their advisory board, and even though I can't travel that far anymore—it's down at Daytona, Daytona Beach—I'm still on the board so I get all the paperwork and I make my comments and stuff like that. So that was very nice. I really appreciate that. Similar thing happened to me with Stalford—we talked about him—at the

University of Oklahoma. I was on their advisory board, the engineering advisory board, for quite a few years, and there I went every year. I think sometimes they met twice a year, in fact—

10-00:21:57

Burnett:

Well, I don't want to jump to another topic right now, but since you—

10-00:22:00

Leitmann:

But that's in service; we'll talk about that, yeah.

10-00:22:02

Burnett:

Yeah, since you mentioned it though, I wonder if you could talk about the role of a senior engineer or scientist in these advisory roles at the smaller colleges. These are often through personal contacts. These are former students of yours who say, "Can you be on these advisory board?" What do you bring when you are on those boards?

10-00:22:29

Leitmann:

Well, I think that if it's somebody, say a professor who gets appointed to the board, he or she really brings there the experiences at his or her university. Those boards are usually made up at least half by industrial people, sometimes CEOs, sometimes vice presidents, people like that. So it's a mixture of these two, and it's the same for our advisory board here, that there's a large contingent from industry because they are the people who ought to tell us what they need. One of the things that I encountered and I happen to be in agreement with that is that we ought to teach—we're going to get into teaching very soon—we ought to teach fundamentals very well, and leave the very advanced methods largely to industry, because they very often tell us, and that's really where this came from in my—they said, "We have methods, methodologies, that are so advanced that, in fact, they're proprietary, and you don't know about them, and you're probably behind us." This is very hard to convince people of at universities, particularly deans who don't like to hear that, of course, because you know, so—

10-00:24:03

Burnett:

Do you concur with that having worked—

10-00:24:05

Leitmann:

I very much concur with that, but of course, I'm out of date. It's the same thing of having universities, particularly, having gone—in industry, that was always true—universities have gone to doing collective research. When I started, there were these ivory towers, sometimes overdone. Now, it's essentially a research *team*, so there may be two, three, four, five, ten, and they meet together. I think that's probably very good from the point of view of coming up with ideas. It becomes very difficult to discern who did what, and that becomes then an arbitrary thing that the professor decides. So there is that problem, and I never had that. I met, even when I had five or six students at one time in the seventies, for example, I met separately. They may have talked

to each other, of course—that’s another story, they should have—but we never met as a group, never, you know—

10-00:25:20

Burnett:

You wanted to keep that kind of reward structure, because your job is to identify excellence, in part.

10-00:25:27

Leitmann:

Well yeah, I felt that, how will I tell who did what, and I found out later on that this used to be, anyway, of great consequence, because when you say, come to a place like this where we have a budget committee that looks at the record very closely, how do you determine what was really done by the student. In the succeeding few years I had a very good example of that happen with a young woman professor in my own department, who was very bright and loved the students. She spent huge amounts of time on office hours. But in the first two or three years, she published one paper and that was essentially on her thesis, and that doesn’t go here. I happened to be on the budget committee at the time, and there they give people who are up for tenure—these are tenure-track positions—a warning after three or four years, that if you don’t mend your ways and publish a little bit more, you’re going to have a hard time getting tenure, and that happened to this woman.

In fact, I just ran into her after years, it’s really interesting, and she had another thing that worked against her. She was a rebel, and so she used to walk around, teach in a T-shirt, and go to the faculty club and use foul language, “pass the f’ing butter” kind of thing, and but this was, in a way, as I found out later on, a reaction to an older sister who was a medical doctor and extremely good looking. This girl wasn’t ugly, but she was a very plain—you know, and so this was all rebellion, of course, and she didn’t get tenure here. She had to leave.

10-00:27:42

Burnett:

Well there’s also a really well known and documented phenomenon of the gendered expectations of labor, and so women coming up through the ranks are often, they’re doing more service work, they’re investing more in teaching, and that ends up putting them, can end up putting them at a disadvantage. There might be a particular things in the case—

10-00:28:10

Leitmann:

Well—

10-00:28:10

Burnett:

—of this person too—

10-00:28:10

Leitmann:

—her case was exacerbated because when she was warned—oh there’s another aspect, since I’m not mentioning names, I can tell you what happened. I found out she lived in Albany in one of those little places, and she got raped.

10-00:28:25

Burnett:

Oh good God.

10-00:28:26

Leitmann:

Somebody crawled in the window and she got raped. So, she was in bad shape, really bad shape. Nobody knew this. Students and young faculty, for some reason, trusted me and so I found out from her, and then, when I was on the budget committee, the chair of the committee was a woman, and I made a motion that we ought to give her really time to overcome this and maybe give her an early warning so that she could publish one or two more papers, and she did that. She took a half sabbatical and made the same damned mistake. She went back to her old professor and worked with him that half year, and they published a paper together, and then when it came to the budget committee the next year, of course, they wrote to him, because both their names were on the paper, and they said, "Now who's the senior author of this paper?" And he said he was. That was the end of her. Yeah, really very sad story. So, anyway, the whole process of giving people appropriate, proper credit has always interested me, and so, I wanted to make very sure that people got credit for what they did, and that gets to be difficult when you have ten people working together.

10-00:30:01

Burnett:

Absolutely.

10-00:30:01

Leitmann:

On the other hand I see the advantage of it, so I'm not saying this is a bad idea. The only thing is, how do you reconcile this, and obviously, the feeling now is that it can be. Maybe it can, I don't know.

10-00:30:16

Burnett:

And this group work is becoming more and more common in industry as well, right, so this is a problem, and—

10-00:30:22

Leitmann:

Yeah, well, it's—

10-00:30:23

Burnett:

—a challenge, yeah.

10-00:30:29

Burnett:

I do want to talk about gender, and over the decades and engineering, and how the college has dealt with that. We can maybe leave that for a bit later, but I just wanted to finish up talking about some of the noted students that you had, and we were talking about vibration control. There was a significant amount of research that you were involved in perhaps indirectly, a lot of work on—

10-00:31:06

Leitmann:

Yeah, I think it was mostly postdocs who were working on that, like Reithmeier, for example.

10-00:31:12

Burnett:

Reithmeier, yeah, and so, I'm wondering if you could talk a little bit about Eduard Reithmeier.

10-00:31:17

Leitmann:

Well, he came to me about 1990, I think, yeah, pretty sure, as an Alexander von Humboldt Fellow, so he was well supported. They still ask that the professor contribute something to supporting the student and that was fine, but the major money came from the foundation. He had just received his degree maybe a year before that, at the Technical University of Munich, which is sort of the MIT of Germany, with Friedrich Pfeiffer, with whom I'm in monthly touch to this day. He became sort of the senior dean of Technical University, Munich, and we exchange emails at least once a month, very often on political topics. A very nice man, and also a lovely wife, Ruth, and they were my hosts—we haven't talked about the Humboldt Prize yet, but then with the Humboldt Prize, after you finish that, anywhere up to ten or fifteen years later, they will fund you for a revisit. It can be somewhere else, doesn't have to be where you were before, and I took mine in Munich with Friedrich Pfeiffer. Eduard Reithmeier, this postdoc we're talking about, was very interested, and the first thing we did together was to look at an application and a way of actually transmitting the appropriate reaction to diminish, for example, vibration.

10-00:33:39

Burnett:

So this is to send a counter-vibratory signal? Or—

10-00:33:44

Leitmann:

Yeah, essentially a control of the body that you know—

10-00:33:49

Burnett:

Through, it could be different techniques, damping, or—

10-00:33:51

Leitmann:

Yeah, could be different. In this case it was electrorheological fluid that he thought of. He'd come across that, and so the first paper we wrote was on that, and, we got a patent on that, he and I.

10-00:34:04

Burnett:

Cool!

10-00:34:04

Leitmann:

Yeah. I don't think it was ever used, I don't know, maybe. We have been very close ever since. I just mentioned to you they came; he and his wife came here, they were married while they were with me, for their twentieth wedding anniversary. So, it's, to me, personally, it's sort of satisfying making me happy if I can have a relationship beyond the technical one, because I think that I sort of look at a professor more in the sense of the old Greeks, where the students sat around in a circle and they were the family. It makes me happy. Maybe this is a very selfish thing or whatever, I had a number of students who, for example, had marital problems. One of them had already become a

professor somewhere else, and flew all the way from Europe to talk to me about what should he do, and my advice was get divorced, and the details aren't important there. Another student who had marital problems whose wife, whose former wife comes to see me. She visits me from the Middle East every couple of years, and somehow, it makes me happy.

10-00:35:52

Burnett:

I think we live in a very secular world, and apart, outside of your family, you need people who are outside of the family unit who offer support, not just in terms of technical, narrow technical things, but life. And so, time was when that would be the priest, or the rabbi, or some kind of community leader—

10-00:36:18

Leitmann:

Yeah, and it seems to me professors are in that group of clergy, if you want to call it that—

10-00:36:22

Burnett:

Right, they are, and you walk—

10-00:36:24

Leitmann:

—or at least should be—

10-00:36:25

Burnett:

—with that person, yeah, or they should be, exactly, and so, is there also a sense in which that approach, when you see yourself that way, that also has dividends for the scholarship; it makes more robust and durable scholars?

10-00:36:44

Leitmann:

Well—

10-00:36:44

Burnett:

Or is it just a happy accident—

10-00:36:45

Leitmann:

—no, no—

10-00:36:45

Burnett:

—that that's the case?

10-00:36:46

Leitmann:

—I think what it does is, if it succeeds is make them see this as one of the aspects of their job as a professor. And so this can be, doesn't always work that way, but it can be, and if it does, that's very good, and because to me, family in a larger sense is the most important thing in life. It's not the fame and fortune and money, which is very nice, but the thing that lasts, I guess, is really to have a close family, a large close family, and we can talk about this a little bit more when we get towards the very end when we can become maybe a little more—

10-00:37:39

Burnett:

Certainly, yes.

10-00:37:40
Leitmann:

—philosophical.

10-00:37:41
Burnett:

Absolutely. Well, it is there in our conversations about the structure of these interviews. You place quite an emphasis on, and listeners don't know this, but I think if you had your druthers, you'd talk a lot about your students, you would talk a lot about your postdocs, you would talk a lot about—

10-00:38:03
Leitmann:

Well they mean a lot to me, actually.

10-00:38:04
Burnett:

—even more than we have, and I think there's been, we've had to talk about you and to talk about your contributions, and I think your instinct is to think about — “I want to mention this person; I want to mention that person — and I think that's—

10-00:38:24
Leitmann:

Yeah, because they're important to me, some more, some less. As long as we're talking about this, people that I met really completely unrelated except that maybe through my assignments at the university, I had the chance to meet them and who grew into really members of the family, and later on we can talk about a couple of those, and in some cases, it's because somebody who is maybe like a postdoc to begin with, becomes a member of the family and then finds out that sort of mutual ties, and that happened to me, also. So it's, well, it reinforces the whole idea that we all have the same problems.

10-00:39:17
Burnett:

Yeah, right.

10-00:39:20
Leitmann:

Unfortunately, not the same solutions. I always, I keep repeating this. I've been extremely fortunate in this also, that I've been able to make friends, and I could, to this day make friends. They can't be very close until we know each other for some time, but it's because getting old is very lonely, and if you can have contact with younger people, that's a big plus, a big plus.

10-00:39:56
Burnett:

Absolutely. And we don't, in our society, we don't think about that.

10-00:40:02
Leitmann:

There are some models, retirement homes where they do that, where they sort of adopt each other kind of thing, but that's very rare, and so you have to do it yourself, if you can, and I haven't really done it consciously, but it just turned out to be that way, and that also gives you geographically a much larger reach because these people can be who knows where. So it's—

10-00:40:33
Burnett:

And, yeah, the Internet has made that—

10-00:40:36
Leitmann:

Yeah—

10-00:40:36
Burnett:

—better too, right?

10-00:40:37
Leitmann:

—much easier, yeah. I don't know what I would do without email, you know.

10-00:40:40
Burnett:

Right, right. [laughs] Well, teaching, let's maybe talk a little bit about teaching, and—

10-00:40:46
Leitmann:

Okay, so I came here in '57, and as an assistant professor. I think I was a step two or three because of my previous experience, and immediately started, when in the first two years, to introduce a couple of graduate courses. They were two-unit courses because the subject was not as broad as a three- or four-unit course would warrant. One of them was essentially aeroballistics, what is glamorously called rocket dynamics. It was essentially a first-year graduate course, and the other one, after a couple of years, was the calculus of variations because by that time, interest in that subject had grown a great deal, and also one of my colleagues, my really closest friend on the faculty, Reinhardt Rosenberg, became interested in that, so we spelled each other teaching that course, and in those days, you could have relatively small classes. That's really no longer possible, but if you—

10-00:42:10
Burnett:

How big were they in those days?

10-00:42:11
Leitmann:

—if you had three or four students, you could teach a class, yeah, and it got to be more and more.

10-00:42:17
Burnett:

Yeah, weren't we talking about a class that you attended where there was—

10-00:42:22
Leitmann:

Just—

10-00:42:22
Burnett:

—you were just you? [laughs]

10-00:42:23
Leitmann:

Well, it was with my thesis professor, yeah, the first year there were two of us, the first semester, there were two of us. Second semester, I was the only one.

10-00:42:34
Burnett:

We don't do that anymore.

10-00:42:35

Leitmann: We don't do that anymore, no, no.

10-00:42:39

Burnett: Well, so, I think one of the things, we're kind of segueing here from talking about particular students to talking about teaching in general, and there is a student that you didn't necessarily know, but of a student of yours in the undergraduate, he didn't—

10-00:42:57

Leitmann: Oh yes, yes, I—

10-00:42:58

Burnett: —go on, he didn't go on to be a postdoc or a graduate student of yours, but he was quite remarkably touched by you, by the teaching that he had from you.

10-00:43:08

Leitmann: Well, that came out much later, much later. Yes, I think you're talking about Stan Lucas.

10-00:43:16

Burnett: Yeah.

10-00:43:17

Leitmann: He took two courses from me in dynamics, must have been in the early sixties. About fifteen years ago, the development people, people who raise money for the college, told me that there was a person, a man, who was a donor, not for huge amounts of money, but every year he gave a regular donation to the College of Engineering in honor of me. They had never told me that and suddenly, maybe it was a new development person, said they ought to tell me about this, and they did, and so I asked who it was and they told me, and I said, "Well, if you tell me who it is and where he lives, I'll send him a thank you note." So we've been in touch ever since. Stan Lucas had a brilliant idea, after he got his bachelor's degree in mechanical engineering. He was not very happy with his experience here. He thought the teaching was crappy, and I and one other guy were really the only acceptable professors he had. He thought they weren't really interested, all they wanted to do was do research.

10-00:44:45

Burnett: Well yeah, he was quite—I spoke with him, and I think that—

10-00:44:50

Leitmann: Oh you did speak with him?

10-00:44:51

Burnett: Yeah, and I think he—in those days, I think, there was much more of an attitude of some of the professors who had training in the top institutions that the students were sort of beneath them, and this very hierarchical, and—and you were one of the few who took time to work with him, and you did not look down on him, you—

10-00:45:30

Leitmann:

Yeah, that never even occurred to me, that attitude. Maybe it has to do having come from a European system where that has been, of course, always the professor, and in fact, they say that an assistant to a German professor—this is no longer true, of course, today—could walk upright without a backbone. [laughter] That was sort of the definition of a student. [laughter] Well, I really appreciated that and it was a time when I went to Long Beach where he's located, now has Lucas Automotive Company that he founded, and he had the brilliant idea that—oh, this came out of the fact that he had a job, when he was a student, with a boiler factory in Emeryville, and found out in the process that in fact, the earliest automobiles were steam-engine automobiles, even earlier than electrical ones. He became interested in those automobiles and then in antique cars, that's what it was. He said to himself, these people who pay sometimes millions of dollars for one car, where do they have it serviced, repaired, or even restored in some cases? He started such a company and became extremely wealthy, even had small tire companies in Japan make the special tires—these things have huge tires sometimes. Then he invested heavily in real estate in that area, Southern Los Angeles and the Long Beach area, and became even wealthier. He's now a very big developer in Las Vegas.

And so we became quite close, and I had to go down there practically every month, because I was the executor of my one of my first cousins' estates, so, that could always be combined. I introduced him to a very good Greek restaurant. He had never eaten Greek food, and he's never forgotten that. Then I came down with Nancy occasionally, and our daughter, Elaine, came with us. He has about 100-plus antique cars. He built a special building, a hangar type building, near the Long Beach Airport. In fact, if we have time later on, I'll show you those cars, because Elaine took a picture of each one of them, and then also of the arrangement, and made up an album for him, and then he wrote on the front page saying why we're so close, so he was very pleased with that. The problem is that he told me right at the outset, he doesn't want to be bothered with development people asking him for money. He said he'll stop giving money if they ask him for money, so I red-flagged him in the development office, and it wasn't until maybe five years ago when the then dean of the graduate division went down there. He actually went to a technical meeting in Long Beach, and I somehow got him introduced, and show the car collection to him, because nobody may see that collection except other—

10-00:49:22

Burnett:

Enthusiasts.

10-00:49:22

Leitmann:

—people with collections and family, and he really had no family. So, I was one of the first people that got to see the collection, and we're still in touch. I just had a very nice letter from him at Christmastime. He always says he's going to come and see me, but then he gets busy in Las Vegas, and so I don't expect that, but we're close, not in a familial sense, but just close personally.

Yeah, funniest thing that happened about ten years ago was, he picked me up, and Elaine was with me, at the hotel in Long Beach where I always stayed—my cousin was always also very close to Elaine—and he picked us up in a 1935 Chrysler Convertible, and it was the automobile that the silent movie star—what the hell was her name? Anyway, she was a silent movie star. She was the mistress of Bugsy Siegel, the—

10-00:50:35

Burnett:

Not Mary Pickford.

10-00:50:37

Leitmann:

No, no, no. [Theda Bar]. Anyway, Bugsy Siegel, who was a gangster, she was his mistress. This was her car, and she wasn't the only one, and so she threatened him. She said, "If you have all these affairs, I'm going to let the police know about you." She lasted only about a week after that. She "committed suicide" by shooting herself in the back of the head, and that happened in the front seat, the passenger seat of that car. Stan assured me that it had been reupholstered, [laughter]. When he arrived at the hotel in this thing, this thing is about, I don't know, twenty feet long and about this high. It has only two seats, and a rumble seat, but the rumble seat has doors—don't have to climb in over the top—and Elaine, I have a picture of Elaine in the back there, and I am in the seat of the "suicide" victim, and it made—it was a big deal and people came running out of the hotel to see—

10-00:51:55

Burnett:

I'm sure.

10-00:51:55

Leitmann:

—this thing. [laughs]

10-00:51:57

Burnett:

I'm sure they did.

10-00:51:57

Leitmann:

Anyway, so there are fun parts to this, of course. What I'm saying is, it isn't all serious.

10-00:52:05

Burnett:

Well, and this is evidence of your teaching enthusiasm, which is not just limited to graduate students and postdocs, and working on research with colleagues; it's that you took teaching seriously as a calling all the way to the undergraduate level—

10-00:52:25

Leitmann:

Well, and learning, well, you mean in terms of Lucas. Yes, in fact, it's a much more difficult job to do that, of course, but for example, as a teacher, I had a rule. I told students, "If you had a party last night and you're very sleepy this morning," at some eight o'clock classes, I said, "don't bother to come in, or if you don't want to come to any of my lectures, I hope that's not the case, but if you can get the information some other way, to pass your examinations, that's

okay with me. I won't be insulted," and occasionally, a student did. They came very rarely to class, and those were sometimes the good students who knew how to get the information out of books, reading papers, and that kind of thing. That's what they'll have to do eventually, so I told them, "This is the one thing you want to learn, is how to get information that you need, and maybe that's the only thing I can teach you, actually, because that's what you're going to have to do on the job. There isn't going to be anybody giving you instruction."

10-00:53:39

And so I think that this worked out very well, in fact. I have unerring aim with chalk. Somebody reading the *Daily Cal* in the last row would always get one right through the newspaper. [laughter] Maybe that wouldn't go today, but it did go, and, secondly, thirdly, I was very fond of open-book examinations. Sometimes the university didn't allow it for finals, but for other examinations I gave, for example, a weekly ten-minute quiz just to find out what really didn't go across. I told them, "You may bring anything you want. You can't bring another person with you, but anything else, I don't care," because I know that if they didn't get the basics, and I warned them about this, they would be worse off, because they would be looking for an example just like the one that they got in the book, and by the time they found anything even close, the exam would be over, but there were a few who tried it and found out very quickly that that doesn't work too well.

So that reinforced the idea that you want to learn the fundamentals, because that will tell you whether, in fact, what you're looking at in the book—which sometimes, given some of the books that we used, isn't always [laughs] okay—that you won't be able to deal, once you go out on the job with getting new knowledge, and using the old knowledge of what you learned appropriately. There is still to this day a fair number of publications which are wrong because the people misused what they learned, and again, not necessarily because of them, but because it was not well taught, and so I've always felt the fundamentals in science may change. It is a very difficult problem, because in the end, you always go back to somebody's result, and particularly in mathematics, so-and-so's theorem, and you can't go back and try to reproduce the proof because that's all you would be doing.

10-00:56:19

And so you're always dealing with this risk, that in fact, what you are doing is all wrong because what you're using to justify it is wrong, and I always tell people, "This is a danger that you have to live with, so don't think that just because, particularly, if you get a result that doesn't make sense, although that's not the—it's just a warning signal. It may be the theory, it's got a flaw in it, and not in terms of what you are doing with the theory but in terms of what you're basing your results on," and that's an ever-growing problem as this exponential growth of knowledge takes over, and students—and in fact, the sad thing is that we are devoting less and less time to fundamentals, and

more and more to techniques, and I was always told, particularly in Oklahoma where we had some very distinguished industrial people, that “the techniques they learn here sometimes are not useful because they already have better ones.”

Now, there are notable exceptions, of course, and particularly probably in computer science, and this skepticism about knowledge, particularly knowledge that is transmitted in journal publications, with this huge increase in journals and number of papers, the danger of getting something which is incorrect and then using it grows, and so the complexity of the system grows, and that’s why you got what are called X events, because it gets so complex that nobody can penetrate where this all came from. Then you’re taking big risks, and that is one thing I feel more and more happening. There are only so many units that you can have in a program, and if you add now another advanced course, you’re taking it out of the basics, and I think it’s a real problem.

