



## An Oral History of the Linus Pauling Institute, August 26, 2011

**Title**

“Getting Started at the Institute”

**Date**

August 26, 2011

**Location**

Valley Library, Oregon State University.

**Summary**

In interview 1, Lawson discusses his background before his employment with the Linus Pauling Institute of Science and Medicine, his hiring at LPISM, and his initial role at the Institute. He also describes his early interactions with Linus Pauling and the evolution of their relationship over time, including their scientific collaborations. He likewise shares his recollections of Pauling's personality and work style, and his memories of Ava Helen Pauling.

**Interviewee**

Steve Lawson

**Interviewer**

Chris Petersen

**Website**

<http://scarc.library.oregonstate.edu/oh150/lpi/>

## Transcript

**\*Note: Interview recorded to audio only.**

**Chris Petersen:** Ok, we'll have you start by stating your name and our location.

**Steve Lawson:** My name is Steve Lawson. Today is August 26th, 2011, and we're in a conference room at the Valley Library.

**CP:** Today we'll be talking primarily about your interactions with Linus Pauling and specifically trying to get a better sense of Pauling's, sort of personal characteristics, but before we get in to that too deeply, I'd like you to talk just a little bit about your life starting out with sort of a thumbnail sketch of your biography before you entered the LPI in 1977.

**SL:** I was born in Pennsylvania and lived there for the first few years of life. I remember collecting donations for the Little League World Championship, typically held in Willamsport, Pennsylvania. Little League is a big activity in that area of north central Pennsylvania. We moved to Massachusetts along the coast north of Boston in the late 1950s. I'm not exactly sure of the date. I went through elementary school and high school in Topsfield, Massachusetts, which is a town in Essex county very close to the coast. We lived in Ipswich originally, which is a coastal town in Essex county. After graduation from high school, I went to Stanford University. I started work at the Linus Pauling Institute of Science and Medicine in 1977 and took a full-time job there in February of 1978, I had a number of different job titles there through the years, eventually becoming Chief Executive Officer in 1993, about a year before Linus Pauling died. I have worked at the Institute for most of my career.

**CP:** What led you to develop your interest in science or think about pursuing it as a career?

**SL:** Well that's an interesting question. I was a little rudderless in college. It was very difficult for me to focus on a career. Some of my friends were very career-oriented and became physicians lawyers and went to business school, and that's pretty typical for an environment of academic excellence like Stanford University. There are a lot of high achievers there, and I myself was a high achiever in high school. California was a big adventure for me, coming from the relatively staid and conventional East Coast. It was a very wild time to be in California in the early 1970s, both culturally and politically. There was a lot of antiwar activity and, of course, the counterculture was really thriving in California at that time. In high school I was very interested in science and took advanced calculus, physics, chemistry, and biology with the idea of perhaps becoming an architect which led me to apply to Rensselaer Polytechnic Institute as one of my college choices because RPI had a very strong department of architecture. Stanford had a department of architecture as well, and I decided ultimately to go to Stanford. After I got there, within a year or so, they had completely dismantled the department of architecture, so that pretty much pulled the rug out from under me with respect to that potential career choice.

I liked science very much. I've always liked science very much. I've always been very curious about nature and how things work, although I was relatively naïve about Linus Pauling in high school and even in my early years in college. I had been taken vitamin supplements since high school. Actually, my mom gave me One-a-Day Multivitamin Supplements even when I was just a kid, but then in high school my friends and I got very interested in supplements and took vitamin E and vitamin C. So the whole mission of the Linus Pauling Institute really resonated with me. That led me to a position there in 1977 that was, however, not science oriented. It was more related to analysis of the Direct Mail Program implemented a few years before I joined the Institute. It provided a relatively steady income for a lot of the non-grant supported research that was going on at the Institute, which, of course, in those days was most of the Institute's research.

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Between my graduation from Stanford and when I joined the Institute, I had a fairly carefree and adventuresome time. I spent a year after graduation traveling around the country visiting family and friends in Florida, Georgia, Maine, and New England. I wanted to see the country. I'd been very impressed with John Steinbeck's book *Travels with Charlie* and also with Jack Kerouac, who grew up in and is now buried in the same area in which I grew up in Massachusetts. Both of those writers had a very strong influence on me, I think, and my desire to experience the United States. When I returned to California after traveling around for a year I took a series of jobs. Cost of living in those days was very low; for about a hundred dollars a month, I could cover my living expenses quite easily, and I was not, as I said, career oriented at the

time, so I did not feel compelled to go to graduate school or begin a career in earnest. It wasn't until I joined the institute that I really started to focus more specifically on science, which really thrilled me.