10-00:58:56

Burnett:

So, back in the founding of the Royal Society, they developed their motto, *nullius in verba*, which is, roughly paraphrased: don’t take received scientific authority; don’t take their word for it; find out for yourself; and you’re saying—

10-00:59:16

Leitmann:

Yeah, and you know—

10-00:59:16

Burnett:

—that we’ve maybe lost some of that emphasis on—

10-00:59:20

Leitmann:

Well, it’s, what, a denigration of the ivory tower, and it’s sometimes put to me very starkly. People tell me, “We just don’t have time to go back and look at all the references, because that’s all we’d be doing,” and I understand that. It’s a real problem now, and people become more and more specialized. Look at medicine. It’s certainly true in medicine. It’s true in engineering too, and I think it’s a major problem.

10-00:59:50

Burnett:

Is there a reproducibility problem in engineering. I mean, this is true in the social sciences, where you have these established experiments, and then someone goes to try and reproduce them, and they can’t, or the experiment itself —

10-01:00:10

Leitmann:

Oh, you mean, okay—

10-01:00:11

Burnett:

—becomes so complex—

10-01:00:11

Leitmann:

—in other words, I can't tell how the guy got from here to there. Well there are many reasons for that. I may have mentioned this with my Soviet colleague, Revaz Gamkrelidze. I once asked him, "Why do I have such a hard time reading some of the applied math papers?" and he explained it to me. He said, "You know, we put just as much emphasis on publication as you people do, no question about it, in academic life, but," he says, "we don't want to put so much detail in it that the other guy can reproduce what we got, and then advance in doing that to the next step too easily, and so we want to have a result that's correct, but not let the other guy know just exactly how we got there." So sometimes it's on purpose.

10-01:01:08

Burnett:

Right. So teaching evolves during your time at Berkeley. In Stan Lucas's day, there was this attitude, according to him, which was, "Come, come, gentlemen, you should just follow along." If students fell behind, that was on them. It was their responsibility to just keep pace. You had an approach where you understood that you need to help a student get from where they are, to a point where they could learn themselves, or at least be fortified for the future that they were embarking on.

10-01:01:51

Leitmann:

Yeah, that's one aspect of it, certainly. I think the other one is that we spend a tremendous amount of time, certainly, say, in dynamics, on special cases, to the detriment of the fundamentals, because there is only so much time, and that leads, very often, to at least difficulty and maybe just the wrong results, because you don't know quite what the assumptions are that made you go to the next step, and sometimes the assumptions are vital, but the person who reads the thing doesn't know that, and as I say, it's particularly true in publications, less so in books, but it happens in books too, of course, and one example I gave I guess was that the chair of the department when I came here had a very popular book, one of the first really popular books, because he had very good examples, but he had to deal with systems of variable mass, and he had it all wrong. You know, for example, you look at, how do you weigh something when you let a chain fall onto a scale? Now what ends up at the scale is a system of increasing mass, and the equations are just saying that the mass is a function of time and $f=ma$ [force equals mass times acceleration] is the other story, and it's not true, and so, it's a problem.

In fact, when I came here, because I had just gone through this very step, and I must have told you, did a stupid thing. Graduate students, doctoral students, they're supposed to give occasional seminars on what they were doing, and I was discussing the dynamics of a variable-mass system and gave as examples of the wrong way of doing it his book, and he was the chairman of the department, [laughs] and I learned not to do things like that anymore. It's one of the first times I learned, that I should have talked to him first, but I didn't warn him.

So, I used the dynamics book, because I was a physics major at that time. I had taken my basic dynamics in the physics department at Columbia, but that book had almost no examples; it was just all theory. And so Goldsmith, my thesis professor, and I put together a little book on examples in the chronological order of his book—and put it out as a paperback. But then he learned very quickly. He wasn't stupid; it's just that that's the first thing people do. If mass is a function of time.

Another example: there are whole sections in books on the motion of particles, so if it's particle dynamics, classical Newtonian mechanics, and then, students get problems of an elevator moving, a doughnut rolling, and the students say, "How is this possible? They're not particles, so how do you decide this is okay?" and you don't have to decide that because you're not talking about particles. Once you've got a particle system, and then a collection of particles sort of the Greek atomic view of the universe, then you can show that $F=ma$ for the center of mass, and so whenever people say "particle motion," they really mean the motion of the center of mass of the body. In a doughnut, that's in the middle of the hole, so there is no particle there. Once they learn that, they don't have to make assumptions, how "small the body is — you read this in examples: "Let us assume the body's sufficiently small so that it can be considered a particle." That's bullshit. That's not what you do.

10-01:06:47

Burnett:

You don't need to think about it that way.

10-01:06:49

Leitmann:

No, you're talking about the center of mass, and then you have to say, "Okay, that's not all the motion that's involved, because suppose the body may be a rigid body that turns." Okay, you're only talking about the center of mass of that body. You don't have to worry about, is it a small body, is it a big body, and somehow, the emphasis on that was just lost when I was teaching, and even fairly recently, I still read books where they talk that way.

10-01:07:24

Burnett:

A long time ago, a mathematician friend of mine pointed me to—"Well, you want to learn math"—he was pointing me to math books from the sixties, and he said, "Don't read the more recent stuff, because it just it doesn't explain the fundamentals. You have to really understand," and what you seem to be saying is that that illustration that may have been meant to make it more accessible, or then it's like, let's say there's a doughnut rolling, or an elevator moving, it's like, oh, that seems like a real-world example, but in fact, it confuses the students, because they start thinking about it—

10-01:08:06

Leitmann:

Yeah, because you're not talking about that. You're talking about the center of mass. Then the question is, okay, now we're applying it to ships, right? Is that still okay? Well, it's not a matter of your taking liberties with deciding what is

small, because that's a very iffy thing. You're just talking about the wrong thing, and so they need to learn that very simple result which is in the first three or four hours of the subject. You prove that when you have a constant collection of particles; $F=ma$ for each particle holds, but it also holds for the acceleration of the center of mass, sum of all the external forces, and from then on, that's what you're talking about. Forget about particle motion, "particle" in that sense.

And so, the whole idea of putting emphasis on two things, the importance of fundamentals, and in a sense, the lack of, what really a fundamental is as we gather more information about the universe. Even the fundamentals are not rigid for all time, because this is not a religion we're teaching, where you have a dogma. That is, in fact, one of the main differences, and so you don't want to, even there, not allow changes over time. But, Newtonian mechanics isn't the end of the world, so then you can go to better—and again, it's in terms of the influence of observing the world that you make those changes and make more complicated theories, but even if you stick with one particular theory, you have to have that caveat, modulo that this is the theory that applies, and it's a warning you have to put out. And so, I think skepticism is one of the things I try to imbue the students with.

10-01:10:39

Burnett:

Well, so there's that, and so, not only do you have to master your material, and you often master it by teaching, by explaining it to someone else, you develop this profound understanding, but you also seem to suggest that you have to have an understanding of the confusion of the students. You have to understand what—

10-01:11:02

Leitmann:

Oh yeah, if you're teaching, of course. Takes two to dance, you know.
[laughter]

10-01:11:08

Burnett:

Well, but this is contrasted to some of the descriptions of the other faculty in those days, maybe even a bit before your time, where there was almost an insouciance, like, it's your job as a student to reach me where we are—

10-01:11:23

Leitmann:

Yeah, there's a hierarchical point of view which is stupid, because I learn just as much from my students and particularly my graduate students as the other way around, and this kind of symbiotic relationship, I think, to me, not only did I like it, but it was very important, because I learned a lot. Very often the graduate students know a lot more about new techniques, and so I can learn that from them, we can use it together, but you have to allow yourself to do that. You can't say, "I'm the end all and be all of knowledge," and some people can do it and some can't. It's a psychological problem, I guess.

10-01:12:11

Burnett:

And you have to be, so, you were flexible and excited to learn new domains. It's clear from looking at your list of publications that you range quite freely and widely, and you give credit, proper credit, to your students.

10-01:12:31

Leitmann:

Well it makes life more interesting. I think it's that simple.

10-01:12:35

Burnett:

Well, so, what were the expectations of the department with respect to teaching, because there are some interesting pieces of this. You served on doctoral committees in other countries, for example, and so, were there minimum requirements met, or was it more of an honor system of, you get the graduate students that you get, and you serve them as best you can?

10-01:13:06

Leitmann:

I don't know exactly what we are driving at, but certainly, the idea, particularly when you give examinations, to have other people come in ask questions, and I think classically, that existed and doesn't exist anymore. For example, even when I was a student here, the final examination for the doctorate was a public examination. In other words, they actually printed programs and posters and put them up and say, "From 2:00 to 4:00, Thursday afternoon, is Mister So and So's doctoral final examination. Feel free to come in and ask questions." Then, the committee, the thesis committee met afterwards and made the final pass or not pass decision, but anybody could come in, and this is why they put out a program and gave the basics about the person and what he's working on and that kind of thing. That's disappeared completely, but that was always classically the model.

10-01:14:20

Burnett:

Yeah, a public defense.

10-01:14:21

Leitmann:

A public defense, right, and I think it's a good idea. I think it's a good idea. Now, of course, the excuse is that, "We've got so many students, we just can't afford the time or the time of the faculty for this," and again, the system is becoming very complex, and something gives. I was lucky that I was still of the generation where we could do that, but it disappeared—same with language requirements. Even in engineering, you had to have two foreign languages. In the humanities, of course, some of that still persists, simply because it's part of the subject, but that disappeared very quickly. This classical view of the university has pretty much disappeared because it's become much more of a factory, I hate to say that, but—and I understand it, how this came about, because—and good things happen. So many more people go to university.

So there are plusses and minuses, but the minus of that is that economically, it's just not possible to do that, but I think that that means that you should pay more attention to how you teach and what you teach, and I'm just using this

particle vs. center-of-mass thing, because when you get to the motion of a rigid body, you take moments of the forces to determine the angular motion, a lot of special cases arise. The problem is made much easier if you take moments about a particular point, or the equations of motion are decoupled when you do the right thing, but that's not the basic part of the subject, and so the students have this huge amount of knowledge that they're supposed to be able to use and particularly pass final exams with, and it's frightening. My view always is, why not make it open book, if that's really the problem? Who cares where they get the knowledge from? It means they have to be able to get the knowledge. That's the hard part, not having the knowledge or writing the equation on your thumb, [laughs] which people used to do, because of this huge amount of specialized knowledge.

10-01:17:11

Burnett:

Right. But you're saying if their reasoning is good, if they have been trained to reason—

10-01:17:17

Leitmann:

And to learn—

10-01:17:18

Burnett:

—and to learn—

10-01:17:18

Leitmann:

—and how to pick it up, yeah, because if the basic knowledge is lacking, or the ability to use it is lacking, then I don't see how you're going to make real progress except sort of at the very tip of something, and it's important too, of course. It isn't a simple problem. I realize that this is a very difficult subject and aspect of teaching and learning, and I consider myself, again, very lucky that I really didn't run into that in my own career, because I got out of this era early enough. [laughter] But it bothers me because the answer I get is, in a sense, who cares? And I think that's the wrong attitude. I think it's very important if you're going to do good things, or avoid bad things. Maybe that's even more important. You better know just what, for example, you are assuming about something. The whole idea of, do you assume that you're dealing with continuous functions or differentiable functions? Very often that's left out. People just put a function down, and then the result is very dependent on whether that function has certain properties.

I'm just thinking of when I published my book in 1980 or '81, *The Calculus of Variations and Optimal Control, an Introduction*. It had some wonderful reviews, but one reviewer, I think he was a Canadian, said, "This is a great book, but it ought to be used with [Arthur E.] Bryson's book." He said, "because Leitmann is so pedantic about this and he's trying to be so precise, and maybe you want to use"—he didn't say humor — but maybe some lighter writing once in a while, and it was an interesting comment. My view was, I'm not writing a novel. I want to make the written part—in some of these books,

that's a very small part of the book—readable and informative. But it's a difficult compromise, how sloppy can you be to make it read better?

10-01:20:07

Burnett:

Well you're interested, it seems, as time goes on, and the body of knowledge increases, the need for specialization *early* in a student's education increases, and so, with the students left underprepared in terms of wrestling with the fundamentals and developing the intellectual muscles to interpret and analyze foundational assumptions that go into these formulas, for example, what you can end up with are these kind of black boxes of formulas, or assumptions—

10-01:20:51

Leitmann:

It's a cookbook.

10-01:20:52

Burnett:

—a cookbook, and you're compiling or you're assembling these black boxes, and then, to pass quickly to the frontier where now we've got the interesting advanced knowledge that we're going to talk about, and that's what you're going to learn as a student, but if they—you're saying that they don't know what's in those black boxes, or they don't know enough about them, or they take them for granted as truth.

10-01:21:19

Leitmann:

Yeah, and there is the aspect of students. Students very often will say, "Let's get out of this basic stuff and let's see some applications and techniques," and I understand that too, because it's not as exciting to have suddenly a result for a real problem or an imagined problem when you're still talking about what kind of functions are you using. It's a difficult compromise, and this is why I think it's important for serious students to learn how to discern what is really fundamental and then to go back later on and do some reading, or talk to somebody. I think the whole idea is that there are now so many people doing technical work, that I think the majority of them say, "Okay, I may not understand how we got here, but we'll just take it, because I have a problem to solve," or the company will say, "Let us worry about whether this is the right theory; you just go and grind it out." And so there are these competing views of what is an education and what it's used for, and it's not a simple problem, because there is always the economic question: how much can we afford to think, as opposed to do?

10-01:23:20

Burnett:

I suppose computing has changed things as well, because it makes—I have talked with other folks about actually crunching the numbers for a particular formula. You had to have a well-reasoned hunch that this particular way of doing it was going to yield fruit, because it was going to take you a month to run the numbers to make this thing work.

10-01:23:45

Leitmann:

Well, as soon as you go to a technique—for example, when I started we had analog computers. There you were dealing with the real world. You built

something, the type of equation that you're interested in, then you build the machine that, whose solution, motion or whatever, is really also governed by equations like that, and then you can say, "Well, in one case it's current. In the other case, it's traffic." That's not the point. The point is that the equations look the same, and so you can use an electronic model for all kinds of things for which the equations are devised: biological systems, whatever. I think there's less danger in that, but they're not good enough. They're not fast enough. They're not, and as soon as you go to a digital world, you're already faced with this business of deciding, how do you digitize a set of equations?

Manela, one of my students, the Israeli student who left right away to go back, he was looking at discrete systems—we talked about that a little—and one of the things he addressed was, you can do two things, say, in optimal control. You can start with a theory of the continuous systems, say continues in time, and you get the classical maximum principle, and then, when you actually apply it to a problem, you have to go to a computer, then you digitize it. So you take these continuous differential equations, ordinary differential equations, you make them difference equations, and there are many techniques for doing that. Or you can digitize the system, and go to the theoretical results, optimization theorems for a discrete system, and we talked about this man who unfortunately wrote a two-volume book on each type of system, and somehow got the same maximum principle, but it depends on a very strong assumption that he somehow missed, okay?

So you can go that way. You can say, "Okay, we have a theory for discrete systems, optimal control theory." So you don't have to digitize afterwards. The system is already digitized, so that step to go into the computer is already done because you started with a discrete system. Now how do these two ways compare, and he looked at, for some classes of systems, he looked at that problem. You can say, in a certain way, which one was better, and then it depends how you define "better," but anyway, it's an interesting problem, and you can't be stuck with just one thing. At least, you have to be open to the idea that something else will work better. So this whole idea that I've now explained the universe, we talked briefly about that. There are serious scientists who say, "Now we have arrived at a theory which describes the universe." It's my basic belief that we will never arrive at that, but, it's, in a sense, a warm feeling to have done that, but I think it's illusory.

10-01:27:46

Burnett:

No, I think I agree with you on that.

10-01:27:49

Leitmann:

And we talked about Stan Lucas. I was very happy. I don't know when it was, I won the ASME teaching award, they gave many of those, so I figured I was doing all right. I enjoy teaching, but I also enjoy many other things.

10-01:28:22

Burnett:

Yeah, although I think what we've been talking about before now, I mean, we were talking about teaching in the formal aspect of lecturing and doing office hours and answering questions and things like that, but throughout your career in dealing with graduate students and postdocs, and even, there's even some teaching back and forth with colleagues and peers—and one of the things that you bring to, when people bring you in as an associate author, to work on something that you feel that you don't really know much about, what you do bring or what you said you bring is that kind of skeptical eye. You're asking questions: How does this work?

10-01:29:12

Leitmann:

That's right, and sometimes you find in doing that that what was put down is wrong, because they couldn't answer the question, "How did you get from here to there?" and they found this very useful. People very often say, "It's good to have sort of a person who is not accepting of what I did just because it's me."

10-01:29:34

Burnett:

Yeah. And so that specialization and complexity is also a problem.

10-01:29:37

Leitmann:

Yeah, that increases, of course.

10-01:29:39

Burnett:

Absolutely. So, I want to begin to segue into something that's also very related to student life at Berkeley, and that is the students' lives at Berkeley outside of the classroom, and something that was—many things were happening on campus in the 1960s, and to start us on that path, can you talk a little bit about what you saw of the student experience on campus in the 1960s? Now of course, I'm also bringing this together with all of the travel that you were doing at this time. You were spending a lot of time working with others in other countries, and going to conferences, and as that is happening, there are tremendous things going on on campus, and what did you see of what was going on in the early sixties, and into—

10-01:30:49

Leitmann:

Yeah, well it was the Free Speech Movement, for example, and I think to this day, the experience—it was about 1964, I think, when the Senate met, and it was the largest meeting in terms of number of people that we've ever had. When we left Dwinelle Hall after that meeting and they found out outside what the Senate had decided, it was the first time we got rounds of applause from students. It was a wonderful experience that I'll never forget.

So, as you say, there is more to a student's life and I'll give you another example. In the seventies, I think this is something they, for some reason, gave up, they decided they want to emulate a little bit the British system, and they established something called Faculty Fellows. I don't know whether that's still in existence. The idea was that members of the faculty would come

in very close contact with students and student groups, or living groups, to the extent that, for example, they would every week eat a meal with the students, or they would invite students to their house. There was a period when I was the Faculty Fellow for Stern Hall, 200 ladies, and this is where my wife, who was wonderfully supportive of me in all this, became very important, because the idea was that at least once a quarter, we would invite the students to dinner at our house, this house, and the most we could accommodate were twenty students at a time, and that's what we did. We had twenty students at a time, every week, because if you look at the number of students, 200 students, and we were on the quarter system, that's ten weeks, right? So in ten weeks, we had to have 200 students, and she managed that very well.

10-01:33:22

Burnett:

Wow.

10-01:33:23

Leitmann:

Yes, really, we had a lot more dishes, of course, [laughs] and it was an interesting experience because it was very mysterious to some of the students just how to behave in that kind of proximity with a professor, and some were adept at this, they were terrific, and some, didn't know when to leave, and some of them, you would meet on campus the next morning and they didn't know you. It's very interesting. I was the Faculty Fellow for a couple of years for a group of anarchists, who had a living group on—what street was it—Durant, I think. Anyway, somewhere in that area.

10-01:34:19

Burnett:

Do you remember what they were called? Did they have a name for the group?

10-01:34:24

Leitmann:

I've really forgotten. All I remember is, that, they didn't know what to do with me, but they thought maybe, I could be useful in getting speakers. So that's really why they accepted me, and I invited Reverend Schwarz, who was an Australian. He headed the—what was it called? It was essentially a nationalist organization. He was a reverend, and I invited him as the speaker. I had them invite him as the speaker. It was very interesting, because for example, he was talking about Marxism, and he said, "They have this thing about 'conflict leads to progress' and this kind of thing was Marx's idea. He said, 'You know, these people, they make decisions by saying, 'Let's go two steps in this direction, one step to the right, one step back,' so that's the way they make decisions.'" I still remember that. They gave him a hard time. [laughter] So, that was a completely different kind of group. The girls in Stern Hall were very organized. They had one good thing. Every semester or quarter they roasted a pig—

10-01:36:13

Burnett:

Wow.

10-01:36:14

Leitmann: —and we got invited to that. It was before my vegetarian days, so I remember that as just a single instant, and they lived very close to here, obviously. Stern Hall is on Gayley Road, and Hearst. It's that corner up there—

10-01:36:32

Burnett: Right—

10-01:36:32

Leitmann: Then I was with a group of students, it was a fraternity, and they could eat a huge amount of—these guys would—I guess they played football, among other things—they could eat a three-pound steak. I've never seen anything like it before. That stuck in my mind, [laughter] and Nancy was terrific about this. She came along to all of those events with me. She's a tremendous help in every way, and it's an evolving system, and I'm not quite sure where it's going.

10-01:37:19

Burnett: Yeah. Well of course, it's become more expensive for the students—

10-01:37:23

Leitmann: Oh yes.

10-01:37:25

Burnett: —tremendously so.

10-01:37:26

Leitmann: When I was a graduate student here, and I only went here two semesters, because it took me four semesters, the minimum time to get my degree because I already had a master's degree and I knew what I was going to do, and I was on the GI Bill, so it didn't make any difference. When I came here, I was on a fully paid fellowship, so that didn't make any difference to me, but, I remember that it wasn't called tuition at the time it was called fees I think. It was about seventy-two dollars a semester. That was not buying books, but I mean just the fees that I paid the university, and I don't know how the state got away with the tuition cost now, because it was my impression that constitutionally, the major part, and this, almost everything, has to be paid by the state, and how they got down to 10 or 11 percent as it is now, I don't know how they—

10-01:38:33

Burnett: Yeah. Wow. So we are in a position now where there's lots of private money that has—and a lot of alumni have made up a lot of the difference.

10-01:38:48

Leitmann: Yeah. I think that's a singularly American system, of course. There's a little bit of that now even in France. They have a tuition now and the economic problem has made them do it, but in Germany, for example, higher education is still free. This has other aspects to it, because, for example, they had to change the regulations when they started getting, after the war, a huge

increase in particularly graduate students and if you had a student card, you only paid half fees for railroad tickets. Students stayed six, seven, eight years, just to have that card so they could live—

10-01:39:52

Burnett: On a discount.

10-01:39:53

Leitmann: Yeah, right. So they had to change the number of years you can stay a student, but at one time, there was no limit.

10-01:39:59

Burnett: Right. So there was a response to the demand or the increase of this Kerr's master plan for the expansion of the university, and you have thousands and thousands of undergraduates, and the universities had to expand very rapidly creating the UC system, of which Berkeley is a part. Originally it was just the University of California, just this one campus. And so they're adjusting to this change, but they're also—it's the Cold War—they're adjusting to the limits of the student experience. The students wanted to talk about politics. They wanted to talk about politics that was considered to be of the opposing side in the Cold War, and that created a number of tensions.

10-01:40:50

Leitmann: Well this whole idea of the politicization of the campus, I can just bring in now. There is a professor—she's now a professor—at Loughborough University in Great Britain, who is a lady—her name is Heike Jöns, who's German originally, and got her doctorate at Heidelberg University in geography, and by the way, it's the first time I suddenly had to think about, geography isn't just looking at hillsides. Geography is a big subject, social geography, human geography—and she's more into that aspect of it. She did her dissertation at Heidelberg supported by the Humboldt Foundation, and that's how she got to me, because her topic was what she calls the intellectual diaspora, people having to leave and some came back, and who came back, and why did they come back, and it was a very interesting thesis. She came to interview me—I think it was 1999—and we became very good friends.

So as she was writing her dissertation, and then working on a monograph to get this published. It was published finally as part of the proceedings of the Royal Society in Geography. She addressed that particular aspect, which has become really important already in the sixties, but then so much more with the Vietnam War, because it was the uproar I was involved in dealing with, no matter what the immediate cause of the trouble seemed to be. Basically, that really was the underlying cause or excuse or whatever. She deals with that in that paper which appeared four or five years ago in the proceedings of the Royal Geographical Society, which she very kindly dedicated to me. She's now working with two Danish professors on the history of the ombudsman idea, exemplified by me at Berkeley, and they just gave a paper at a geography conference at Loughborough, and they will reach—they're

working in a more basic way which will end up in a bigger piece of work. She was here three times last year because she was looking at the history of sabbaticals at British universities and at American universities, and how all this came about. She worked in the archives of the president's office a lot, because all this is in the old records of the senate. She did a long paper on that. These people work years on a single paper, not like in engineering, you know. [laughs]

10-01:44:40

Burnett: The humanities and social sciences have perhaps a lot to answer for, but—
[laughter]

10-01:44:44

Leitmann: When she was up for—becoming a professor is a big deal in Great Britain, because as a reader, you're already what we would call a professor.

10-01:44:55

Burnett: Yeah, it's like the full professor level.

10-01:44:57

Leitmann: That's right, and she just became a professor of geography there last year, and she sent me the documentation required to be even considered. Thirty-eight pages it was. Every minute of her career from when she got there first in 2002. They know the system, and these people keep minute records on everything they do.

10-01:45:28

Burnett: Yes, because that is how you are—and there's a thwack factor of throwing down the—the thicker it is, the heavier it is, the better, and I think that's part of the evaluation process.

10-01:45:42

Leitmann: Yeah, it's really interesting. I wasn't aware of that. That would have driven me crazy, but, I don't know which is a better system.

10-01:45:53

Burnett: Well, she's researching part of your career that has to do with the student unrest on campus in the—

10-01:46:01

Leitmann: Right—

10-01:46:01

Burnett: —1960s.

10-01:46:02

Leitmann: —and it's really in that context, of course, that she's doing that, because she feels we're again at such a stage.

10-01:46:10

Burnett: Interesting.

10-01:46:11

Leitmann: Yeah, so it'll be interesting to see and I'm looking forward to it. It will be a while, judging by the other one that took ten years to produce. [laughter]

10-01:46:22

Burnett: Well, so, what was going on that led to a kind of crisis? Can you set the scene? We may not get through all of it today, but set the scene in the late 1960s. I think you rightly point to the Vietnam War as being the prime generator of the tension surrounding student protest. There were other sort of proximate causes—

10-01:46:51

Leitmann: Well, there was—

10-01:46:51

Burnett: —more locally, but—

10-01:46:52

Leitmann: —of course, the minority representation both in students and in faculty, a political system. I didn't realize what a riot meant then and what a riot means now. Where I live, we have a UC wall, being UC-affiliated, and there is a wonderful picture during the Free Speech Movement with Mario Savio and those people. That was on Sproul Plaza, essentially a riot developing, and all the rioters wore suits and ties. You know?

10-01:47:31

Burnett: Very deliberately, too, actually.

10-01:47:33

Leitmann: Well, no, I think it was just a normal thing, and I think it points up the sort of—I don't know if there's a single word, but the difference. If you look at the few riots we had in the last couple of years on Sproul Plaza, the people weren't in suits and ties.

10-01:47:56

Burnett: No, no, definitely, and there is a whole identity, or multiple identities that are very associated with that.

10-01:48:03

Leitmann: I think the first time I ran into the violent aspect of this was during the Free Speech Movement when there was an occupation of Sproul Hall. The students came in and sat down in the various offices. The removal of the students, which was broadcast—there were no television cameras around then, but there were reporters there and they recorded and told what was happening. They were dragging the students down by their feet, down the concrete steps with their heads bouncing up and down. So that sort of basic violence is maybe part of the political process. I have a great fear that we're getting into this, particularly in terms of all the things that are happening between the base of one party and the base of another party, and what is involved. I'm very fearful, because I saw, I experienced this in Europe, where Hitler was elected,

legally in 1932, '33, and then, an emergency was declared, because the Reichstag burned down, and the constitution could be suspended, legally, for six months. Habeas corpus, all that was out the window, and so this whole idea of a national emergency, which is such a big thing right now looming, really worries me, really worries me.

10-01:49:47

Burnett: Timothy Snyder has been warning about this since the very beginning of the Trump election.

10-01:49:51

Leitmann: But in terms of what happened on campus with the Free Speech Movement, it had a very good ending, but then very quickly, within a couple of years, it went the other way.

10-01:50:08

Burnett: So things got resolved and there were new policies put in place.

10-01:50:14

Leitmann: Yeah, right, the whole Sproul Plaza, yeah.

10-01:50:17

Burnett: And so in '65, '66, things were quieter?

10-01:50:22

Leitmann: Yes, and there was also an academic aspect to that, because the whole idea of teaching evaluation, that whole idea that you don't look at how professors teach, they are there to teach and you don't have any right about this, all that has changed, of course.

10-01:50:44

Burnett: And that's—

10-01:50:44

Leitmann: It's all not always positive, of course, because what it leads to sometimes is that professors want to be popular, and say they do theater, rather than teaching, and we have some dressing up as Darwin or as Newton and teaching the class. There was a lot of that going on, as—

10-01:51:13

Burnett: Oh, even like at that time in response to the development of student evaluations, there was a sense that I need to be—

10-01:51:20

Leitmann: Yeah, you want to be popular. So, there are all kinds of aspects to this thing and they're very interesting to watch that.

10-01:51:30

Burnett: But all this to say that things took a turn for the worse as the Vietnam War grew worse, as criticism around student activism grew, so there is more

tension between the kind of the “establishment” and the “counterculture” in the late sixties that’s brewing, and that’s setting up the—

10-01:51:56

Leitmann:

Yeah, and there too is again a number of aspects to that, because there is the general public, very large portion at that time, who did not go to the university. It put those two, the people who are either students or faculty and what I call the town rather than the gown, became extremely adversarial because students could get exempted from the draft, much more readily than just ordinary people, okay, so that’s a very bad aspect of it. Also, of course, meant that if you had a lot of influence, you could get exempted, you could buy it, or you could buy the doctor who said you had flat feet, or whatever—

10-01:52:46

Burnett:

Or bone spurs.

10-01:52:47

Leitmann:

Let’s talk about the first thing that happened with riots after the Free Speech Movement, had to do, I guess, with the Third World Strike, which was ’68, I guess, or early ’69—

10-01:53:12

Burnett:

In ’69, early ’69.

10-01:53:13

Leitmann:

—early ’69, yeah, and then the push to let the pendulum swing the other way and start introducing things that really, in the long run, don’t make sense. I have a very—we talked about this I think a little bit before—I have a very strong notion that—let’s just be very specific, talk about getting more minority students, and that meant particularly African Americans, because there’s no problem with Asian students since they have the family pushing them, and unfortunately, Native Americans, they’re percentage-wise so small. It pushed the introduction of curricula which were in part, maybe a large part, tailored to the political aspirations rather than teaching a subject, like the history, for example, of slavery. Those were really good courses, but then there was a lot of much more wishy-washy kind of thing. Ethnic studies, I’m not quite sure what that involves. I’m sure sometimes it was very good, and sometimes it was really an easy course to pass and get a good grade in, and that’s not the problem. The problem is, what are you teaching in terms of these people improving their economic position? If you teach them to become an engineer, or even a mathematician, or go to medical school eventually, so you teach the basic sciences, that is the basis for increasing the ever, the increasing—

10-01:55:19

Burnett:

The earning power of that group.

10-01:55:19
Leitmann:

—the earning power of that population. But we've gone really the other way, because now, the two populations are coming apart. The people who could afford to go to a prime university is ever smaller, so there's a bad aspect to that. These things never come all positive and all negative in the other direction, so, but I ran into this when I was on the budget committee. If you looked from your point of view, at these courses, you said, "What are they teaching?" and that may be unfair. Maybe if I had been in that group, I would have said, "No, that's what I want to learn," and that's also good, but are you preparing them, in fact, economically, and I think of the two aspects to this, and so I think these have to be somehow brought into some kind of equilibrium, and that's very difficult.

10-01:56:32
Burnett:

So the Third World Liberation Front wanted a college of ethnic studies. There were negotiations around that, but basically, diversity was an issue, seeing themselves represented among the faculty, so there was poor faculty representation of African Americans across the board, so changing that up, but I guess there's also a notion of a curriculum that would be critical, and part of that criticism is viewing the university itself as being part of some kind of imperialist machine, right? [laughs]

10-01:57:19
Leitmann:

Well, it goes both ways. On the other hand, to the people on the right, the university is really a nest of reds. So it's the other thing. I mean, to one part of the population, they're the fascists, either because they're in power or they've turn out people who can make money, whatever. They could afford that. So there, it's not a single aspect and how you bring those into some kind of compatible system is very difficult, and very often, we have these pendular swinging. They either go too far one way, too far the other, and it's in everything. With sexual harassment, you get exactly the same thing. You just have to be accused now is enough, essentially, to lose your job, and in many cases, unfortunately, it's justified, but I'm sure it's not justified in all cases, so—

10-01:58:20
Burnett:

And it's the working through of a movement for change that is overall positive, but there are these casualties, as you might mention.

10-01:58:28
Leitmann:

Yeah, and these are somehow all revolutionary times.

10-01:58:34
Burnett:

Yeah, yeah, and things are coming to a head at the end of the sixties with the Third World Liberation Front and the Third World Strike, and the governor's office is putting pressure on the university to shut this stuff down, and so, that's the kind of context in which you find yourself, and so maybe we'll pick up next time to talk about your role as ombudsman to help—

- 10-01:59:05
Leitmann: Well that was as exciting as being in the Army.
- 10-01:59:09
Burnett: I bet it was.
- 10-01:59:09
Leitmann: No, I'm serious. [laughs]
- 10-01:59:11
Burnett: I bet it was. Well, what we should talk about next time is your life experience, faced with the conflict that you saw at the end of the sixties, and you'd seen a lot of conflict of various kinds, and so, I wonder, it must have had a particular impact on you.
- 10-01:59:32
Leitmann: I'll give you one example; it'll only take five minutes. We haven't mentioned the fact that in teaching, one thing that was involved was that I sometimes taught at another school, and this was my experience in Argentina when I got an OAS professorship—that's the Organization of American States—which means I got my full Berkeley pay for teaching in Argentina where my salary, of course, was tremendous, and I arrived the first day on the campus of the University of Buenos Aires in a riot. There was a riot going on.
- 10-02:00:10
Burnett: What year was that, do you remember?
- 10-02:00:11
Leitmann: 'Sixty-nine.
- 10-02:00:12
Burnett: 'Sixty-nine, oh, okay.
- 10-02:00:14
Leitmann: So I had just come out of the riot period here where I got involved in the violence personally, and I arrived here, and there the police came in on horseback swinging sabers, and I said, "This is not what I'm"—I went quickly back to where I lived, because—and it, very stark to see the differences. I had a wonderful group of graduate students. I was teaching a course in optimal control, and they had never encountered the professor who gave office hours. They were amazed, and they gave me a wonderful present of a book on their national hero, Martin Fiero—we'll talk about that next time—as a present, all, with calligraphy and writing from the students. So, that is an interesting and I think a worthwhile experience to see how systems differ in different countries, yeah.
- 10-02:01:21
Burnett: Absolutely.
- 10-02:01:23
Leitmann: Okay.

Interview 11: February 12, 2019

11-00:00:21

Burnett:

This is Paul Burnett interviewing George Leitmann for the University History Series, and it is February 12, 2019, and this is our eleventh session, and the last time we talked, you were introducing the story of unrest at UC Berkeley at the end of the 1960s, and a controversy surrounding demands for a college of ethnic studies and a resulting Third World Liberation Front strike, and there arose concerns around the extent of the unrest and some of the behavior of the police in dealing with the unrest, so I'm wondering if you could talk a little bit about that set of events. What really came to your attention when you learned about what was happening in terms of violence on campus?

11-00:01:33

Leitmann:

Well, first of all, the whole involvement of the ombudsman—by the way, a committee of one of the academic center, probably the only committee ever of one, and I was my own chair—that came about in the fall of 1968, and very quickly thereafter, as you mentioned, there was the unrest in connection with ethnic studies, and everything that revolves around it, and then the question was, given the initial charge to the ombudsman, there would really be no involvement by the ombudsman because I was supposed to deal with academic problems and complaints by students, and the Senate, in its wisdom, decided that being arrested, being victimized in a demonstration, or being involved in the unrest, if you want to put it that way, was in fact an academic problem because it would have an impact on the academic standing and life of a student, and so they decided that it would still be covered under the original charge, so that's how it got to me.

It became clear that this was going to be a more violent kind of confrontation than we had expected, and were ready for it, really. In 1961, '64, during the Free Speech Movement, there was some of that, when Sproul Hall, for example, was occupied by the students, the sheriffs came in and dragged people out by the feet. So that was the first one, and I did get involved in that only as a spectator. There was no filming of it, but there was really a definite and really very good look at that, particularly through public radio—they did a lot of it—and so I, in fact, at that time, made a tape of the radio program that described the scene. That was really sort of my first involvement, and then the faculty got extremely involved in the Free Speech Movement, and I think the largest Senate meeting they've ever had, including [up to] today, took place when they were considering the Free Speech Movement, what eventually led to the Sproul Plaza solution, in a way. It's still in my mind one of the most touching experiences of my long life at the university, because when we came out of that Senate meeting in Wheeler Hall, there were hundreds and hundreds of students standing around applauding us. It makes me cry. So, I was, in a sense, involved at least personally in all this. So, to my surprise, I got this appointment, and violence escalated very quickly during the Third World Strike.

11-00:05:08

Burnett: So now this is five years later, right?

11-00:05:10

Leitmann: This is five years later, yeah.

11-00:05:12

Burnett: Yeah, and it's a different campus climate at this point.

11-00:05:16

Leitmann: Absolutely, and just to bring in my own view, even at that time, and it became more obvious later, that the underlying cause, what you might call impetus given to unrest was really the war in Vietnam. Of course, there was the antiwar movement, and I got involved by occasionally going on a march. In fact, just as a side story, we mentioned Angelo Marzollo, who was a visiting scholar from Italy. It must have been 1964. We went on a peace march with him in San Francisco. So here was this visiting Italian scholar, and he really appreciated being involved in that, and—

11-00:06:13

Burnett: At some risk to him, right?

11-00:06:15

Leitmann: Yes, but it was, in terms of what we consider violence now, very peaceful. The only time when I personally got involved in violence was when Berkeley and Oakland were involved in peace marches, and so did the Hells Angels, and I remember that the Hells Angels were actually let through by the Oakland Police to attack the demonstrators or the people with signs and that's really my first, actual, personal view of people hitting each other.

11-00:06:54

Burnett: When was that, do you remember?

11-00:06:55

Leitmann: That must have been in maybe 1967, just before that period that we're now talking about, so I wasn't involved in earlier, what you might call demonstrations, but this was really my first exposure, when it got violent, the police, and by the way, interestingly enough, under the agreement among the local police forces—we have so many local ones, ranging from Berkeley, to Albany, to Richmond, to Oakland, to San Francisco, to Alameda, the sheriffs, the highway patrol. I think there were seventeen units, from—

11-00:07:46

Burnett: Departments

11-00:07:46

Leitmann: —different departments and agencies. They acted very often quite differently from each other, and particularly the campus police tried to stay very much out of it, because they knew that they had to live with us afterwards, so they were the least involved in physical action. So when tear gas was started to be

used and that kind of thing, I personally got involved because I tried to be as close as possible to the action to be able to say something if a complaint came about, and the first serious complaint I received was from a professor—I don't remember his name—who was giving a seminar in the main library on the second or third floor in one of the rooms, and suddenly, the blackboard in front of him got hit by bullets, actually embedded themselves in the blackboard, and he came to me the next day, and I was able to find out that a contingent of the San Francisco Police were chasing demonstrators from North Gate into the campus and were firing over their heads. Just crazy, absolutely crazy.

So I was very upset about this, and of course he was, because he was standing under this. I brought this to the police chief in San Francisco. He was very nice about it. He said, "Well, that was certainly my unit, but they didn't do this," and so I said, "Well, here's the proof." He says, "Well, I don't think so. They have strict orders not to use live ammunition." Since I got nowhere, I thought the only thing I can do is make it public, and so I wrote a letter to the editor of the *Chronicle*, the *San Francisco Chronicle*, just simply relating the story. Completely neutrally, I said, "This was brought to me and it was a contingent, by the police chief's own records, and something ought to be done," and I got nowhere—

11-00:10:21

Burnett:

With the *Chronicle*.

11-00:10:22

Leitmann:

—nowhere with the *Chronicle*. They said to me, "This is a new police chief, and we want to give him every chance to be successful, so, I'm sorry, we won't publish your letter to the editor." So the one vehicle that the ombudsman has is really to go public, and it was a failure. I got nowhere. I just wanted to give you a couple of failures, so there's a failure. Maybe we'll just keep it to one each, as my involvement got to the point where I set up an observer corps. I asked the Senate to put out a call for faculty, in particular, to volunteer as observers—we even had armbands. I think there may be one in the file that you have—and that had a calming effect, there's no question about it, and I'll give you an example of that.

This is already in somewhat later time, towards the end of this whole period, so it must have even been after the People's Park unrest—we'll say a little bit about that—and the connection to the Vietnam War became really obvious. There were demonstrations against the Naval ROTC, which is located in a building behind Spieker Plaza, on this little street that comes in from Oxford. It got extremely violent, and the police—it was Berkeley Police, city police, involved in the one I'm recalling—they became pretty, pretty bad. They had their batons out, and of course, they had face shields, and, they had taken off their badges, so there was no way to identify them. For example, I saw one incident where they had knocked somebody down and they were stomping on

his testicles, that kind of thing that was actually a more successful experience for me. I went to see the Berkeley Police chief about it, and he said, “Well, they have to take their badges off because the demonstrators grab them.” I had a very simple solution. I said, “Well how about painting the number of the individual—every one of the policemen has a number—on their helmets? They’re certainly not going to take their helmets off,” and he said, “That’s a wonderful idea.” And so, they did that, and there wasn’t a repeat of that kind of thing, as soon as people get identified.

About the same time, there was another incident. There had been a riot, and it was after the riot had calmed down, and campus police were patrolling the campus. It was behind Sproul Hall. I forget the name of the street right behind Sproul Hall, where they park the police cars.

11-00:13:46

Burnett:

Oh yeah, Barrow Lane?

11-00:13:49

Leitmann:

Maybe so. Anyway, it goes up behind the Economics Building. There was a complaint brought to me that a student said that he had been stopped—he was just walking there—by the patrol, maybe four campus policemen, and they got a little violent and in fact, his glasses came off, and they broke his glasses. They stepped on them on purpose. So I had brought that complaint to the Berkeley Campus Police chief, and he said, “Well, I’ll ask,” and of course, there was a uniform “this never happened” kind of thing, because people support each other. But fortuitously, there was a cameraman from one of the local stations who was still on a balcony, and he actually took photographs of this incident. How it got to him, I don’t remember, but it did. He came to me and he said, “This happened. I photographed it, and of course, you can have it.” So then, with that evidence, I went back to the campus police chief and he said, “Well that’s outrageous,” and so the officers involved were suspended for a month. Okay, so that was a success in a way. There were also sort of funny incidents. I was standing in front of Moses Hall while there was a demonstration in front of Moses Hall, and it was very heavily peopled by actually very young people, high school students and others. It got to be very exciting.

11-00:15:37

Burnett:

Oh, is this the—

11-00:15:38

Leitmann:

Yeah. And so I was standing there. I was clearly not of that crowd, and a kid, maybe twelve, thirteen years old came walking up to me and said, “Who are you?” and I said, “I’m an observer.” He smiled at me, picked up a rock and threw it through a window. He said, “Observe this.” [laughs] So, there were humorous aspects on—

11-00:16:05
Burnett: Well so there was violence, and I think in your final report you said, “apart from one or two incidents—

11-00:16:11
Leitmann: Incidents, on the whole—

11-00:16:12
Burnett: —on the whole,” it was and I think this is after your presence, after you and others—because you were not alone. You had—

11-00:16:22
Leitmann: Yes, observer—

11-00:16:22
Burnett: —other faculty, and this observer corps was there, and it was known, and the effect of, you were going to the chiefs—

11-00:16:29
Leitmann: I think it was a calming—

11-00:16:30
Burnett: —of police.

11-00:16:30
Leitmann: —effect. Yeah, I think so. I had very prominent observers, for example, Marian Diamond, who’s certainly a big—she was one of my observers, I remember, yeah. So—

11-00:16:44
Burnett: Well, so, there was, in the press reports of the time, I was reading about the use of rock salt in shotgun shells. There was a student who was shot.

11-00:16:59
Leitmann: Off a roof.

11-00:17:00
Burnett: Off a roof.

11-00:17:01
Leitmann: Yeah, that was on Bloody Thursday, May 15, and the date and the name stick in my mind because just around 11:30, twelve o’clock, Nancy and I were having a lunch at Larry Blake’s—it was a restaurant on Telegraph Avenue—and obviously a riot was in the making because it was the day after the university put up a fence around that area, People’s Park area, and people were trying to tear it down. And so the whole thing started, and they were chasing the rioters actually towards the campus, and they came through, and by that time, they were firing birdshot already, and then when they ran of birdshot, they started using buckshot. What to call them, rioters—I don’t know what a better name is—they were throwing things down on the police from the roofs, bottles, and so it got violent on both sides, and the fellow who

got eventually killed was one of the ones who was throwing things down, and they were firing up. By that time, they were firing up buckshot, of course.

11-00:18:27

Burnett: They were under attack, as they saw it.

11-00:18:29

Leitmann: So, I immediately sent Nancy home, because having a Welsh temperament, [laughs] later on when the National Guard came in, she used to march through to get to her car, to the swimming pool. She wasn't going to take any of this crap, and so I immediately sent her home. By the way, just as a side comment, that period, say from the spring of '69 through the summer, when I took my semester in Argentina, there were actually weeks when she didn't see me for three, four days at a time. I just didn't leave the campus, and in a way, it was exciting because I thought I was back in the Army. [laughs]

11-00:19:24

Burnett: I was going to ask you about that—

11-00:19:25

Leitmann: Yeah, a very, very similar experience.