**CP:** Were there any - you mentioned your interests in supplements when you were in college and I would assume that Pauling was probably someone you were aware of at least by the time you had graduated. Was that true and are there any other scientific role models or mentors you had during that time period that come to mind?

**SL:** Well I began taking vitamins when I was in grade school, and that continued into my early years in college. Of course, Pauling had written *Vitamin C and the Common Cold* in 1970, and friends of mine had opened health food stores and had supplement products as part of their merchandise, so I was aware of this whole field early on without really knowing specifically much about Linus Pauling. As a matter of, I knew very little of Linus Pauling even when I went to Stanford as a freshman. It wasn't until probably about 1972 that I became more aware of him, and it didn't have anything to do with science. I don't believe he taught undergraduate courses in chemistry at Stanford, I don't think he was teaching graduate level courses at Stanford either, but he was very politically active, and of course, this was during a lot of the vehement anti-war protest around the country. Stanford was no exception to that kind of activity.

At Stanford there was a tenured professor of English, H. Bruce Franklin, who was one of the world's foremost authorities on Melville and has subsequently done a lot of work in recognizing science fiction as an appropriate academic discipline for study. He had joined the Revolutionary Union and Venceremos, which was an organization I believe based in Redwood City, California. There were very politically active. They were kind of Maoist organizations and really wanted, not only an end to the Vietnam war, but more dramatic upheaval in the federal government. Stanford eventually had had enough of H. Bruce Franklin and decided to fire him. Of course, he was a tenured professor, so that was a big ordeal that they had to work through in order to fire him.

Stanford trustees were quite conservative. I remember walking to class down at the quadrangle at Stanford and saw a protest outside the President's office. There was a group of undergraduates and other people assembled around a couple of people waving protest signs. The signs said the University had no right to fire a tenured professor for his political activity. I saw these two fellows holding the signs and they looked grandfatherly to me. One was Linus Pauling and the other one, I believe, was Hal Holman, who was Chairman of the Department of Medicine at Stanford University Medical Center and a good friend of Linus Pauling at the time. I remember being very impressed with the fact that these two elderly gentlemen were taking the time out from their busy schedules to go and actually protest in front of the President's office. I thought that was quite remarkable because most of the political activity was concentrated among very young people. Sometimes it was very random and chaotic and inchoate as well, but I thought that it was quite remarkable that there were older people who were also interested in demonstrating for the rights of people to express their political opinions.

So that was my first awareness of Linus Pauling from the political perspective, not really from the scientific angle. It wasn't until I joined the Linus Pauling Institute that I really became much more familiar with Linus Pauling's work in chemistry and biology.

[0:10:27]

**CP:** What was your area of study at Stanford?

**SL:** Well, when I went to Stanford, as I mentioned, I was very interested in architecture as a potential career, so I was taking engineering and architecture courses. I also had kind of a renaissance concept of undergraduate education. I thought it was very important to take courses in as many different subjects as possible, so I was taking calculus, biology, art history, literature, and poetry—just everything that I thought I should be exposed to as an undergraduate. I graduated with a degree in anthropology, but I probably had almost enough credits for a degree in biology, or at least a minor in biology.

At the time, there were a couple of pretty notorious professors in Stanford who made a big impression on me. One was Colin Pittendrigh in the biology department. He also was director of the Hopkins Marine Station in Monterey. He taught a terrific course in biology and also Katchadourian in human biology, William Dement taught a very popular course in sleep and dreams that was very popular among undergraduates. Philip Zimbardo taught a very popular psychology course and he organized the Stanford Prison Experiment, which was quite influential at the time.