11-00:19:28

Burnett: This is the first violence—

11-00:19:31

Leitmann: Yeah, since that time—

11-00:19:32

Burnett: —since that time.

11-00:19:33

Leitmann: —so, in a way, it all sort of came back to me. I didn't analyze it because there was too much going on. So I was standing then at the corner of Bancroft and Telegraph actually on the campus side, next to a—I think it was the *L.A. Times* reporter, who was looking at this whole scene going on in front of us on Telegraph Avenue, and he turned to me and said, "You know, your arm is bloody," apparently, I had been hit by birdshot. It's like a scratch. It's really not a wound, but he suggested that I have it looked at, and I went up to—what was the student clinic [Cowell Hall]—

11-00:20:31

Burnett: It wasn't—

11-00:20:33

Leitmann: They wouldn't do anything for me. They said, "You are not a student, can't do any"—had to go to Alta Bates, and it was nothing. It was really only a scratch, so it was absolutely nothing. It was sort of a little experience of the day. So in the evening, I got in touch with five area hospitals. I identified myself, and I inquired. I think at Alta Bates, I was already there so I asked them in person,

but the other four, I just called, and I said, “You can verify that I’m bona fide, call the university police.” The governor had come on, Reagan at the time, on TV and said that “the police was being slaughtered,” the phrase he used, “in Berkeley,” and he would call in the National Guard, declare a state of emergency, and that’s when I really wanted to find out how true this was. I remember the numbers, approximately. I think there were five policemen who had been treated at those hospitals, all released—

11-00:22:06

Burnett: Within the day.

11-00:22:07

Leitmann: —within the day, and about fifty or sixty demonstrators, most of them being shot in the back running away, or wounded anyway, running away, and I don’t know how many were shot, wounded anyway, and some of them had to be treated more seriously for that reason. So that was the counterpoint to the slaughter of the police. It was something that I think became important politically later on, the way he was a man of action, that he took action.

11-00:22:42

Burnett: Without question.

11-00:22:42

Leitmann: Yeah, so, next morning, the National Guard arrived in Berkeley. They bivouacked down at the marina. They had armored cars, no tanks but armored cars, and they stretched barbed wire across intersections to block people and vehicles, and then they had actually, on Telegraph Avenue—because the bank was one of the first targets, where they had their windows broken, the Bank of America on that corner, Durant and Telegraph—they had National Guards with fixed bayonets standing at—it was off and along Telegraph Avenue, so it began to look very grim, and I had again a humorous experience. We had visitors, two, I think, two or three, from the Soviet Union staying at the Durant Hotel, and they were looking out the window and there was an armored car with its cannon pointing at the Durant Hotel, and they asked, “Ah, what’s happening?” and in my inimical, stupid way, I said, “Well you had the October Revolution; now we’re having ours.” [laughter]

11-00:23:57

Burnett: Oh, God! Did they laugh?

11-00:24:00

Leitmann: No, they didn’t know whether I was serious or not, you know, but—

11-00:24:04

Burnett: Right. On my goodness.

11-00:24:05

Leitmann: Well, then there was a response to all this. You may remember, a few days later, there was a call to demonstrate. People were invited from all over the United States to come to Berkeley in a big demonstration against this whole

military action that was going on in Berkeley, and I was really worried. So on the day of that march, which I think was later in May, actually, the preparations were going on, and I was invited to see what preparations they had made for this. The National Guard was still here, so the Berkeley Police took me in a squad car to all the places where they had barriers, so that I would know, because they knew I had this observer corps, when trouble was expected, and what really worried me was that they were dragging machine guns onto the roofs with boxes of ammunition, and I thought, it's going to be a slaughter, and it turned out to be a very happy day. Twenty-thousand people came, maybe more.

11-00:25:22

Burnett: Twenty-five.

11-00:25:23

Leitmann: Twenty-five. I remember there were huge crowds. It was the day when, very often, the ladies used to expose their bodies to sort of amuse the National Guard. [laughter]

11-00:25:37

Burnett: Or protest—

11-00:25:38

Leitmann: It was a whole—

11-00:25:38

Burnett: —or something, yeah.

11-00:25:38

Leitmann: —a whole happy experience. They put flowers in their rifles. There wasn't one instance that I knew of where there was anything bad going on during that day.

11-00:25:48

Burnett: Right. Didn't work so well in Kent State, I suppose, just a—

11-00:25:51

Leitmann: No, that was, of course—

11-00:25:53

Burnett: —a year later.

11-00:25:54

Leitmann: Yeah, right, that was. So, the upshot was that something good could be done with that kind of operation like the ombudsman, because having observers is very important. People on both sides calm down a little bit; they don't want to look bad. For example, my student, François Litt, who came from Belgium, he was at that time a very devout Catholic; he suddenly became a revolutionary. He used to go to class—there was almost no interruption, by the way, of school during that whole period—and then he'd run away and man the

barricades, and then run back to the next lecture, and he became a very liberal person. Suddenly, all his conservatism left him, and to this day, very often, because we correspond so often, we recall those days, yeah, that's '69. That was about a year after he came to me as a graduate student.

So there were all these sort of personal things, of course. Another one, which I think is very telling, and I don't remember who the chancellor was at the time, but there had been a big riot right on campus, and they had used tear gas, and those canisters get very hot, and so grass and bushes had started to burn on campus and all that kind of thing. I walked across campus with him; he was crying. He said, "I don't understand what this is all about." He had no clue, really, of what was behind all this and why this gets so violent. Really, he was destroyed because the campus had really suffered very badly from this, and the broken glass from windows, and all that. No, he was baffled. He was truly baffled by this. We can find out who the chancellor was [Roger Heyns, 1965-71]. He was a very nice man, not one that I was very close to—but he was, truly he was hurt. It was a personal kind of insult to the university and to him, that this kind of thing should happen.

11-00:28:29

Burnett:

Well it's the beginning, maybe not the beginning because the Free Speech Movement was something different, but I think because of the actions of Ronald Reagan, this is the beginning of a turning point in the national reputation of UC Berkeley. It becomes a symbol of a perceived out-of-control, violent radicalism, and it's a narrative that has been kept alive and revived in recent years, and deployed strategically to—

11-00:29:02

Leitmann:

And there was recent violence, of course, on both sides.

11-00:29:07

Burnett:

Yeah, although I would suggest that the narrative is more powerful and more important. As you pointed out, Reagan's exaggeration of the quote/quote "slaughtering of police," similar kinds of hyperbole were on display.

11-00:29:25

Leitmann:

Well, they are used to other things now, for example, what's happening at the border and that kind of thing, and it's something which speaks to people who are really worried about their own position in society. Religious, the things come into it, and here it was really new, and by the way, if anybody who is listening to me some day wants to get a better notion of what was going on, YouTube has quite a few videos on this whole period. You can see the helicopter flying over the campus dropping tear gas and pepper spray, and that kind of thing. Used to upset Nancy very much because she'd be swimming, and suddenly, the tear gas would drift across the swimming pool and they had to close it.

Interestingly enough, at that time, usually nothing happened until sort of mid-morning, until everybody got up, and the police came to the campus. So for example, very often a riot would start when the police were trying to pick somebody up for whom there was a warrant out. The campus police stayed away from that, but the other police forces, they had a list every day with sometimes photographs of the people and so on, from the previous day or few days before, there was a warrant out, and they tried to go and arrest the guy, and that started another riot that might last, again, all day until the night, and I think they've learned a little bit about not doing this now. I think that the tactics are quite different, but at that time, that was one of the major causes for starting a riot. It's just almost automatic, but there'd be resistance, and people trying to interject themselves between the police and the—it was, and you see a lot of that on these videos. They're very interesting, yeah. Hope they don't—

11-00:31:41

Burnett:

Scrub them?

11-00:31:41

Leitmann:

—stop showing them, yeah. If you just type in “Berkeley riots, 1968 through '70” or something, you get all this. So, the one that I alluded to before, talked about, against the Naval ROTC, that was clearly a Vietnam protest already. That was in early 1970, and being the ROTC and all that clearly had nothing to do with People's Park or anything like that. So it was, yeah, so it was—

11-00:32:17

Burnett:

And once Vietnamization was underway, and the draft ends shortly after that, the level of protest really goes down quite dramatically—

11-00:32:29

Leitmann:

That's right, absolutely.

11-00:32:30

Burnett:

—but, I don't think we should underestimate the effect of early forms of calming and de-escalation, and you can see the ombudsman as a kind of de-escalation where, as you say, the very fact of observation produces less behavior outside of—

11-00:32:58

Leitmann:

Yeah, on all sides.

11-00:32:59

Burnett:

—norms.

11-00:32:59

Leitmann:

Well, now that the ombudsman office has been discontinued now for about twenty years, almost, my feeling, and again, even the complete non-remembrance of that period by the administration really amazes me, because this was clearly a very important part of the university history, and Heike

Jöns, who coauthored this recent paper with two Danish professors, sent a copy of her abstract of the paper to the chancellor, and she [the Chancellor] said, “Thank you very much,” and that was the only response she got, which is again, interesting. [Narrator: There was in fact a small commemoration in 2018.]

11-00:33:57

Burnett:

Well, there’s more remembrance of the Free Speech Movement, which has a kind of happy ending, I suppose, but I think there’s something darker going on at the end of the sixties.

11-00:34:08

Leitmann:

Yeah, I don’t have an explanation for it, but it really surprised me that—it was the fiftieth anniversary this year of the ombudsman, well, last year, because it was ’68 through today, and that was exactly fifty years, and there’s no mention of that anywhere, which I found really surprising.

11-00:34:33

Burnett:

And it is now fifty years since the Third World Liberation Front strike, so, that’s how we are marking it today. [laughs]

11-00:34:41

Leitmann:

Yeah, right. I thought it would be interesting to you as a historian that, what to me seemed an important historical event, is completely forgotten.

11-00:34:56

Burnett:

Well, defining the contours of history is as much what is not included in archives as what is, and that’s important to recognize.

11-00:35:07

Leitmann:

Better left out, kind of [laughter] left out.

11-00:35:09

Burnett:

Yes, from those who don’t want it to be there. So, turning then from that to other experiences, you were on the budget committee and you served on the budget committee, and when was that, roughly?

11-00:35:24

Leitmann:

That was ’79 and ’80, and there was one major assignment I had, and it was extremely interesting, because it made me more aware of the quality of the faculty in non-engineering and science areas, and it was a happy experience from that point of view. It was extremely labor intensive. It’s a half-time appointment for two years, and every weekend I would spend in the administrative—it’s the one, the chancellor’s office wing of the administration in that building, where the files were, which were under a triple security system. You had to have a password, and you had all that stuff to get into there—those files are kept very, very close to the chest—and as a member of the budget committee, I was able to see everybody’s file except my own, of course. So it was an interesting experience there.

I think, at that time, there were eight members of the budget committee, and so they tried sprinkling all the different disciplines into the budget committee. I was from the professional schools in science or engineering. There were people from the humanities, of course, people from the pure sciences. I know there was somebody from chemistry at the time, and there was somebody from the English department, I remember that, and it was an interesting experience because, well, seven of the members, not the chair, were assigned different segments of the faculty, and what happened is that the proposal either for promotion or for hire would come eventually to the chancellor, who would immediately send it to the budget committee for comment. The budget committee, of course, doesn't make decisions, but is advisory to the chancellor, and by the way, as just an apropos, we handled in one year almost 1,000 different items, and of no—

11-00:38:01

Burnett:

And what's the range of items?

11-00:38:02

Leitmann:

The range is, of course, hiring new faculty, and then promotions from rank to rank, and in Berkeley and UCLA, even salary increases. I think the other campuses, they're handled more locally. That may have changed since. I don't know what the system is right now, but at that time, and there were a lot of cases. You're talking about a faculty, what, about 1,500 people? So right there, if you look at when people are promoted or will get salary increase, some of them, that makes a thousand cases easily, and there were a few other items considered, and I think at least one year, I remember only two cases were overruled by the chancellor from our recommendation, and he was more severe than we were.

So that was very interesting in itself, and I guess I mentioned to you the other day, the little episode where a member of a humanities faculty was surprised that the engineers got doctorates, because he thought they build bridges and repair cars and that kind of thing, and so the whole question, what is research, came up, and I had mentioned to you that, maybe semi-facetiously, I said, "Well, you know, in the humanities, particularly, and maybe even the social sciences, they do not do research, but scholarship," and I said, "It's undoubtedly incorrectly believed by some people that the difference is that, in the sciences and the science-based disciplines, research meant coming up with new basic ideas, whereas in the scholarship part of this picture, the idea was to go to the library and read what other people have said about a particular subject, and sort of put it together from a new point of view, but," I said, "I'm sure that's not true," and it isn't, of course, and the other aspect of it was that there were certainly disciplines, subjects, where we had no cause, or no grounds for making decisions, other than go to the experts, for example, in literature, the critics; same thing in particularly the graphic arts, because there would be tremendous non-competence to do that. You know, the people like modern art, they hate modern art, just—

11-00:40:41

Burnett:

The criteria were inaccessible or nonexistent.

11-00:40:45

Leitmann:

Well, the real criteria should be by people who are the experts, and so we always insist that we should really look at what the reviewers of a play, for example, or a book, say and not on how we feel about it, because I had this example: We had somebody who had written a book, and he came up for quite a high promotion. I don't know whether it was the Step Ten—there were only nine steps or eight steps then—or maybe a special salary. Anyway, there really had to be a good look at that person, and so we were all urged to read a recent book he had written. We all did that, and I was very puzzled, because as I looked at it more closely as I read it, it didn't sound right, and what he had done is change the gender of the subject he was talking about from one mention to the next, so he was he and then she, and then he was he and then she, and so I wasn't fond of that, but I wasn't the critic, so I stayed strictly out of that.

So, it had that aspect, but as I say, if I looked at the overall impression I got of the campus, faculty, particularly, I was really impressed, because generally, people in different disciplines don't look at the quality in other disciplines, and so this was one way of doing that, because every case, the cases were presented by the person who was in charge of that area of the faculty. So it was, for me, engineering, paleontology—I don't know why—and physical education, because I swam, I guess, and then the case would be presented, with all the recommendations, and in those days, it came from the department, to the dean, and from the dean to the chancellor, and then to us. There you saw exactly how the thing worked, because the different steps in the process sometimes had completely different views of an action. For example the dean may not at all agree with the department in respect to a recommendation. And very often, this came up.

When I chaired the privilege and tenure committee, this came up, in fact, and I was able to introduce changes in the rules. I felt that this was outrageous, and what should happen is that every recommendation, in fact, everything in the file should go from step to step, and that's now the rule, and furthermore, the individual is invited to make his or her own comments at the beginning, which I thought was very important particularly in the humanities where there are often bitter rivalries between different people, and if the reviewers of the committee would not know that, this could be pointed out by the individual. They would go by what was said about this person, and it was very bad. So that, again, is something that I was able to introduce as legislation.

11-00:44:23

Burnett:

So you instigated a small, due-process revolution at that time.

11-00:44:27

Leitmann:

Yeah, and it seemed very obvious, of course, but until that time, and I was on that committee maybe a year or two later. So, it's one of the things I remember with a certain amount of pleasure, because it's not very often that you can do something positive. It's a very complicated process.

11-00:44:53

Burnett:

Well there are those who suggest that the existence of the budget committee and this expansive notion that all Berkeley faculty are peers of one another, and that that has consequences according to some, that this accounts for the quality of the hiring—

11-00:45:13

Leitmann:

Well, that has been my view, and this still is my view. Now, particularly, administrators such as deans feel that the process is too complicated, it takes too long, and there's a constant push to somehow lower the impact of the budget committee, and I don't know what it is right now, but I knew that that was going on. Even when I got into the administration, I noticed there were big complaints: it takes them forever, because, particularly for the higher ranks, you have to go to two references from the outside, and that sometimes takes time. They have a budget and they want to go and use the budget, , so it's constantly a process, not all agreeing whether the budget committee is so important.

11-00:46:08

Burnett:

Right, right. But it is something; it's fairly unique. I mean, it is at UCLA, you said—

11-00:46:17

Leitmann:

It's all the campuses.

11-00:46:18

Burnett:

All the UC campuses.

11-00:46:20

Leitmann:

Yeah, this is a system. This is a system.

11-00:46:22

Burnett:

So was it part of the Kerr reorganization?

11-00:46:25

Leitmann:

I don't know where it came from frankly.

11-00:46:27

Burnett:

That's interesting.

11-00:46:27

Leitmann:

But I know that this is a system, and I think the budget committee came in in the early twenties, so it precedes Kerr, I think. I'm pretty sure that I remember that—

11-00:46:39

Burnett:

So it begins at Berkeley and then it's exported to other—

11-00:46:41

Leitmann:

Yeah, well this was the mother campus, but it was something I'm very glad I was able to participate in, particularly in view of what I learned about other faculty, yeah.

11-00:47:00

Burnett:

Well, for your sins, you were recognized as someone who could assist in the leadership of the College of Engineering, and—

11-00:47:10

Leitmann:

I suppose; I don't know whether that had anything—I never know what leads to what, you know, because all that sort of sub rosa kind of thing. [laughter]

11-00:47:20

Burnett:

I'm discovering through interviews that there's a real service ethic in the College of Engineering. You talked about in, let's say, other unnamed universities in other domains, such in the humanities for example, that being a department chair is a real chore, it's something that is going to take away from your research, and at the college, there's a sense that it's a ship that has to be steered, and it's a duty and an honor to do that, and I'm sure that the attitudes towards it vary, but, that's the story that I get from people.

11-00:48:02

Leitmann:

I think so, and certainly in my happy case, I always wonder how I got praised so often in some way or another. That certainly had something to do with it. Karl Pister became dean of engineering I think in 1980, and one of the first appointments he made was that he appointed me associate dean for graduate study and research. Those are usually four-year appointments.

So that was my first appointment, and it was very nice, first of all, because in addition to having a little bit of influence, I got to meet a lot of people, and I think I mentioned to you once that that's how I, for example, met Evelies Mayer. She came here with a delegation from The Technical University of Darmstadt. She was a professor in the social sciences there. She came with the president, Helmut Böhme—I got to know him very well afterwards—and a couple of other administrators, to study graduate study in the United States, and so they visited quite a few campuses, but obviously Berkeley was one they visited, and so that's the first time I met her, and it led to her becoming my sister, so to speak, eventually. I got invited to a lot of places I wouldn't have. For example, I was invited to the University of Utah. They came up and invited me once in the eighties, I think, to be on a committee of their graduate studies program. That happened to quite a few places. I found out more, and I loved traveling anyway, so that was sort of a side effect, and—

11-00:50:12

Burnett:

And you had done that for a long time, too. Just as a faculty member—

11-00:50:17
Leitmann:

Yeah, but not—

11-00:50:17
Burnett:

—you had been invited to review other programs, and—

11-00:50:20
Leitmann:

Yeah, but this really gave it another push, and now that they could say that they were on this committee, “we had an associate dean from Berkeley.” There is that aspect to it. It even got me to the University of Hawaii once. So that was the first four-year period. 1980. That time was a very interesting, happy year for me. I got elected to the National Academy of Engineering that year, and I received the Humboldt Award, Alexander von Humboldt. It was at that time called US Senior Scientist Award, because it was only Americans who were at that time eligible for Humboldt Awards. They had Humboldt Fellowships and they had Humboldt Awards.

I actually postponed that award because the first couple of years, I wanted to be really full-time available for my associate deanship, then once I got used to it, I took off two summers and in one case, a sabbatical semester, and went to Darmstadt where my Humboldt Award was in '83 and '84. So those were administrative appointments. My second one, I think—I'm just looking at the list. '81 through '85, those four years, and then in '86, I was appointed to a second four-year associate deanship called associate dean for academic affairs.

11-00:52:27
Burnett:

Yeah. Did you go back to back, or did you have a year off, between those two?

11-00:52:32
Leitmann:

No, this was back to back. In later ones, I think there was some intermitting, yes, right, and in '88, I was the acting dean of engineering because Karl Pister went on sabbatical, so that was during that period, '86 to '90, academic affairs. Usually, when cases come from the department to the dean, the dean, him or herself, now, will look at each case. Karl was really too busy for that, and so the decision was made that I would handle all promotion and hiring recommendations from the departments, unless there was anything that I thought he should know about, and then I would confer with him. And so I would write the recommendation from the college to the chancellor. So I would draft that and write it, and of course, if I had any doubts about it, I would show Karl what I had written, and generally he was perfectly in agreement. So that was an interesting thing to do, and again, it made me more aware of what was going on in the other departments in the college, which I thought was also a good thing.

11-00:54:04
Burnett:

Well I wanted to ask, you had these associate deanships and what we'll talk a little bit about, you had a third round, associate dean of research services, in

the college, and so this is a span from 1981 to 1994, and my question is, viewing from that vantage point, can you talk a little bit about how the college was changing, from '81 to '94, research priorities? Maybe there's, the deans of the college have an impact, like they put their stamp on the college, in terms of their leadership. Do you have any reflections on those changes?

11-00:54:48

Leitmann:

They were fairly slow, so I'm not really sure that I can think of any quantum jumps. They were certainly very heavily colored by who the dean was, and I don't want to name particular people, but they ranged from truly outstanding, to very good, to so-so, let's put it that way. I went through probably four or five deans in that whole period. That extends into my post-retirement period because I had another associate deanship after I retired, a four-year one.

11-00:55:37

Burnett:

Well is there a change in the relative size of—I don't know if that's something that you were able to observe, but changes in the relative size? Does electrical engineering become more important because of the high-tech boom, or is there—

11-00:55:54

Leitmann:

Well, first of all, electrical engineering did not have computer science earlier. It was in a number of other departments in addition to engineering, and I don't recall exactly when the decision was made that computer science would reside entirely in engineering. So people who wanted to be involved in computer science had to be either in engineering or have a joint appointment, that kind of thing, and it certainly became, for obvious reasons, just looking at development of technology, again, I don't want to make a judgment, but certainly the most popular department, maybe drawing some of the very best students for that reason, and certainly the largest department, because it was split into two divisions: electrical engineering and the more traditional power engineering, and computer science; from a prestige point of view, and given the rise of computer science in general, became the drawing point.

In fact, it's very interesting, and again, this came up with my connections particularly with German universities—I don't know the exact date, but it was in the first part, I think, of the 1980s—they were trying very hard, really pushed by the computer industry in Germany, to establish something sort of like the computer science departments at MIT, Stanford, Berkeley, and of few of the German technical universities so that they would have their own MIT and such, and I think they went through seventeen—the number sticks in my mind—seventeen of the technical universities and got turned down cold. There was just no interest. We're talking about the early 1980s, very surprising, and that's what led to the International Institute for Computer Science. Located off campus, I think on Milvia. Its constitution states that the head of ICSI is always a professor in computer science. It became strictly a German-US operation for a while, but is now international.

11-00:58:34

Burnett:

So you're saying that the Germans went to seventeen different technical universities in the US, to—

11-00:58:40

Leitmann:

No, no, in Germany, technical universities in Germany, and couldn't get anybody really very interested, so that's when they turned to the United States and talked, I guess, to people at MIT, at Berkeley, and so on; finally asked Berkeley to set up this independent kind of university-affiliated institute—it's called the ICSI [International Computer Science Institute]—and that, again, added, of course, a great deal of prestige to electrical engineering in particular, and to Berkeley in general, when it became truly international, I don't know how many universities worldwide have people here. They get visitors; they have grants and everything. I haven't looked at that institute for quite a few years, but I know at that time, it was a real coup to get that into Berkeley. Stanford, of course, was always very strong in that because of their proximity to Silicon Valley. Clearly, there's a money aspect to that too, always. There's no reason why it wouldn't be.

11-01:00:16

Burnett:

Clearly, the United States emerges as a leader in science and technology education, after World War II. Europe is decimated, and a lot of faculty have left and populated American universities, and then several generations later, you have the current situation in the eighties and nineties, and they're looking to American universities. Was there a kind of science-diplomacy aspect to what you were doing, in all those trips that you were taking over the decades?

11-01:00:59

Leitmann:

Well, yes, I think again, it may not have been terribly conscious, but simply to sort of represent this culture, Berkeley, it wasn't my specialty, so I wouldn't participate necessarily in the technical aspects, but simply from the point of view of the emphasis that was placed here, and again, in a certain respect, the German universities, particularly technical universities, have a very, very unique—there's no such thing, but no, an Oxford dictionary now allows it—new aspect, and this is more the apprenticeship kind of aspect that we don't have. So in German technical universities, and I had a lot of contact with them and still do, the emphasis is generally on degrees, doctoral dissertations which have a very applied aspect to it, and the support comes primarily from industry, so that the doctoral student is fully supported for three years, once he or she starts on the research, which is, of course, very important, and we don't have that.

I think the downside is that, to explore things that don't have a very high probability of success—and many of the big things come from taking a chance, and failing, which is also an important aspect of research—if you show that this can't be done that way, I think is, in itself, important, that's sort of lacking, and you see that, for example, in the absence of Nobel Laureates for Germans in the sciences—it's very rare that there's an engineer involved.

A rare example is MRIs, where medical doctors, engineers, and electrical engineers were the ones who really did that.

Now, this is changing in Germany and it's changing, but certainly, when I was involved, in particularly the eighties with the Humboldt award and then later with postdocs from Germany, they were very good, extremely well trained in the basic subjects, but their interest was much less in working on something that had, say, a theoretical aspect to it. There was always a great emphasis on the eventual aim of the thesis being a contribution to—when I was in at the Technical University at Darmstadt, I served on a doctoral committee for one of the students. It was when I was doing things with uncertain systems. I was beginning to do that, and so the application there was to deal with the effect of vibrations and other perturbations for high-speed turbines, you see, so sort of the basic underlying reason for doing this kind of research is, in the end, that there's something to which it's applicable, and that, very often, was not the case here, but maybe more so in computer science than in anything else, because there was the speed and the accuracy, and so there was a very definite aim, and then the machinery that was used very often was designed for that kind of thing.

And so, there is, in a sense, an amalgamation worldwide because we're no longer really the only player—India, for example, becoming very important in computer science, probably China now. So I think that the whole picture is changing, and—

11-01:05:31

Burnett:

Well the United States just won back the fastest computer status after five years in a row being held by China. So— [laughs]

11-01:05:45

Leitmann:

And if you look at their super computer—I read an article about that—it's absolutely incredible. So, the world is changing and I think maybe the American empire is beginning to crumble, in many ways, and this may be one of the signs.

11-01:06:01

Burnett:

Maybe. Well, one of the things that's changing in the eighties and nineties is at least an apprehension about the state of the public research university in California and elsewhere, the declining proportion of state funding for the operating costs of the university, and the possibility and eventuality of private research funds being expended, the Novartis deal in 2000, and before that, the concern about biotech and the biotech revolution and what that would do. According to some, this is less of a concern for the College of Engineering because there's a long tradition of private industry support for research in the college. Is that something that you were talking about with folks during that time, right around the time when there's a budget crisis in the early nineties for Cal? Is that something that you were talking about?

11-01:07:10

Leitmann:

Well, there has been maybe an increasing push to turn UC and maybe particularly the main campuses, at Berkeley, UCLA, maybe San Diego, into a private university in the system. I was on the—I still am, for some reason—on the executive committee of the college, and very often the discussions turned to, how do we convince the president's office that there is a difference among campuses, and that those who are sort of the research campuses really should have a different fee structure, really not be sort of bound by—again, I don't want to pick particular campuses, but by some of the restrictions, and I think that push is going on, because you can make a case for that, of course. The only thing is, it's counter to the constitution of the State of California, so you would have to involve the legislature, of course, but that may be possible to do. I think that kind of push is certainly not over, so that there will be sort of two tiers of UC campuses, and you had that in Germany, for example, about ten years ago. The federal government introduced a program that would support what they called "centers of excellence" and then each of the particularly technical universities would vie for being in those circles, so there is the Technical University of Munich which is sort of the MIT of Germany, but then that kind of internal competition grew there, and I think that that's one vehicle that people envision, even within the UC system.

11-01:09:35

Burnett:

So, in the UC system, it is looking abroad at other models. They're looking at other ways of doing things.

11-01:09:44

Leitmann:

Yeah, I don't know whether they get the impetus from that, or simply because it's a natural kind of thing. The administrators have a different view of the world, from say, just ordinary faculty, and they're faced of course with having a budget and all that—I understand that—so it has happy and unhappy consequences.

11-01:10:11

Burnett:

Well, before we go into talking about the associate dean for research services, in 1991, you retire technically.

11-01:10:22

Leitmann:

Yeah, I was emerited...

11-01:10:23

Burnett:

Yeah, you were emerited in 1991, and was that under the voluntary early retirement—but you weren't early, at that point. You had been thirty-six, thirty-seven years.

11-01:10:33

Leitmann:

Yeah, well, relatively so. There was a personal aspect to this, as well as just maybe another one. I had come down with colon cancer in '89, and very happily discovered early enough so that I survived to this day, [laughs] which is a long time ago. So what's that? Thirty years ago. So I was a little tired. I

got out of the hospital and I guess it must have been, and I had two or three postdocs, and my accepting the next final associate deanship was one would be a full-time appointment. It was Dean [Richard] Newton's idea, that it was an important enough aspect of what he considered important in the college, and so suddenly, I was sort of freed from doing anything such as, well, teaching, in particular—research, of course, was an aspect of the general picture—and it was, in fact, almost an irresistible thing to take, for somebody say in his mid-sixties. We had people who tried to do this in their mid-fifties, and naturally some of them did. We had Nobel Laureates who left under VERIP and then obviously got fantastic jobs, all research jobs at another university just so they would have a Nobel Laureate, and they would—

11-01:12:32
Burnett:

Right, which was controversial, right?

11-01:12:33
Leitmann:

—draw two full salaries for the high ones, and under VERIP you could retire. Stay in the retirement system provided your remuneration was 49 percent or less, so you could, in a sense, draw another half's maximum salary, and stay emerited under that program, and it got better—that was a three-year program—it got better in that sense, every year to the extent that in year three, the chancellor said that he would resign if Berkeley gets such good terms in the third year, and Berkeley will get all the good terms the other campuses get, the chancellor said, "I'll just resign," since he might lose his best faculty members.

11-01:13:39
Burnett:

That was Chang-Lin Tien?

11-01:13:40
Leitmann:

Yeah, Tien. He was a fantastic chancellor, and he was a colleague in the department. He was a man who was single minded in terms of doing the job, doing it well, and working really hard. He would clear his desk every night before he left work; everything that was pending, and wasn't really long range, he did. He'd walk around the campus and pick up paper, and throw it in the can—my own experience with that was when I retired from the research services, the staff, and it was a very large staff the whole west wing of the first floor in Hearst Mining were offices for that operation, staff gave me a party, and he was the first person to show up, and that was universal. First of all, he enjoyed doing it, of course, but he was always not trying to get out of that kind of thing, and we've had other chancellors at that level who try to get out of any distraction, you know?

11-01:15:11
Burnett:

Yeah, well, it's a very demanding job and he was clearly an extraordinary individual who would—

11-01:15:16

Leitmann:

He really was and it's a tragic thing. They were neighbors. They lived on the next block, corner of Avenida on the next block. They had that house up on the hill, and I remember Di-Hwa, his wife, sometimes came walking by in the morning, and we would talk. His stroke was terrible. He was in a coma for three years—it was awful—and he knew it, but he couldn't communicate.

11-01:15:49

Burnett:

So he was shut in, in that—

11-01:15:51

Leitmann:

Well, yeah—

11-01:15:51

Burnett:

—syndrome.

11-01:15:51

Leitmann:

—he was just lying there. It was just a bad operation he got at Stanford, actually, and no, it was terrible. It was really terrible. Anyway, so, what I really sort of wanted to say was that the influence of a chancellor is very big, and he was one of the really outstanding ones. I cannot make more definitive observations about some of the other chancellors. I knew Mike Heyman quite well. I knew him well, because our son was going to run for president of the student body, and ran into real politics. He formed his own party, and became vice president, and once in awhile, particularly at the beginning of my term, when I met Mike Heyman, he said to me, "Oh, you must be the father of Joe Leitmann. I know him very well." He knew him better than me! [laughter] So, my son also had a bigger office, by the way, than I did.

11-01:17:24

Burnett:

A trend which I believe continued, probably. [laughter]

11-01:17:28

Leitmann:

So, there are so many aspects to—and it's only in retrospect now that I sort of have to think about it, because before then, I never took time to really analyze what was going on. So it's really interesting to note how important the chancellors are.

11-01:17:47

Burnett:

Oh, absolutely, and Mike Heyman did a wonderful job of fundraising during the 1980s, and put [UC Berkeley] on a fundraising track.

11-01:17:55

Leitmann:

Right, so the influence of administrators. Some of them did a good job, but they sort of just keep the ship afloat. The just-retired dean of engineering, for example, is a sort of an outstanding example of being deeply involved in the operations. Some of them were not quite as deeply involved.

11-01:18:26

Burnett:

You're speaking of Dean [Shankar] Sastry [2007-17]?

11-01:18:28
Leitmann:

Yes.

11-01:18:31
Burnett:

So, along those lines, we are talking about your service here, and just to round this out, can you describe briefly what research services covered, when you were an associate dean there from 1990 to '94?

11-01:18:47
Leitmann:

Research services covered first of the budgetary considerations for all the operations, institutes, and special programs, and in our case. It's now a campus operation the Richmond Field Station. There are many labs out there, It's a hundred acres on the Bay, and there were constant plans for developing it—this came up recently—and it always failed. For example, while I was the associate dean and thereby also in charge of the Richmond Field Station, I think there were two studies by private contractors to see what could be done. They both came up with pretty much the same idea, a research park kind of thing, but it cost a lot of money to do that. The chancellors changed during that period, and one chancellor had it under consideration as a site for a global university. The only thing where I got personally involved in that kind of development was that, I was able to attract the EPA, which was looking for a site for the Western United States EPA lab. This would, be a lab for testing and such, and we attracted them to the Richmond Field Station. They leased three acres, and there's another consideration here. When it came to picking the contractor who would do this—this was going to be land that a private contractor would develop for the EPA—it turned out to be Rich Robbins, whom I knew at that time, so I had to recuse myself.

11-01:21:10
Burnett:

You recused yourself from the process?

11-01:21:11
Leitmann:

I recused myself from who was picked. He had a lot of experience in the whole Bay Area, particularly this region, and there was a big, big to-do when that opened, when that was built, the lab. There was a big opening. The chancellor, Tien, came but a year later, I got a lot of flak for doing that. They said, "We need that land, and you" a twenty- or thirty-year operation—

11-01:21:49
Burnett:

Lease.

11-01:21:49
Leitmann:

—lease—"you lost us that land." They were really pissed at me.

11-01:21:54
Burnett:

Who?

11-01:21:55
Leitmann:

Some people in the administration in the planning office there. They made such a big deal of it when they got it, and actually, there's no loss at all,

because the place has still got plenty of undeveloped land there, so that's not—

11-01:22:11

Burnett: So it was three acres out of a hundred?

11-01:22:14

Leitmann: Yes, and it was at the northern end where there was no interference with the other labs but I got a lot of flak for that, yeah, but, it didn't make me unhappy because I was out of the whole thing by then. So, I don't really care, but it's interesting how these things develop, and then it depends pretty much on who the chancellor is at that time. I don't remember and I don't even want to think about it, but it had to do with who the chancellor was.

11-01:22:44

Burnett: So retrospectively, they were angry that this had happened, right?

11-01:22:48

Leitmann: That's right. It was after I stepped down, by the way, I still—and the other humorous aspects of having that job was, I met with either the director, who was Frank Giunta at the time. He had been a senior person in my office, and he became the director of the Richmond Field Station—I met with the administrators of the various labs at least once a week out there, and so I had my own parking place behind Hearst Mining, and it was the only one, except for one other category. It said, "Reserved for Dean Leitmann." You know, there are lots of designations such as for Nobel Laureates, but not a particular one. The Field Medal recipients were named, on their parking signs, so that's my distinction of having a named parking spot, [laughs] because I had to commute back and forth. There was a bus service, of course, and I guess there still is, but I had to be there at a particular time, like 8:00 in the morning. Anyway, so that was something that came to me. [laughter]

11-01:24:11

Burnett: Well, it might be a good time to talk about awards of a particular kind, but maybe first, I would like you to briefly tell me about the Alexander von Humboldt Association, and your involvement with that. That's in the—

11-01:24:31

Leitmann: Well, as I said, I had—

11-01:24:32

Burnett: It's in the late nineties, isn't it?

11-01:24:33

Leitmann: —I had a U.S. Senior Scientist Award; it's now called Alexander Humboldt Research Award. So I came into very close contact, of course, with the foundation. They have been terrific to me, and they gave me a number of awards. I got the Heisenberg Medal, and Alexander von Humboldt Medal. The then-secretary general, Heinrich Pfeiffer, was, in fact, the re-founding secretary general of the foundation, because it had fallen into disuse twice. It

was founded in, I think, 1862 and it was meant to be the main vehicle for contact between German and other countries' scientists. During the First World War, it died. It wasn't till sometime during the Weimar Republic that it was re-founded, and then it fell into complete disuse during the Nazi period, '33 through '45. It wasn't reconstituted, until Germany became independent which was the mid-fifties, '56, I think—

11-01:26:15

Burnett:

Yeah, '55, '56.

11-01:26:16

Leitmann:

—yeah, '55, '56, and he [Heinrich Pfeifer] was a very strong personality. He was secretary general for thirty-six years, just died two years ago. We became very close. We became good friends, and he believed that I, coming from a country like the United States, might become useful, because by that time, the Humboldt Foundation had given awards, prizes, fellowships, whatever, to over a hundred countries, and the main recipient of their largesse was the United States, because only U.S. citizens research award. Then when it was internationalized, we still got the major portion of it, so they were a little miffed, I guess, that we never had an alumni association unlike most other countries.

11-01:27:29

They had a liaison office in Washington, and the foundation, the secretary general—there's a president too, but again, the president is not as important as the secretary general, sort of like the chancellor, the vice chancellor at the British university kind of thing—called a meeting. It was the fall of 1994, yeah. They called a meeting at the University of, I think it was Wyoming and invited former prizewinners as well some fellows who had become very important to this meeting. They had maybe fifty or sixty people they invited to discuss the establishment of some kind of, I'll call it alumni association—that's not really what they had in mind, but—in the United States. There was a long discussion. It was a three-day meeting, I was there for two days over the weekend, and they took a vote, and generally, the vote of the people there was definitely to establish such a thing. But I had to leave on Sunday because I was going to Europe with Nancy on, I forget now what it was, either Monday or Tuesday, but when I got back—it must have been Sunday night—there was email saying, "You've been chosen as the founding president." They said, "You know, obviously you have to accept this, but that was the vote."

11-01:29:24

I thought about it quite a bit, because it was certainly attractive, in a way, and by that time, of course, I was retired. I'd also finished the term of the associate deanship, and so it became attractive and I accepted. The terms were going to be, I think, one-year terms. They changed that immediately to two-year terms, because they couldn't get people to volunteer to do this. [laughter] My vice president was Klaus Beyenbach, a professor at Cornell, originally from Germany. He was in the medical school as professor of physiology. He then

succeeded me as president after the second year. It was wonderful from many points of view. First of all, it again involved a lot of travel, so, all over the place, because when a particular organization of that type, alumni or other, wanted to invite somebody, they wanted to invite the United States president or former president of the Humboldt Association. It was called the AVHAA, the Alexander von Humboldt—no, the American Alexander von Humboldt Association, the American Alexander von Humboldt Association.

11-01:31:20

Burnett:

Of America, maybe that's the second *A*.

11-01:31:22

Leitmann:

I forget now what the written title was, but that's what it stood for, with offices in Washington, D.C., and for those first few years when I was involved, the foundation was really supporting this fully, all the travel we did and the staff we had in Washington. The offices were in the building that the NRC used, right on the canal in Georgetown. It was a wonderful operation. So that's how I got into it. I'm not involved now, I haven't been for a number of years but with the second AVH secretary general, Manfred Osten, I've been in touch constantly, and the third one was Georg Schütte. He was quite young, an outstanding guy, later he became the head of the Fulbright operation in Germany, and about three or four years ago, he became what we call deputy minister of education. In Germany they have something called *Staatssekretär*, and he's usually the operational—it's a very much again, like chancellor and executive vice chancellor, so he really runs the ministry. The minister is a political person, of course. We've stayed in touch until a couple of years ago, then he became very busy. I still get birthday cards, but it's not a really close relationship. It's been terrific, and they've been involved in everything. When I had my eightieth birthday celebration, there were three secretary generals, those three that I just mentioned present.

11-01:33:45

Burnett:

Well, we should just say that the George Leitmann birthdays are something to behold. You've had a sixty-fifth celebration, an eightieth celebration, and—

11-01:33:57

Leitmann:

My seventieth actually was a big one, a really big one, and again, the then-two secretary generals came all the way from Germany, Pfeiffer and Osten. The consul general of Germany came, and a month or a year later, I got the Knight's Commander Cross of the Order of Merit of Germany, stuff like that, which is in a way sort of funny. I'm a Knight of this and a Knight of that, and they used to kid me in the dean's office here. They used to say, "Good night to the good knight." [laughter] The older I become, the more I realize that these things are nice, wonderful, but they're not really important. They're just nice, and sometimes I wonder just how this all came about.

11-01:35:03

Burnett:

Well there's one award I would like to ask you about in a moment, but I thought this would be a good segue to talking about what's important, and I'd like to ask you now to reflect a little bit on the role of Nancy in your career and in your life. Can you talk a little bit about her?

11-01:35:23

Leitmann:

[Narrator Addendum: Perhaps it is best to tell you how it all began. It was early 1954 when I went to a performance of Noel Coward's "Blythe Spirit (Blithe Spirit)" at a theater, primarily a movie one but filled with a small stage. It was there I saw, indeed experienced Nancy, who played the second wife in his play, which was presented by the China Lake Players, a local amateur theater company. I found out later that Nancy always and still is entranced by the theater and acting. Some years later, already married to her and back in Berkeley, we joined the Drama Section Club at UC to which we belonged for more than fifty years and in which Nancy acted in many of the plays put on by the Club. She also belonged to the Italian and French Section Clubs.

To get back to the story, seeing her only from afar on stage was enough to arouse my interest. Fortunately, soon we met in person in a UC Extension Russian Class and I found out that she was a "permanent" houseguest of the Bothwell family whom she knew from a days in Cambridge, Mass. Frank Bothwell was then Technical Director of the USNOTS of China Lake [mentioned prominently in the book "Magnificent Mavericks" a history of the USNOTS, China Lake, by Elizabeth Babcock, published by the Naval Historical Center in 2008]. Of course I knew Frank and I soon became a frequent guest at the Bothwell home. This led to our engagement and marriage in January 1955, and the beginning of 64 years of happiness, and still going on.]

She is really a rock I stand on, because our personalities are 180 degrees out of phase. I'm a worrier, not a warrior, but a worrier, and basically pessimistic, given to moods of pessimism, and she's exactly the opposite. She is very pragmatic. For example, I worry about what am I going to do for our kids after I'm dead, and in a sense, she thinks this is not only ridiculous, but it's almost funny. She says, "What are you going to do? You're not there." It's that simple to her, and to me, my worry is that I won't be there to worry, and she says, "Once I'm gone, I'm gone," and she's an optimist, and, well, she's a treasure, let's put it that way, and we very often talk about how different we are. We realize that, and we very early we decided a number of things and never talked about them again. For example, I'm a Jew; she's a Protestant. Well, it turns out I'm an agnostic Jew, and she's an atheist Protestant, so that was no problem at all. I signed, in my mind, an agreement, which has the short title, "I Make the Money and She Spends It." [laughter]

Isn't it funny? It's not true of course. She has a master's degree in Romance languages. In the early sixties, when she realized, here I was, a professor and she doesn't have a PhD, so she was going to get a PhD. Her main interest, as I mentioned before, was the theater, drama. So she was going to get a PhD in the English department, essentially in drama. It didn't work at all because first of all, we had two small children. She was very involved in being my hostess, which was important both, well, it was pleasurable, obviously greatly preferable, but it was just important in terms of dealing with a lot of visitors that we had at that time, like Pontryagin and people like that, and she just couldn't do it. She called me one day in the office, and she said, "I hit a man with the car." I said, "Don't say another word. I'll be right there." [Narrator Addendum: Fortunately, it was only a minor fender-bender, but caused by Nancy who was truly exhausted from raising a family and her many commitments, many for UC. Clearly, she did more than I already, and we decided she would forego the doctorate, a decision neither one of us ever regretted].

11-01:39:15

Burnett:

So, a major role of Nancy in life was as this support and this kind of counterpoint to your personality, and her tremendous support over the decades in doing all sorts of work, the social work around welcoming faculty and colleagues, and the students at Cal that she fed for years and she was primarily instrumental in raising your son and your daughter, so I'm wondering if we could talk a little bit about Elaine and Josef.

11-01:39:52

Leitmann:

Sure. Our first child was born while I was at China Lake, in the Mojave Desert.