**CP:** How did you come to know about the Linus Pauling Institute and the job you ultimately applied for?

**SL:** I had lived in Menlo Park or Palo Alto for many years. The Institute was founded in 1973 on Sand Hill Road, opposite the Stanford Linear Accelerator. There were publishing companies, real-estate companies, and insurance companies on that street. Now that area is a hive of venture capitalist activity. The Institute shared a relatively modern office building with the Kemper insurance company. I was aware of the sign out on the street, and I'd driven by there many times on my way to the coastal range or to the beach. I think that I saw a job advertisement in the paper. I went to interview at the Institute with Art Robinson, who was then the President and Director of the Institute, and he hired me.

**CP:** Do you recall the first time you met Linus Pauling?

**SL:** Well, the first time I saw him of course was, as I said, back in about 1972 when he was demonstrating against the firing of H. Bruce Franklin. I met him in 1977 because my office was just off the hallway that Pauling took to get to his office, which was adjacent to Art Robinson's office. He was very aware of who was working at the Institute, and he stopped in to introduce himself to me when I was in my office one day. Of course I remembered him from Stanford and held him in high regard. He impressed me as a very warm, charming, friendly person. He was not distant at all, not aloof. He was very welcoming.

**CP:** Was he frequently around the Institute? I mean, I know he traveled a lot, but do you recall him being a regular presence in those early days?

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**SL:** He did travel quite a lot, but he was a regular presence and when he was in town, he would come by the Institute. That was his office and his main base of operations. If he had to do interviews or see guests, it was typically at the Linus Pauling Institute. I remember meeting many of his friends at that time, including an emissary from Szent-Gyorgyi's lab at Woods Hole, Ronald Pethig, who was very interested in telling us about Szent-Gyorgyi's new theories regarding vitamin C and the electronic basis of life—vitamin C, serving as an electron donor, essential to life, far above its role in preventing scurvy. Dorothy Hodgkin also visited the Institute at that time, and I also met Ewan Cameron, who, of course, was a Scottish surgeon collaborating intensively with Linus Pauling on vitamin C and cancer.

**CP:** Did you meet Dorothy Hodgkin?

**SL:** I did. At that time, I believe I was working in the laboratory. I became friends with a biochemist named Alan Sheets at the Institute, as well as Dick Willoughby, who was a biologist working on the skin cancer and vitamin C experiments that Pauling had designed and were being carried out by Art Robinson. As Art's influence in the Institute started to wane, people responsible for a lot of the biological/biochemical research were encouraging me to get more involved in that work. I seemed to have the aptitude for it and I was quite interested in it, so I moved out of the role that I was originally hired to perform at the Institute, which was Direct Mail solicitation and analysis, into kind of a research assistant position in the labs. That led me to come in contact with people that Pauling would bring through the laboratory to show them the work that we were doing. Bill Aberth was running a volcano-source field-ionization mass spectrometer, used for metabolite profiling, which was a big part of the Institute's research at the time, so people were always interested in coming down to the first floor and seeing that Rube Goldberg contraption. It never seemed to be quite ready for routine biological experimentation, and was always being tinkered with and modified.

**CP:** So, you were hired in 1977 at LPI and have built a career there. Pauling lived until 1994, so that's a 17 year period. I'm wondering if you could just generally characterize your relationship with Pauling, how it evolved over those 17 years?

**SL:** Sure. Originally, I saw little of him because my work did really not overlap with his research interests, and, of course, he was heavily involved in the financial status of the Institute, and Art Robinson was the main force behind the Direct Mail Solicitation program. But Pauling was certainly aware of that and once I started working in the laboratory more, we had more to talk about. I became involved in the skin cancer program, which was testing the effect of different levels of vitamin C in a mouse chow diet on the development of skin cancer in hairless mice exposed to ultraviolet light. There were other parts of the program that involved feeding raw food and seeds to mice to see whether that also inhibited the development of skin cancer. They were given carrots, pears, sunflower seeds, tomatoes, and other raw food to find out

what effect that would have. When Art Robinson left, Pauling asked me to calculate the amount of certain micronutrients in the food that the mice were fed because as scientists, we tend to be reductionist. We want to find out if there is an effect with whole food and what is it in the food that might be protective.