11-01:40:04

Burnett:

In '57, is that right?

11-01:40:06

Leitmann:

He was born in '57, and I had returned there in '56. I got my degree in '56, and I returned, actually in '55 already, to China Lake, and spent the second year of my graduate study there, finishing my dissertation with their better computer, and then Josef was born in April of 1957, right, so essentially, two years after we were married. He was always an interesting kid. Just to give you one example: We lived in El Cerrito at the time, and one night—no, that was already here, so it was '61, so he was four years old. We built this house in the fall of '61. It was finished in, I guess, November, '61, then he and Elaine had separate bedrooms up here on this level and we had a separate bedroom wing for ourselves. We could keep that door closed, but one night, I heard him come down the steps. I am a very light sleeper, and I didn't say anything. He walked up to my bed and he went, "*Psst, psst*, Dad, *psst*." Finally, I—oh, he knocked on my forehead, that's what he did. He finally knocked on my forehead, and I said, "What do you want?" He says, "What

comes after ninety-nine?” and I said, “A hundred,” and he turned around and went back to sleep.

He was always that kind of kid, [laughs] and he was a very good student at all levels. He had a double major at Cal in development studies and economics, then got his degree here; went to Harvard to the Kennedy School for a master’s of public policy—a two-year program—as a Kennedy Fellow. Actually, Princeton wanted him. They really wanted him, and he should have gone to Princeton. I think so, but anyway, so he got the MPP, then he volunteered for the Peace Corps immediately after that. While he was a student at Berkeley, he had been an intern at the Peace Corps Foundation in—well, the Peace Corps operation in Washington, D.C., and he was very instrumental in setting up the Peace Corps Foundation, actually. He got very involved in that, and one of the things he learned were the codes for the various Peace Corps operations in different countries, and he found out that there was going to be one in one of the African countries, and they had sent one of the princesses—it was a kingdom—one of the princesses to meet him while he was an intern, and that’s why he was going to go there.

11-01:43:36

Then the money fell through, and he found out there was going to be another one in an attractive place, the Cook Islands, so he was the first Peace Corps head for the operation in the Cook Islands. That got us to, right away, to one of the many places that his subsequent jobs got us to. We had great visits to Rarotonga, the capital, and so that was great, and then he got involved with the World Bank. He got an internship there, and then in the late eighties, he joined the World Bank. I think he just finished his thirtieth year, or he’s just in his thirtieth year now, and that was just the kind of job he wanted. It was interesting. He had always been interested in doing good things, and then the World Bank thought he ought to get a PhD, so they gave him a leave of absence. He came to Berkeley for his PhD in city and regional planning with Professor Webber.

11-01:45:11

He did very much what I did. He spent one year here on campus and then finished it at the World Bank, second year. His dissertation grew into a book called *Sustaining Cities*, which is still considered a breakthrough text, because his theme was that you don’t have grand theories in general, but every place has its own problems and you ought to have themes, and operations, and tools for that particular—he became interested in megacities, very large cities, and so, he’s been very involved really all along. He had five-year appointments, first in Turkey, and then in Brazil five years, and then five years in Indonesia, so that got him to about 2010.

So about that time, he got recalled to headquarters and where he has been ever since, in Washington, D.C., and he’s a lead specialist. In his case, it’s disaster and risk analysis and operations. So, for example, he was in charge of the

reconstruction funding for Indonesia after the tsunami, then he was the head of the international reconstruction fund for Haiti for four years, and I have a wonderful picture—I don't have it here—of his interaction with Bill Clinton, because Bill Clinton and the president of Haiti, at any given time, were the co-chairs of his advisory committee. At first he didn't get along with Bill Clinton, because one of the reasons they picked the World Bank to operate that reconstruction fund is because it had the best oversight. The IMF, for example, is very political, and it gives grants to small places, big places. The World Bank only gives funds to countries, as does the federal government, and Bill Clinton thought it was an insult to the president of Haiti to be asked to give a monthly report of how he spent this money. That's not what they do in the Clinton Foundation, and Josef said to him, "Mister President," he said, "That's it. You have the Clinton Foundation; I have the Haiti Foundation," and in the picture of him with Clinton, he has his arm around Josef. They're in deep conversation, and I asked Josef right at the beginning, "Why does he have his hand around your shoulder?" He says, "He couldn't reach my balls." [laughter]

11-01:48:28

So, they got along very well after that, and so that's what he's doing now. He's sort of contemplating retirement—he's sixty-one—from the World Bank, now it's over thirty years, and they have changed their operations. It used to be that their positions were tenure positions, but about ten years ago, they went to the system in which everybody's a consultant for three years, so they get out of paying all these extra emoluments. The only people who are temporary anyway are the president and the vice president. Josef gets along very well with most of them. One of them, in particular, was an Australian billionaire [James Wolfensohn] Josef was in charge of the Amazon project. The World Bank one time underwrote a project which wrecked much of the Amazon Region because it led to widespread deforestation. With Josef's project the World Bank aimed to reverse the terrible effects of the earlier one. This was the reason for Josef's taking Wolfensohn on an extended trip to the Amazon. They got along very well as seen on a photo, clearly after some substantial imbibing of the local brew. [laughter]

11-01:49:56

Burnett: Now you're telling tales out of school.

[The section here below has been expanded and redacted by the narrator]

11-01:49:58

Leitmann: Yeah, but remember that Josef's interesting overseas assignments allowed us visits to Turkey and Brazil five times. For Indonesia two visits sufficed. I have now told you a lot. So let me focus on our daughter Elaine. In many ways her personality is very different from Josef's who travels a lot (to Brazil, Indonesia, and Portugal in the last few months and India, Switzerland, Lebanon and Portugal again, in the next couple of months). She has one or two good friends at a time; he has 300 all over the world. Her happiest time

was riding her Percheron in Berkeley hills and doing equitation competition with Fjorling winning many blue ribbons. The judges were astounded seeing a Percheron doing equitation and jumping. After Fjorling died, she got 'Tank,' Kataaka Kiuki, an Appaloosa whom she rode in the Napa hills near our house below Mount George. To underline that we are an ecumenical family, Elaine became an Episcopalian, attracted to St. Marks in Berkeley by the music and its social programs, e.g., she prepares hot meals every month for about 200 needy people. She is also in charge of the church gift project during holiday season. After graduating from Berkeley High (before then she had been in private schools, Bentley, I believe) she started in the pre-vet program at UC Davis with the animal science major. After graduation she worked for 6 months as a veterinary assistant, but she just could take being so close to suffering animals and quit veterinary medicine. She was always attracted to gardening, especially food plants. Her first job was head gardener in complex in Yountville.

We had in the interim bought a place in Napa, which was near the Silverado and Napa country clubs. So when she and Bob Parker, both students at UC Davis, he in oenology and viticulture, got married and upon graduation they moved into a wing of our Napa house. This lasted for 13 years when they separated. He was in charge of the tasting room of the Inglenook Winery. Elaine, a vegetarian and teetotaler, had primarily the love of animals in common with him, but it was an amicable separation. To this day, they travel together and spend Christmas holiday at a dog-friendly hotel in Carmel.

11-01:56:05

Burnett:

Well there's a common denominator of travel. I think there's a travel theme throughout the family. She is also artistic, no?

11-01:56:25

Leitmann:

Yes, she is artistic. But she decided to become a math teacher probably due to my advice, which I now regret. She always loved linguistics but I said, "Great, but I wonder how you are going to make a living as a linguist. But you also like math." So she decided to return to school at age 39, and spend four more years to get a degree in mathematics. Then she started teaching in the Vallejo district. It was a terrible experience, for in place of teaching math, she spent most of the time trying to control the class. It was so bad in this district that the district had an arrangement with a psychological clinic in Sacramento for the teachers with nervous breakdowns.

11-01:57:38

Burnett:

Wow.

11-01:57:39

Leitmann:

So she quit, and being very interested in Celtic culture and art, accepted an appointment in a school in a castle near Cambridge in England. The students were worse than those in Vallejo. For example, when she asked students to put away their cell phones, they gave her the finger. So she quit and being

motivated by a desire to “do good”, she took a job in high school near Nogales in Arizona. This, too, did not work out, not so much for discipline problems, but except for a few students, most of the students just did not care for mathematics. So, she decided to retire and return home to Berkeley “to take care of the old folks” which she has done splendidly since 2008 until last year when we decided to live at a UC-affiliated senior place but 15 minutes from our family home. Now, Elaine and her beloved dog visit us daily. She is also tutoring math and is a true neighborhood “factotum.”

Both she and Josef are exercise fanatics by osmosis [from their parents’ habits]. She was a marathon and long-distance runner, Josef is a squash player and cyclist. We have three grandchildren. JR, [Joseph Rafael] the oldest, arrived in the mid-seventies, due to an indiscretion, when Josef was 17, with the visiting granddaughter of a girl friend of mine in the late 1940’s, who was with the Guatemalan embassy. I seem to recall that I mentioned this earlier. Anyway, Josef adopted JR and everything has been great since then. JR went to SF State University and upon graduation worked for a financial firm in SF. He is now acting director of a D.C. foundation to educate and support minorities, primarily Hispanic and African American, in financial and business affairs. His wife Lourdes, we call her Lulu, is an administrator in the Arlington County government. They gave us two great-grandchildren ages seven and three. Josef and wife Reiko have two children, Alessandra “Sasha,” a Reed College graduate and a documentary filmmaker, and Nicolas, a physics student at Washington University in Seattle.

[End portion redacted by narrator]

11-01:59:56

Burnett:

On the theme of ecumenical families, you have spoken about how there are kind-of honorary family members, or adopted family members, a number of the students that you’ve had, and you see your students in general as family.

11-02:00:17

Leitmann:

Yeah, extended family. For example, Eduard Reithmeier, I already mentioned him. He was a German Humboldt Fellow and we’ve become very close. He got married here in Las Vegas while he was a postdoc with me. That’s about twenty-three or twenty-four years ago. He was with me in the early nineties, and then came back for their twentieth wedding anniversary with Renate, his wife, and their two children. I got him a suite at the Faculty Club as a wedding anniversary present. They came here for a week, and it was wonderful. I hear from him all the time. We’re still in very close contact. In fact, he was going to provide this project with a history of his experiences with me, but it turns out, in the typical German fashion, he has a file which is probably—I don’t know how big, and he wanted to put it all together. I said, “Do it, and we’ll just put it in the archive, that’s fine, yeah.”

11-02:01:38

Burnett:

And there are others too. I think you've spoken about Evelies Mayer, and she—

11-02:01:44

Leitmann:

Yeah, she and Dusan Stepanovic, are my two really adopted family. Evelies Mayer was a sociology professor, head of the department, at Darmstadt. She's a Socialist, belongs to the Frankfurt School of sociology, and there are representatives in San Diego, some of the people probably dead now, who came from the Frankfurt School.

11-02:02:13

Burnett:

Well I think Herbert Marcuse was in this area, wasn't he? Yeah.

11-02:02:16

Leitmann:

Well yes, and then there was [Leo] Lowenthal in Berkeley, who was very important. Evelies came here and did his library, which was a major project, and she came here two or three times since then for long periods of time. She stayed in this house for three months once. It was during my second Humboldt term, when she stayed here, and so, we've become brother and sister. We call each other that, and we correspond at least once or twice a week, at least.

Dusan is another interesting person. I guess it was a meeting in Los Angeles where I gave a paper, and he came up and introduced himself. At that time, he had just got his PhD at the University of Santa Clara, from a very prestigious man, a man from Yugoslavia originally whose name is Siljak, Drago Siljak, who worked in large scale systems, very, very good work, and Dusan was his student, and then postdoc-ed with the wife of our Dean, who was a professor at Stanford at the time. During that period, he found out my family history. We talked about it, and he said he was going to make it his life's job to find out what really happened to my father. He was able to do that. I knew where my father had been when the Germans took over Yugoslavia, and so it was very clear from, even the Red Cross, that he could not have survived, because all the Jews in that town were killed, the town of Niš, which became important when we had our war with Serbia in the nineties. We bombed the hell out of it—and he's influential. He comes from a very prominent Serbian family. His father was the city architect of Belgrade, still alive, and he has an older brother.

11-02:04:51

And he did, he was able to do that. He had enough contacts. Particularly, when he found out that my father's last address was Niš, he spoke to the people at the National Museum there and they were able to find a monograph which they had underwritten in the seventies, which essentially deals, [clears throat] excuse me, with the fate of the Jews of Niš, and in particular, the concentration camp the Germans called Red Cross, which was right in the center of a town, one of the only places where they had established it because there was a police compound they could use. And so he translated that

monograph. I helped him with the English a little bit—his English is very good, but just—and it turns out that that monograph contains also the records that were found in the concentration camp when the Partisans liberated it in '44, and there is an appendix listing Jews from Niš, Jews who came to Niš from Yugoslavia and what they called foreign Jews, and number fifty-one on that list is my father. So that established his fate officially. There was a breakout from that camp the year after the Germans took over. My father, who had served there in the First World War spoke the language fluently and knew the area, I hope he participated in that, and if he got killed, at least he got killed that way, because the remaining Jews who had not participated, were all shot in one week, with dum dum bullets to their heads.

11-02:06:54

So, I can't guarantee what really happened to my father, but at least I know where he ended, and so, Dusan and I became very close. He had an aunt here, a much older aunt, who lived in Orinda, whom he came to see every month from Illinois where he's a professor. Now he has bought a condominium in Walnut Creek, and he's going to do that continuing with us. He was here two weeks ago. He came back from Europe last month; he has a research project there. So, I gained two family members. I have a few others who are close to family members, but not that close.

11-02:07:48

Burnett:

And so, getting some—the word “closure” gets overused, I think—

11-02:07:55

Leitmann:

Yeah, well—

11-02:07:55

Burnett:

You learned something, let's say, and in more recent years too, you have had some additional recognition, I suppose, and I think we have talked about it in an earlier session, but it was the actual final recognition from the French government, and—

11-02:08:21

Leitmann:

Well, it's interesting that it was left to the French to do that. I didn't get any special thanks from the US Army, other than their participation in that ceremony, of course [as I had mentioned earlier]. They came around, and well, my unit, as we discussed some time ago, was for a time attached to the First French Army during the campaign called the Colmar Pocket, which was sort of contemporaneous with the Battle of the Bulge. They first gave me the *Croix de Guerre*, which I presume we received, but just standing in line, and general de Lattre de Tassigny, the commanding general of the First French Army, handed me the medal ribbon and kissed me. Later on, they looked into the record and they decided they were going to give me the real medal. So on my eightieth birthday, the then-minister of defense of France, who was a woman at the time, invited Nancy and me to Paris, when they were celebrating the liberation of Colmar, so it was 2004—2005, actually, already.

Yeah, it was in the spring of 2005, so it was my eightieth birthday. Was it my eightieth? Yeah, 2005, and the liberation of Colmar, so they awarded me the medal at the time. It was handed to me by General Jean-Louis Roche, who later became inspector general of the French Army. We stayed at the main officers' club in Paris, and Josef flew in from Jakarta for that.

11-02:10:26

So, that was one thing, then more recently, another very close friend, we've mentioned her before, Gertrude Humily, whom I met while I was, again, pretty much like I met Evelies Mayer: I met her through contact with the National Research Council. I was a member and later vice chair of their fellowship committee, and she was the head of exchange programs for all of France, which is under the Ministry of Defense in France, and so she had lots of contact with the National Research Council here, and we met through that in the eighties, and we became very good friends. She's originally Viennese, but became French, and when she retired—this must be maybe six, seven years ago—she was made Knight of the French Legion of Honor, and she thought maybe I ought to get that too, given my contributions, so she was, I'm sure, behind the nomination for that. You never know exactly, but I can't think anybody else who would have done that.

So then in a little over three years ago, I was informed that I received the Knighthood of the Legion of Honor, and it turns out that they did quite a bit of that for US veterans who were close to the French Army, sort of in the French campaigns, important, and so that was simply a more personal way of getting it, and I got it separately. In some of the other cases, they had three or four veterans, and they had a ceremony somewhere, but here, they made it very personal, and it was very nice. How old was I? I was ninety, I guess. They said, "You don't have to come to Paris, we'll do it in situ," and there Shankar Sastry was terrific. He was very pleased with this, and he said, "I'll underwrite the whole thing here. We'll have a big ceremony at the Faculty Club." And so the ROTC battalions here were going to be the US Honor Guard, and we had a group of about fifteen doctoral students here from the *École Polytechnique*. They were all lieutenants because they get the rank when they graduate—Napoleon had started the *École Polytechnique*—and they were the French Honor Guard, and they even airmailed the uniforms, their parade uniforms, over here for them to do that.

So that was in the Great Hall of the Faculty Club, and Shankar paid for a little film company to actually film the whole thing. It's about a half-hour YouTube movie, and the French government has a much shorter one. They also did one of those, only about ten minutes long. So that was very nice. Again, and I'm not fishing for compliments, there are lots of people who deserve it, as much or probably a lot more, so it's again, not what you know but whom you know. I'm very aware of this kind of thing. You've got people speaking up for you, and it's wonderful, but other people deserve it and they don't have those connections, so I think it's obviously tied up.

11-02:14:50

Burnett:

Well, these events are celebrations of a person, but they're celebrations of events and connections and history and tradition, and they symbolize— One of the reasons why so many people were so enthusiastic about these celebrations, and also your birthday celebrations where there could be 400 people in attendance, is that you have led a life of service of varying kinds, and sometimes there are small things that you've touched people, that nobody knows about that you've helped, and in other times, you've been involved in world-changing events. You've been there, and you've witnessed them, and in some cases, you weren't the beneficiary, and so there has been suffering in your life as well. I think what's remarkable, what comes through this oral history, is a kind of equanimity, that you have reckoned with things that have happened to you, and you have made decisions about how to move in the world, and something that comes to mind is a sense of balance. There's something very balanced about your approach to life, and I think it's been—

11-02:16:15

Leitmann:

Yeah, well I've always felt, for a long time, that the most important thing in life is family, in whatever sense you want to use that term, connections with good people, and that's about the one thing that makes me happier than anything else, no question about it. No question about it. We've talked about this a lot: I've been very fortunate. No matter how you want to look at that, the more I think about it, the more I'm convinced that—I'm sort of amazed, let's put it that way.

11-02:17:01

Burnett:

Doctor Leitmann, thank you very much for sitting with us.

11-02:17:03

Leitmann:

Thank you.

[End of Interview]

SUMMARY OF BACKGROUND AND EXPERIENCE
 GEORGE LEITMANN
 Professor of Engineering Science

Academic Training

B.S., Physics, Columbia University, New York, 1949

M.A., Physics, Columbia University, New York, 1950

Ph.D., Engineering Science, University of California at Berkeley, 1956

Professional Experience

1950-57	Physicist and Head, Aeroballistics Analysis Section, Research Department, U.S. Naval Ordnance Test Station, China Lake, California
1957-59	Assistant Professor of Engineering Science, University of California at Berkeley
1959-63	Associate Professor of Engineering Science, University of California at Berkeley
1963-	Professor of Engineering Science, University of California at Berkeley
1958-64	Staff Scientist (part time), Applied Mechanics Department, Lockheed Missiles and Space Company
1957-67	Consultant for Martin Company, Denver; Aerojet General Corporation, Sacramento; U.S. Air Force Academy, Colorado Springs; Guggenheim Laboratory, Princeton
1966-67	Research Professor, Miller Institute for Basic Science
1968-70	University Ombudsman, University of California at Berkeley
1971	Chairman, Division of Applied Mechanics, University of California at Berkeley
1972-74	Vice Chairman, Graduate Study and Research, Mechanical Engineering, University of California at Berkeley
1981-85	Associate Dean for Graduate Study and Research, College of Engineering, University of California at Berkeley
1986-90	Associate Dean for Academic Affairs, College of Engineering, University of California at Berkeley
1988	Acting Dean, College of Engineering, University of California at Berkeley
1990-94	Associate Dean for Research Services, College of Engineering, University of California at Berkeley
1991-present	Professor Emeritus
1994-98	Chairman of the Faculty, College of Engineering
1995-present	Professor in the Graduate School
1999	Acting Director, Engineering Systems Research Center
1994-2001	Coordinator for Foreign Exchange Programs
2001-2003	Director of International Programs
2003-2012	Associate Dean for International Relations
2012 present	Special Adviser to the Dean

Professional Service (Partial list)

Department Representative, Assembly of the Academic Senate
 Member of various Senate committees; Chairman, Committee on Privilege and Tenure;
 member, Committee on Committees, Budget Committee, etc.
 Chair, Adv. Committee on University Research Expeditions (1988-1997)
 Chair, Adv. Committee on Research Center (1990- 1994)
 Vice-Chair, NRC Associate Programs Advisory Committee (1986-1998)
 Scientific Advisor, A.v. Humboldt Foundation, Bonn, Germany
 Founding President, Alexander von Humboldt Association of America (1994-1997)
 Permanent member, Board of Directors, A. von Humboldt Assoc. of America
 Member, Scientific Council, International Centre for Mechanical Sciences (1969-1995)
 Member, Academic Advisory Committee, AME School, Univ. of Oklahoma (1996-2003)
 Member, Astrodynamics Committee, AIAA; Committee on Rescue in Space, IAA;
 Committee on Math. in Control. IFAC; etc.
 Sponsor for Astrodynamics, ASME (1963-65) (1967-70)
 Visiting Lecturer, SIAM (1968-69) (1969-70)
 Member, Board of Editors, M. Dekker, Inc. (1969-75)
 Honorary Editor, Journal of Mathematical Analysis and Applications
 Honorary Editor, Communications in Appl. and Nonlinear Analysis
 Editor, Journal of Math. Analysis and Applications (1985-2000)
 Associate Editor, Journal of Optimization Theory and Applications
 Associate Editor, Int'l Game Theory Review
 Associate Editor, CUBO (Chile)
 Editorial Board, Dynamics and Stability of Systems (1984-1995)
 Editorial Board, Annals of the Int'l Soc. Of Dynamics Games (1991-1999)
 Editorial Board, Dynamics and Control (1994-2002)
 Editorial Board, Engineering and Automation (Russia)
 Editorial Board, Int'l J. of Pure and Appl. Mathematics
 Editorial Board, J. of Aerospace Engineering
 Editorial Board, Dynamics of Continuous, Discrete and Impulsive Systems
 Member, Review Board, Meccanica (1970-1985) (Italy)
 Member, International Board, Northern Institute of Technology, Hamburg, Germany
 Member, Int'l Review Committee, Japan Advanced Inst. of Science and Technology,
 Kanasawa, Japan
 Chair, Board of Directors, ARTSHIP Foundation (2005-2011)
 Advisor, Dynamical Systems, Int'l Institute of Applied Systems Analysis, IIASA
 Consulting Honorary Member, World Innovation Foundation
 Member, UCB Graduate Fellowships Advisory Board (2007-2015)
 Member, Engineering Advisory Board, Embry-Riddle Aeronautical University (2010-present)
 Member, UCB Office of Global Engagement Advisory Board (2011-2015)
 Member, Advisory Council of the UC Magnes Museum (2015-present)

Honors (partial list)

Member, National Academy of Engineering, U.S.A.
 Foreign Member, Academy of Science, Bologna, Italy
 Foreign Member, National Academy of Engineering, Argentina
 Foreign Member, Academy of Natural Sciences, Russia CIS
 Member, Academy of Engineering, Georgia CIS
 Foreign Fellow, Academy of Sciences, Georgia CIS

Corresp. Member, Bavarian Academy of Sciences
Member, International Academy of Astronautics
Fellow, American Institute of Aeronautics and Astronautics
1977 Pendray Aerospace Literature Award, American Institute of Aeronautics and Astronautics
1978 Pi Tau Sigma Award for Excellence in Teaching
1980 Mechanics and Control Flight Award, American Institute of Aeronautics and Astronautics
Alexander von Humboldt Senior Scientist Award
Levy Medal, Franklin Institute
Medal of University Liege, Belgium
Honorary Doctorate, Technical University of Vienna Honorary Doctorate, University of Paris
Honorary Doctorate, Technical University of Darmstadt
The Roscoe and Elizabeth Hughes Chair in Mechanical Engineering
Humboldt Medal, A.v. Humboldt Foundation
The Berkeley Citation
1995 Rufus Oldenburger Medal of the ASME
1995 Bellman Continuum Award of Excellence
Commander's Cross of the Order of Merit, Germany (Grosse Verdienstkreuz)
Commander, Order of Merit, Italy (Commendatore)
Member, The Berkeley Fellows
1995 "Russian Federation Proclamation on the Occasion of Prof. G. Leitmann's 70th Birthday."
2002 Distinguished Engineering Alumnus Award, UC Berkeley
Distinguished Emeritus Award, UC Berkeley, 2004
First recipient, Isaacs Award, Int'l Society of Dynamics Games, 2004 (with Y.C. Ho)
2005 Werner Heisenberg Medal, A. v. Humboldt Foundation, Germany
Biyearly "Leitmann Lecture," University of the Armed Forces, Munich, Germany (established in 2005)
2009 Bellman Control Heritage Award, Amer. Automatic Control Council
2009 Medal of Honor, Universitaet der Bundeswehr, Munich
2012 "City of Berkeley Proclamation Honoring George Leitmann"
2013 Austrian Cross of Honor for Science and Art, First Class
2013 Knight of the French Legion of Honor

Publications

1. "Crosswind Launching," *Ordnance*, 41 (219) 550, November 1956.
2. "A Calculus of Variations Solution of Goddard's Problem," *Astron. Acta*, 2 (2) 55, 1956.
3. "Stationary Trajectories for High-Altitude Rockets with Dropaway Booster," *Astron. Acta*, 2 (3) 119, 1956.
4. "Determination of Air Density at High Altitude by Means of an Earth Satellite," *Am. J. Phys.* 25 (2) 115, February 1957.
5. "Rocket Trajectories," *Ordnance*, 41 (221) 916, March 1957.
6. "Optimum Thrust Programming," *Aeronaut. Eng. Rev.*, 16 (6) 63, June 1957.
7. "An Optimum Pursuit Problem," *J. Franklin Inst.*, 263 (6) 499, June 1957.
8. "On the Equation of Rocket Motion," *J. Brit. Interplanet. Soc.*, 16 (3) 141, July 1957.
9. "A Note on Goddard's Problem," *Astron. Acta*, 3 (4) 237, 1957.
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13. "Optimum Thrust Direction for Maximum Range," *J. Brit. Interplanet. Soc.*, 16(9) 503, September 1958.
14. "On a Class of Variational Problems in Rocket Flight," *J. Aero/Space Sci.*, 26 (9) 586, September 1959.
15. "Some Remarks on the Optimum Operation of a Nuclear Rocket," *Proc. 10th Int. Astron. Congr.*, London, August-September 1959.
16. "Über den Energienbedarf von Raketen," *Raketentechnik und Raumfahrtforschung*, 3 116, 1959.
17. "Establishment of a Circular Orbit by Double Impulse," *J. Brit. Interplanet. Soc.*, 17 194, 1960.
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Non-Technical Publications

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Patents

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2. "Semi-active Control Apparatus for Damping Vibrations of a Body," No. 5,389,785 (with E. Reithmeier).

EDITORIAL

In Memory of Austin Blaquière



Professor Austin Blaquière, 1923-1993.

It is with deep sadness that I must inform you of the unexpected death of Austin Blaquière, Professor at the University of Paris VII.

Austin Blaquière was born in December 1923 in Nîmes, France, and died in April 1993, but a few months short of his seventieth birthday. He was educated at the Ecole Normal Supérieure de Saint Cloud and at the

University of Paris from which he received the Doctorate in Physical Sciences in 1953 and the Doctorate in Mathematical Sciences in 1957. He joined the faculty of the University of Paris VII in 1961 and rose to the rank of *Professeur Titulaire Classe Exceptionnelle*. Prior to his appointment to the Paris faculty he held professorships at various other schools in Beauvais, Bordeaux, and Paris; he was also a visiting professor at the University of California at Berkeley in 1964 and 1967.

Austin Blaqui re's research interests and contributions spanned an astounding range of topics including nonlinear differential equations, nuclear reactor theory, electronics, nuclear physics, optimal control, differential games, vector-valued optimization, and since the mid-sixties, quantum and relativistic mechanics. His contributions are memorialized in over 120 publications in major archival journals and in 10 books.

Austin Blaqui re's work is distinguished not only by its breadth but especially by its depth. He eschewed trivialities and concentrated his great talents on the investigation of fundamental problems which he treated with the rigor so typical of the best French tradition. For his contributions he was honored by his peers, most recently by the Prix Charles-Louis de Saulses de Freycinet of the French Academy of Sciences. This year, the Sixth Workshop on Dynamics and Control was held in Vienna in his honor. He was also an active member of the scientific community, organizing and chairing many meetings and symposia. He was a member of the Scientific Council of the International Center of Mechanical Sciences, and on the editorial boards of the International Journal of Nonlinear Mechanics and of Dynamics and Control.

In addition to his scientific accomplishments, Austin Blaqui re was a talented painter. His paintings and drawings were exhibited by various galleries in Paris, Warsaw, and Oslo.

On a personal note, allow me to mention that Austin was not only a dear friend but a valued colleague whose deep understanding of and rigorous approach to problems of mutual interest provided me with inspiration over more than 30 years and into the future.

Austin Blaqui re is survived by his wife Paulette, daughter Noelle, and son Marc, together with their families. To them we offer our sincere condolences.

George Leitmann
College of Engineering
University of California
Berkeley, California



Grandfather Alexander Leitmann



Family portrait, grandfather in uniform, c.1890s



Leitmann family - grandfather Alexander and father Josef in uniform



Father Josef and mother Stella, wedding, 1924



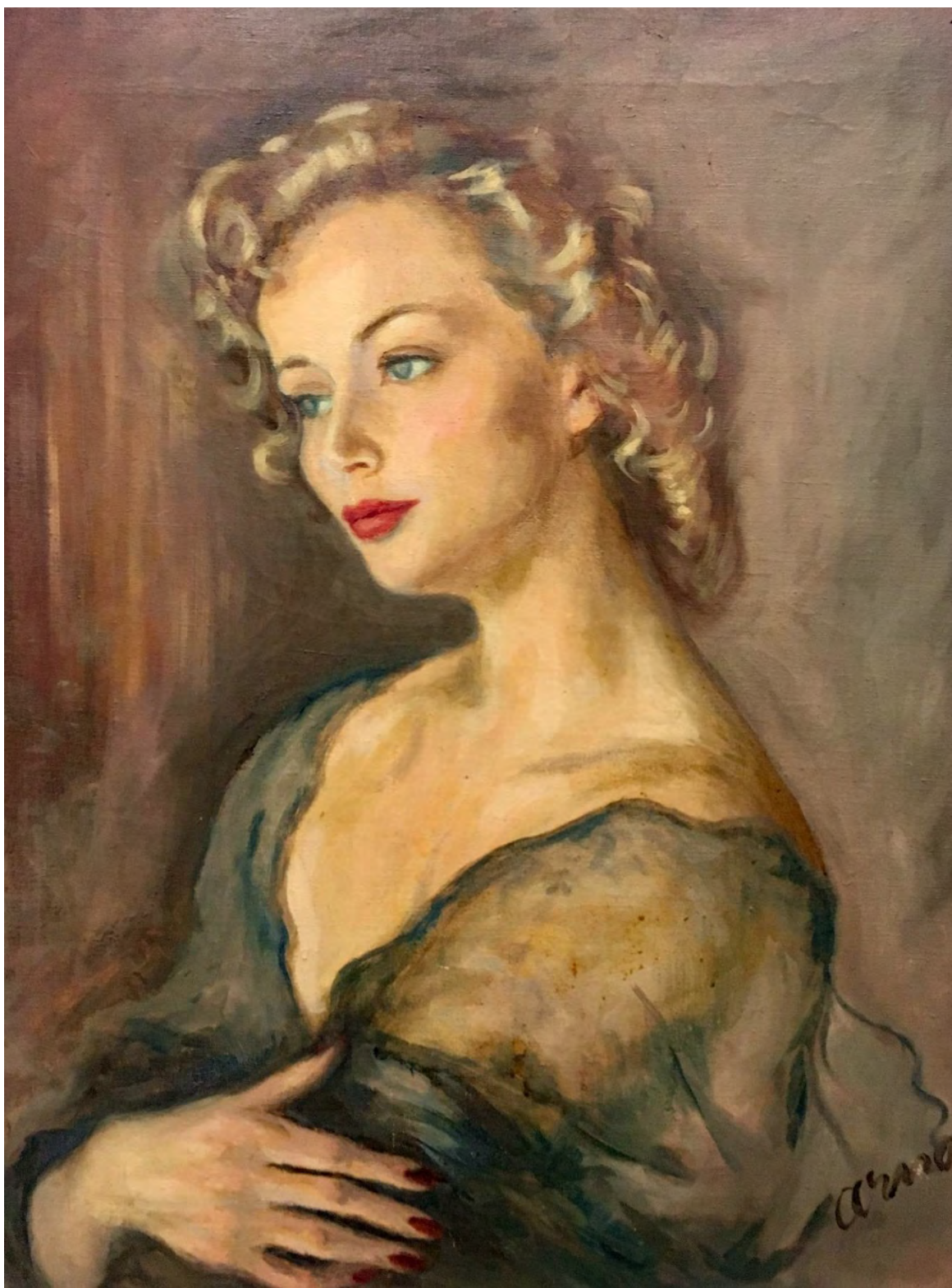
Baby George and friend, whom he almost drowned



Leitmann as a boy dressed inappropriately for play, Vienna, early 1930s



George Leitmann with father Josef



Maria Palmer, George Leitmann's cousin



Mother Stella, grandmothers Cevia and Fanny en route to the United States, May, 1940

IMMIGRANT IDENTIFICATION CARD
UNITED STATES
DEPARTMENT OF LABOR

LEITMANN
SURNAME

Georg
GIVEN NAME

Germany **May 24, 1925**
COUNTRY OF BIRTH DATE OF BIRTH

German **brown**
NATIONALITY COLOR OF EYES


NEW YORK **M/V SATURNIA**
PORT OF ARRIVAL STEAMSHIP

APR 18 1940 **Immigrant**
DATE ADMITTED STATUS OF ADMISSION

Georg Leitmann
IMMIGRANT'S SIGNATURE

ORIGINAL IMMIGRANT INSPECTOR

Georg Leitmann



George Leitmann's immigrant identification, 1940

Alien Registration No. 5108808

Name George Leitmann
 (First name) (Middle name) (Last name)

RIGHT INDEX FINGERPRINT

George Leitmann

Birth date May 24 1925
 (Month) (Day) (Year)

Born in Vienna Austria
 (City) (Province) (Country)

Citizen or subject of Austria
 (Country)

Length of residence in United States 2 yrs., 10 mos.

Address of residence 91-01 48th Avenue
 (Street address or rural route)
Elmhurst Queens New York
 (City) (County) (State)

Height 5 ft., 8 in.

Weight 170 lb.

Color of hair Brown

Distinctive marks None

Dolores E. Voelker
 (Signature of Identification Official)

Application filed in Alien Registration Division. Copy filed with Federal Bureau of Investigation office at **NEW YORK, N. Y.**

(Signature of holder) George Leitmann
 16-20160-1

FLUSHING, N. Y.
 FEB 25 1942
 M. O. B.

George Leitmann's alien registration card, 1942



Leitmann with Sid Shapiro, early 1945



Pontoon Bridges of the 286th Combat Engineers, Germany, 1945



Work of the 286th Combat Engineers, 1945



Bridge repair at Battle of Colmar, February 1945



Major Spence, Executive Officer of the 286th Combat Engineers



George Leitmann in an abandoned Messerschmitt on the side of the Autobahn, 1945



Explosion at Gerabronn, Germany, 1945



Landsberg concentration camp, 1945



Leitmann informs a German officer of the surrender procedure for his unit, April 1945

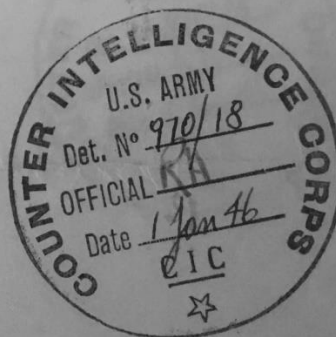
SCARS: NONE

"Counter Intelligence Corps Personnel will not be delayed in the execution of their assigned duties by the observance of standard customs or prohibitions nor by the Military Police or other military agencies....."

By command of General EISENHOWER

George Leitmann

Signature of Bearer



Leitmann, Counter Intelligence Corps authorization papers, detail, 1946

HEADQUARTERS
COUNTER INTELLIGENCE CORPS
UNITED STATES FORCES, EUROPEAN THEATER
Region II (Frankfurt)
APO 757

Sub-Region Wiesbaden

8 Mar 46

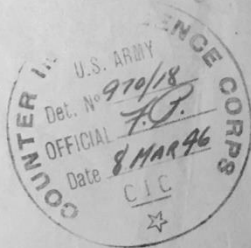
SUBJECT: Travel Orders

TO WHOM IT MAY CONCERN

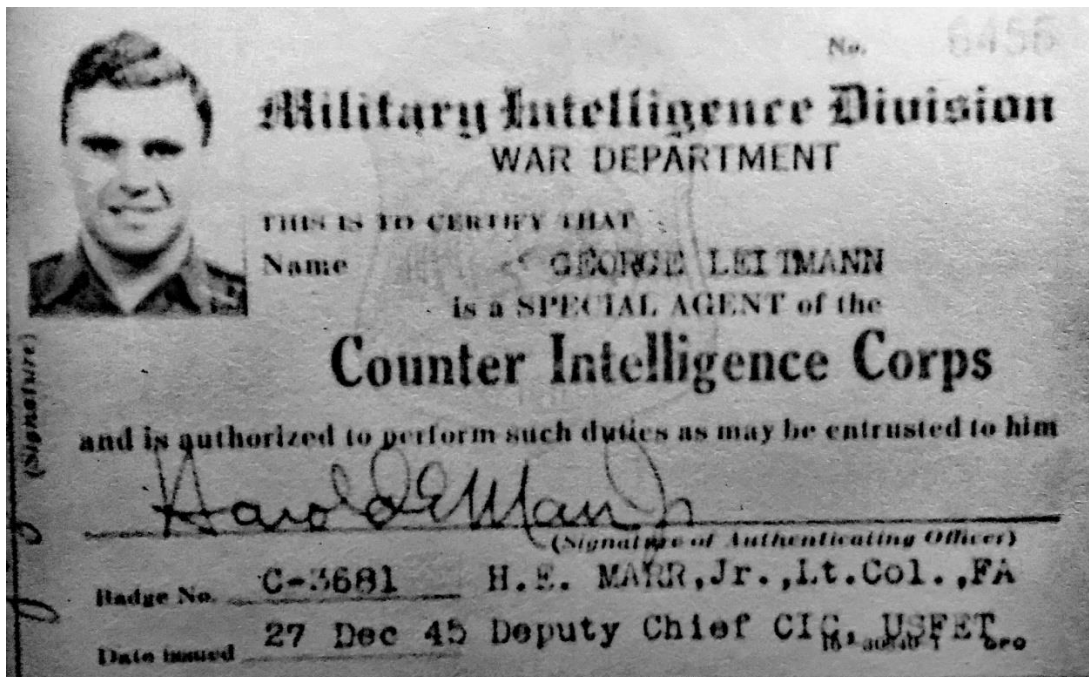
Special Agents Thomas K. HERRMANN and George
LEITMANN will proceed to PARIS on a confidential CIO mission. Upon
completion of said mission they will return to their proper station.

Government transportation authorized.

Frank Patton, Jr.
FRANK PATTON, JR.
TEAM COMMANDER



Counter Intelligence Corps security pass, Case Poupette



CIC Identification



CIC badge



CIC core group, from left to right –
Bob Leech, Sam Kraut, Arno the dog, Gunther Kessler, Leitmann, 1946



Counter Intelligence Corps colleague Tom Herrmann, c. 1946

HEADQUARTERS
U. S. FORCES, EUROPEAN THEATER
Office of the Commanding General

12 April 1946

SUBJECT: Commendation

TO : Chief, Counter Intelligence Corps, European Theater
(THRU: Assistant Chief of Staff, G-2, United States
Forces, European Theater.)

The successful completion of Operation NURSERY climaxes a long record of splendid performance on the part of the Counter Intelligence Corps in this Theater. In that operation, by good judgment and efficient functioning, the Regional and Sub-regional offices have eliminated a dangerous element from German life.

The achievements of the Counter Intelligence Corps in this connection gain all the greater significance when the nature of the organization just smashed is taken into account. Here was no fly-by-night scheme of harassment, concerned with chalking swastikas on buildings or with cutting wires. It was an organization planned in terms of years, not of months, whose ultimate aim was subversion designed to upset the aims of the occupation, to keep alive the spirit of National Socialist ideals and to undermine subtly whatever new German leadership may emerge from the country's downfall.

With patience and resourcefulness, the Counter Intelligence Corps Agents under the case direction of Lt. J. D. HUNTER developed the slimmest of leads and by close and careful surveillance of its key personalities recorded the growth of an underground organization since its inception in June 1945. Since the scope of this operation transcended the borders of the occupational zones, these Agents had to secure the cooperation of other occupying powers. Such a task called for tact, detailed knowledge of the situation, and sure judgment. That the development of this operation in all its ramifications proceeded without compromise is a tribute to the security-mindedness of the Agents; a single leak of important information would have alerted the subversive organization and driven it out of reach, even further underground, with its potential danger to the occupation greatly increased. The Agents were opposed by men with demonstrated records of ruthlessness. These men had money, arms, organization. The agents not only risked their own lives on numerous occasions; they had to persuade indispensable German informants to take innumerable risks, for which they could not even expect recognition.

Operation Nursery commendation letter



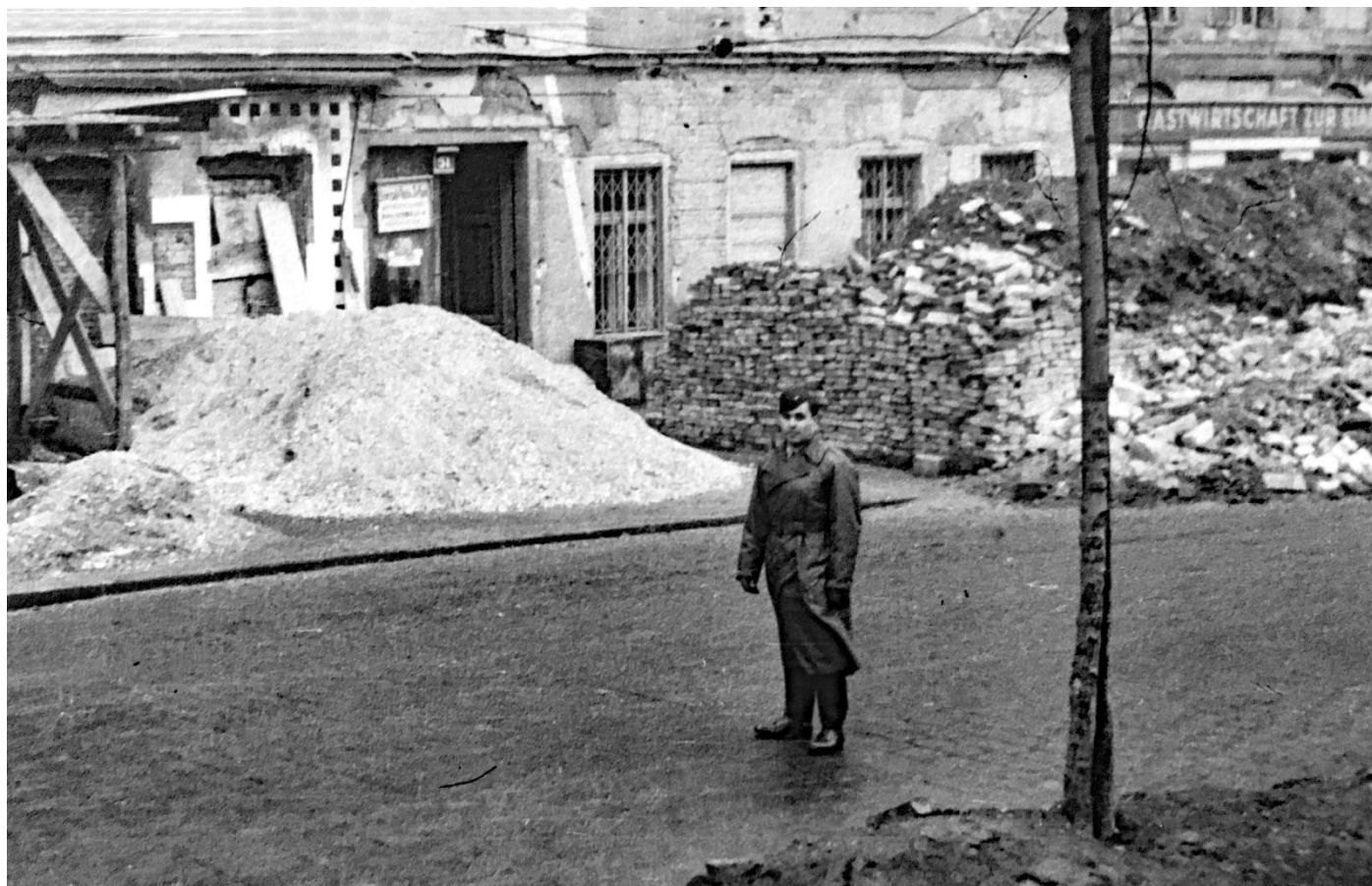
Leitmann as interrogator at Nuremberg Trials, 1946



Fischer Bros. Clothiers



Fischer Bros. and George Leitmann immediately post-war, February 1946



George Leitmann in Vienna, February 1946



George Leitmann, Counter Intelligence Corps training, c. 1949



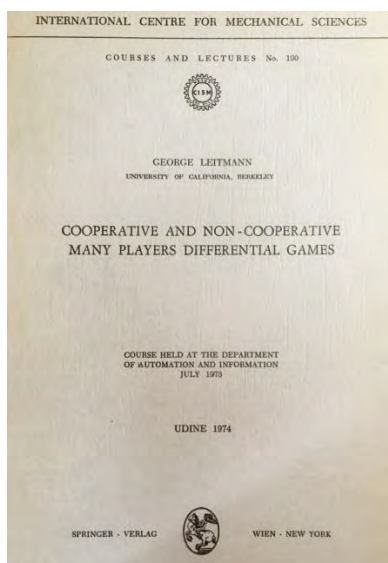
Stella Leitmann, late 1940s



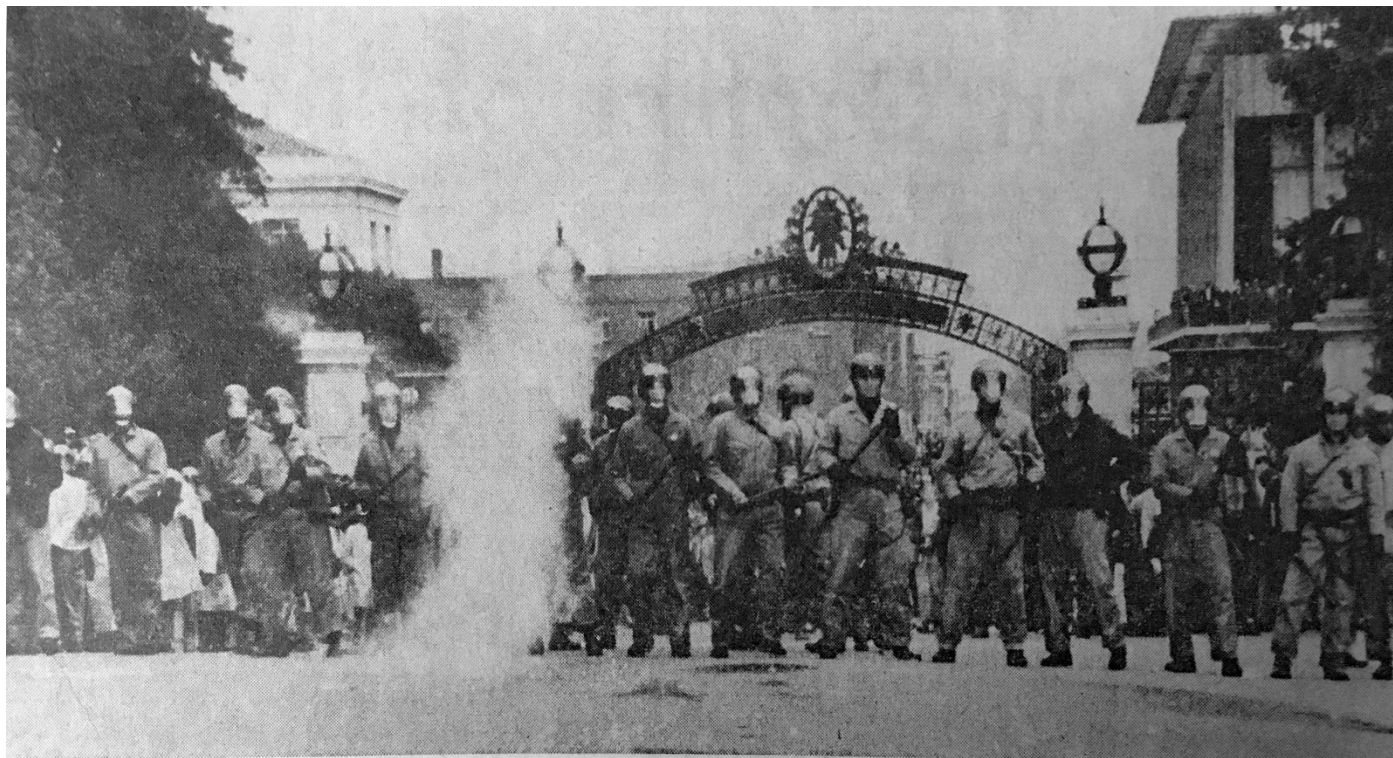
George Leitmann on rocket test track, China Lake, mid-1950s



George Leitmann's publications in German, Russian, Polish, and Japanese



CISM monograph, Udine, 1974



'Cherry bomb' type firecracker explodes in front of police line at Sather Gate on U.C. campus

Tribune photo by Lonnie Wilson



Campus unrest, UC Berkeley, *Oakland Tribune*, 1969

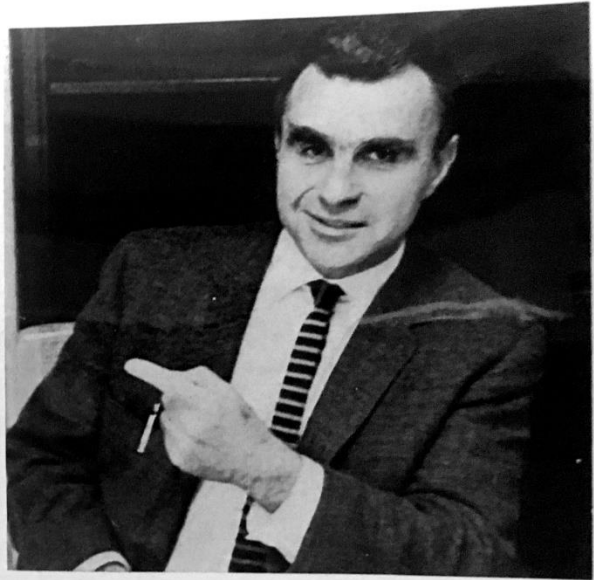
"Ombuds what?"

**That's right, the Ombudsman.
The truth behind those funny
little signs revealed.**

Every engineer, in his Northside habitat, must have noticed the funny little signs pointing to the dilapidated observatory building declaring: OMBUDSMAN. We're not sure about you, but the CAL ENGINEER was curious about this person with the strange title, what he did and why. There was even a rumor that this person was an engineer. In its glorious tradition of spreading truth and light among the engineering community, the CAL ENGINEER talked with the Ombudsman to elucidate its loyal readers.

Dr. George Leitmann, professor of Engineering Science, is the first and current holder of the position of Ombudsman. His visitor is immediately put at ease in his office which has been transformed from its vintage WWI ugliness into a warm, human place by the addition of numerous travel posters and a "Fate L'Amore—Non La Guerra" print. Here at last is help for the poor student, lost in the tangle of the bureaucratic web which governs the University.

"The office of the Ombudsman grew out of a feeling that there was a need for someone to whom students could turn for help in this large, sometimes impersonal university," explained Prof. Leitmann. "The legislation that resulted in the creation of the Ombudsman was



Ombudsman Professor George Leitmann

course of your investigations and recommendations?

"There is a psychological resistance, a natural drawing-back. When I call them in the course of investigating a complaint people tend to think, 'What have I done wrong,' though in no sense am I accusing them of doing anything wrong. My primary function is investigative. I'm also here to protect people against *unwarranted* complaints.

"If I question people in regard to a complaint, some feel that I am threatening their prerogatives. I have had a few cases involving the fairness of grades. Some professors feel that a grade a student receives is his prerogative and no one else's."

Leitmann as Ombudsman, magazine article, 1978

Thurs., Apr. 23, 1970 ☆☆

San Francisco Chronicle 5

A Report on How Police Acted During UC Clashes

The ombudsman of the University of California in Berkeley said yesterday he saw some "rather rough treatment of arrestees" by Berkeley city police during last week's campus commotion and witnessed one beating.

But the complaint czar of the campus, George Leitmann, a professor of engineering sciences, gave rather high marks to University of California campus police.

"With one exception," he said, neither he nor his observers saw campus police use excessive force on the some 80 persons arrested during the three days of disturbances.

EXCEPTION

That one exception occurred April 17 when six officers ganged up on a single juvenile, he said in a report issued late yesterday.

"The juvenile's face was pushed into the ground, an officer knelt on his back; after handcuffing he was pulled to the Sproul Hall stairs, pushed down, fell and was then dragged inside."

When a faculty observer tried to remonstrate with the arresting officers he was "threatened with arrest," the ombudsman said.

Berkeley police usually took anyone they arrested to Sproul Hall under guard by four Berkeley officers, he said. The four would throw the arrested person to the ground, spread-eagle him, and search him while one officer "knelt on the small of his back."

WITNESS

He personally witnessed one beating during the demonstrations, Leitmann said,

when he and a colleague saw three officers (he wasn't sure whether they were Berkeley or campus officers) catch a fleeing male.

"While two officers repeatedly struck the person's genital area, one officer ran up from behind and struck him on the head, felling him."

The officers then left the bleeding man and chased off after other demonstrators, he said.

ROCKS

The ombudsman made no report on the conduct of the rock-throwing protesters except to note that "at one time, near Moses Hall, two youths, one white, the other

black, asked me what I was doing."

"When I informed them of my role they picked up stones, said, 'Now observe this,' and threw them in the direction of the police."

Campus Police Chief William Beall said his department will investigate the incidents recounted by the ombudsman and will take disciplinary action "if the findings support such action."

However, the chief said the ombudsman's report "should be read with the fact in mind that a majority of this department received injuries in last week's rioting and that injuries were inflicted on other police and newsmen."

★ Bay Region ★

HOW ROUGH IT WAS—The University of California's faculty ombudsman says there were some instances of "rough treatment" by police during last week's campus demonstrations, but that there was no excessive force witnessed.

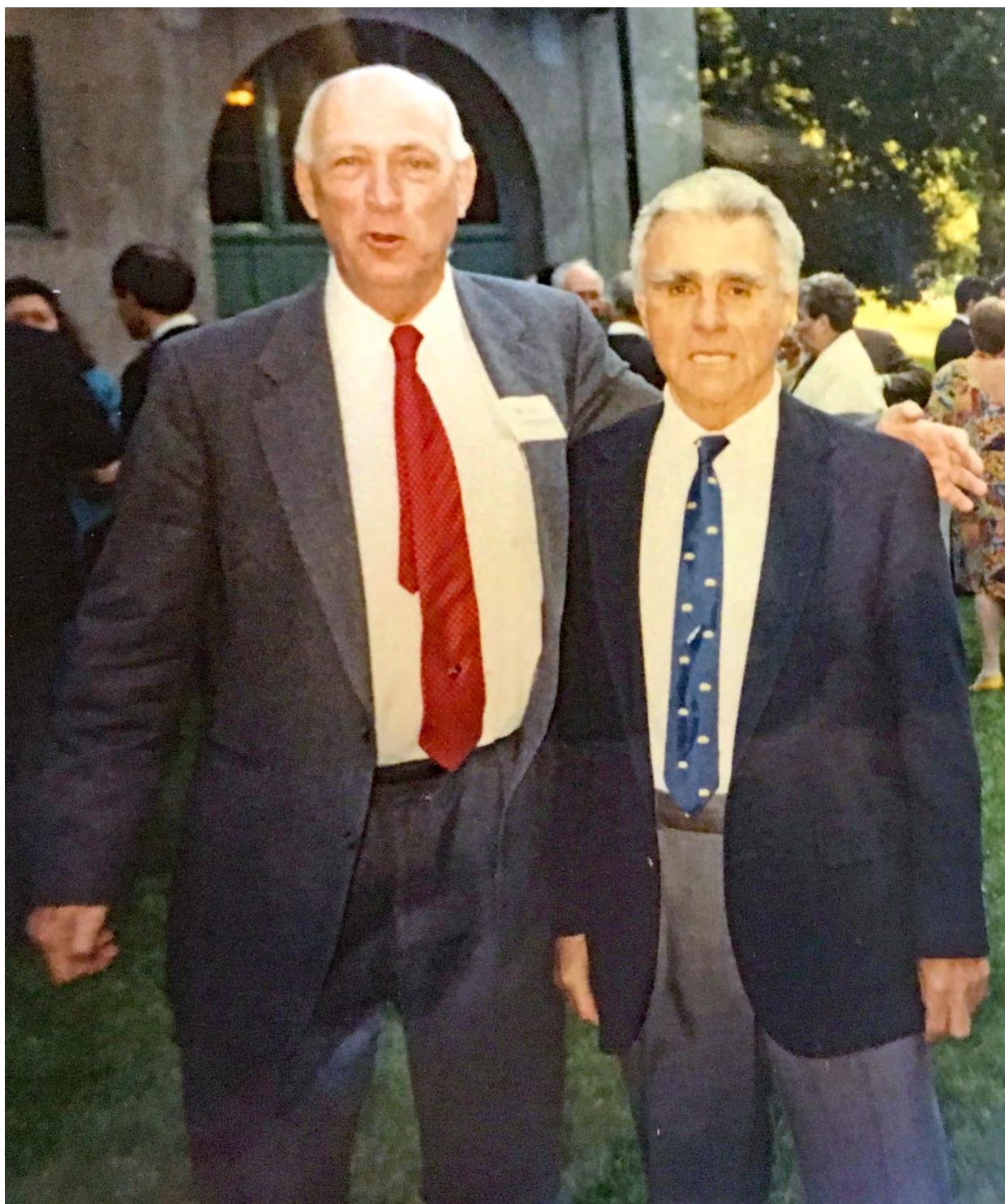
Page 6

DOCTOR'S UNION — Resident doctors and interns at Highland Hospital have organized a bargaining unit in their efforts to win better salaries. They have affiliated with the Alameda County Employees' Association.

Page 25

SMOG BATTLE—Rep. Jeffery Cohelan today joined 29 other congressmen in introducing legislation which would ban all large horsepower automobile engines by 1975 as a move against air pollution.

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George Leitmann with thesis adviser Dr. Werner Goldsmith



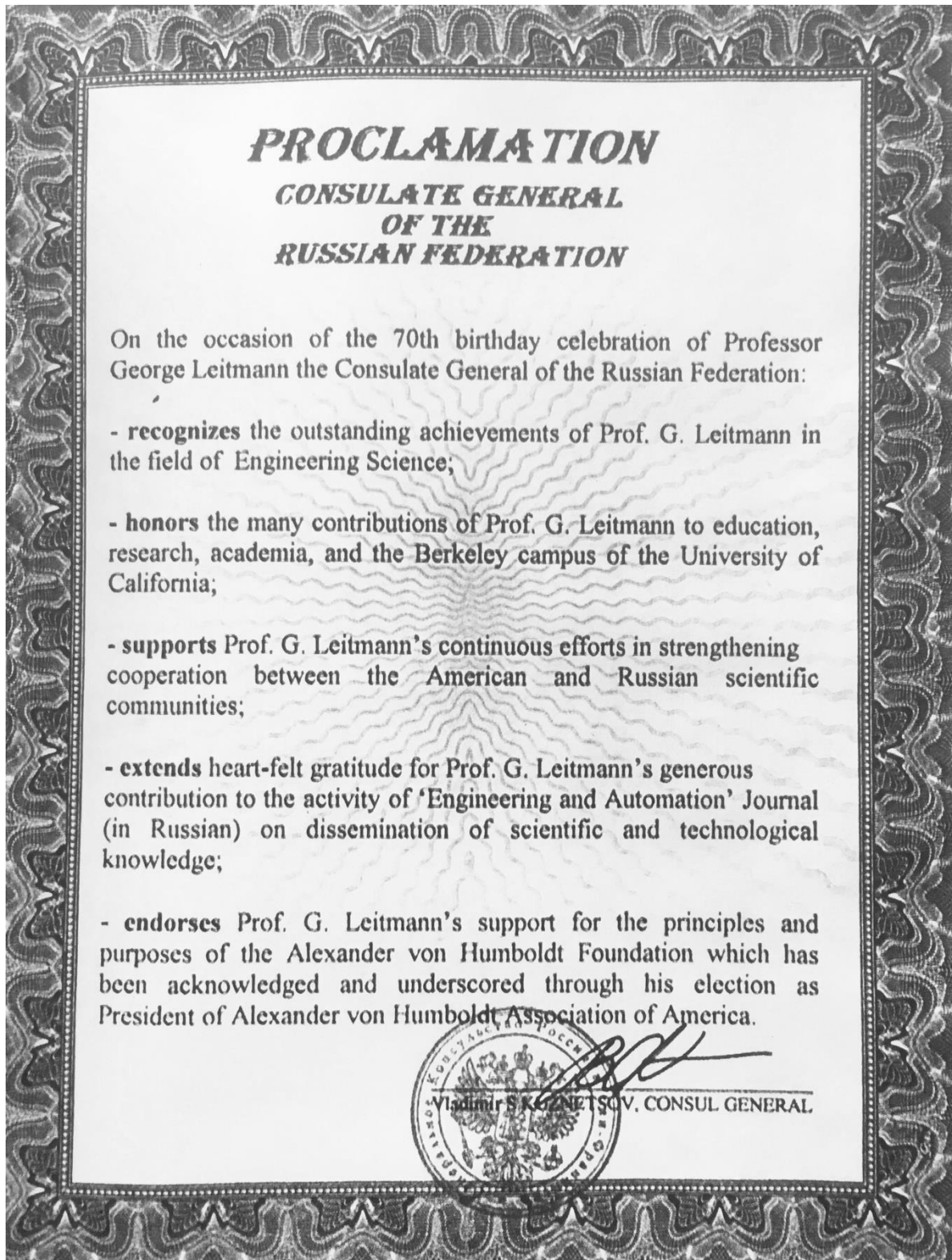
Meeting with President Etsujiro Shimenura of JAIST
and Eberhard Hofer of the University of Ulm, October 9, 1998



Austin Blaqui re and George Leitmann



Dr. Heinz Fischer, Federal President of the Republic of Austria with George Leitmann on the occasion of Leitmann's 85th birthday, Vienna Hofburg, May, 20



Proclamation of Russian Federation in honor of George Leitmann, 1995, on his 70th birthday

George Leitmann and the Reshaping of Modern Science

I met George Leitmann in June 1951 at N.O.T.S., China Lake, Ca. George was working in their Ballistics Division on trajectory problems. I had been at the U.C.B. Math Dept 4 years and was a consultant at N.O.T.S. on O.D.E.'s and applied math. I was at the start of a 47 year uphill battle to get U.C.B. Math Dept to expand the number of faculty, grad students, and course offerings (e.g. calculus of variations) in applied math.

George, recently released from the Army, had an A.B. in Physics. In the Ballistics Division George was well known for his quick wit and breath of knowledge. It was rumored his section head spent most of the lunch period reading the Encyclopedia of Britannica to be able to one-up George in casual talk. My position did include talking to George and others working on trajectory problems. It was a pleasant surprise to find someone having only undergraduate training who was thinking and working at a level of a mature scientist.

I realized his is an exceptional talent, and did all I could to get him to come to Berkeley and enter the Math Dept. Well, George did come to U.C.B. for his graduate work, but it was in Engineering and Professor Goldsmith will speak about that.

As all of you here at this meeting know George has had a major role in the development of Control Theory as a significant area of scientific study and applications. George's role in this development has been established by outstanding and prolific production in his own research, the development of top flight scholars, and various editorial activities.

All this work has not left George a dull boy, for he is well known for his expertise as an art critic, a food critic, and his humor.

George is also known as an astute political analyst, but he personally is an antidisestablishmentarianist. Despite this, George and many of his students and friends - e.g. most of those here this evening, have played a key role in a change in the map of the world of the sciences. Areas of great activity that were labeled mathematics 40+ years ago are now being ~~carried out~~ by scientists in a variety of different disciplines. To put in another way: Areas that belonged to mathematics 40+ years have been lost by default, erosion, gerrymandering, or rights of eminent domain [e.g. Control theory, mathematical economics, linear programming, statistics, computer sciences] and are now in a variety of other academic departments. This switch in who controls and/or fosters the work in applied mathematics has been a good one for science for in the climate outside of math departments these fields have blossomed beautifully.

It is a pleasure to acknowledge the role played by the work of George Leitmann, his students, and his colleagues in changing the landscape of modern science and to congratulate George, his students, and colleagues.

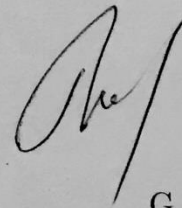
Stephen P.L. Diliberto 6/12/95

Dear Professor George Leitmann,

It gives us a great pleasure to say a few words of congratulations on the occasion of your jubilee. You are world-famed as one of the most eminent scholars of the present whose labors resulted in the creation of the modern theory of control. Your outstanding scientific achievements are well known in Russia and influenced noticeably the development of control theory in our country. This influence was appreciable since the very beginning of your scientific career and proved that even the iron curtain is not enough to break off the dissemination of ideas of the real novelty. Since that time we were lucky to enjoy many your bright works concerning the theory of control of uncertain systems, the theory of optimal control, the theory of differential games as well as many other branches of systems and control theory. Your book *"Introduction to optimal control"* was translated into Russian and is highly popular in our country. We are pleased to note that you are very popular among the students of the Mathematics Department of Saint Petersburg University. Your works are constantly reviewed by the students to be reported at the regular seminar on control theory.

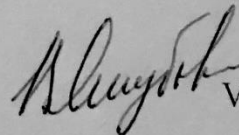
From the opposite side of the Earth, we wish you good health for many years to come, a long creative life and many new pupils for it is people such as you are that sow the seeds from which the reachness and the vital power of our science grow.

Dean of Mathematics Department
Saint Petersburg University
Professor



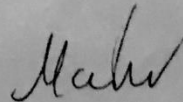
G. Leonov

Chair of Theoretical Cybernetics
Saint Petersburg University
Professor



V. Yakubovich

Scientific Secretary
Chair of Theoretical Cybernetics
Saint Petersburg University
Professor



A. Matveev

Russian colleagues' congratulations on George Leitmann's 75th birthday



Leitmann lecture, Berkeley, 2018



Son Josef Leitmann and Bill Clinton discussing World Bank disaster relief, 2011



Son Josef, Wife Nancy, and Grandson Joseph Rafael



Daughter Elaine
California Marathon, 1987



Grandchildren Alessandra and Nicholas



Elaine, 1969