That was a very challenging project that Pauling gave to me, and I was a bit frustrated because the way that the food had been provided to the mice made it very difficult to determine how much an individual mouse consumed of any particular food stuff. The mice were also caged five to a cage, and there were social patterns that developed—a hierarchy that developed within the cage—so there were dominant mice who would eat the preferred food and then the less dominant mice would get what was left. There was some competition for the best food. The food was provided to them as blended food and sometimes as chunks of carrots and pears and so forth. The sunflower seeds were given to them in small glass containers with metal lids that had a hole in them, and the mice would spray the sunflower seeds out all over the place. It was really impossible to determine the intake of any of these foods for a specific animal. Even for a cage of animals divided by five, it was really not appropriate calculation. There was a lot of waste, and a lot of food would fall through the mesh to the floor of the cage rack. Sometimes they would defecate and urinate in the sunflower seed container as well, and then wouldn't continue to eat once they already spoiled seeds in that container.

There were other issues as well. For instance, the vitamin A content of carrots can differ appreciably depending upon how long the carrots are stored after they have been picked. There was no concurrent nutrient analysis of the food that was provided to the animals. Depending upon the variety, ripeness, length of storage time and whether it was refrigerated or stored at room temperature, the micronutrient content of the food could vary appreciably. I made the calculations, and I sat down with Linus Pauling and told him that in my estimation it was really impossible to glean any really useful detailed information about the micronutrient intake of the animals and that the best we could do was say that the raw food diet seemed to be protective. I think I did a very thorough and good job in making those determinations, but ultimately what I concluded was a bit disappointing.

**CP:** And how did he react to that?

**SL:** Oh he didn't react with disappointment, he seemed very pleased to get the information.

It was not long after that that Ewan Cameron began spending quite a lot more time at the Institute. Cameron, of course, was very interested in high-dose vitamin C as an adjunctive therapy in cancer and he and Pauling had done some clinical work together over the years beginning probably in 1971 that even preceded the founding of the Linus Pauling Institute of Science and Medicine. Cameron was concerned with some of the practical issues about the administration of high-dose vitamin C to cancer patients who might also be taking chemotherapeutic drugs.

Cameron asked if people were taking chemotherapeutic drugs, should they take high dose vitamin C at the same time that might have an effect on the efficacy of the drugs? So he didn't want to recommend to people that they take high-dose vitamin C while taking chemotherapy if the vitamin C was going to somehow adversely affect those drugs. He wanted to find a way to study this, and in association with Linus Pauling and Alan Sheets, we came up with the idea of using a fish toxicology experiment, which is a pretty well established model for studying pollution in bodies of water.

Alan and I reconstituted water from distilled water by adding back certain salts to provide the proper aqueous habitat for fingerling trout. The idea was that we would have fingerling trout that would be exposed to vitamin C and chemotherapeutic drugs dissolved in their water and we would monitor any observable effects in the fish. We hypothesized that they would be detected by abnormal behavior in the way they swam, the distance that they covered in a certain period of time, and so forth. There was some technology that was also developed to make these kind of measurements.

Unfortunately, we had to vacate the building the 2700 Sand Hill Road in Menlo Park, ostensibly because there had been cracks in the building due to small earthquakes and settling over time. The landlord wanted to get the tenants out and make some structural corrections in the building. I don't think the landlord was overly excited about our laboratory in the building anyway. We were sharing the building with Kemper Insurance company, and I think he was happier having a nice, insurance company than having wet labs there with animal and other experiments. So those experiments had to be wrapped up before fruition when we moved to the facility at 440 Page Mill Road in Palo Alto.

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**CP:** Were there other scientific collaborations with Pauling over the course of your relationship?

**SL:** There were. He would come into my office from time to time and we would talk about ideas he had. Occasionally, he would ask me if I would be interested in doing something like writing a book review. When Evelleen Richards' book, *Vitamin C and Cancer: Medicine or Politics?*, was published, I believe in 1991 or 1992, Linus Pauling came into my office with a copy of the book and asked me if I would be interested in writing a book review to be submitted to a nutrition journal. I said yes and wrote the review, which was subsequently published in the *Journal of Applied Nutrition*. I would go into his office from time to time and talk with him about different things.

Zelek Herman, who was collaborating with Pauling on problems in theoretical chemistry and physics over the whole period in the late 1970s through Pauling's death, and I were both rock hounds. We had collected rocks and minerals when we were kids, and we knew that Pauling was a great mineralogist and understood a lot about the silicate minerals, which are quite interesting. The three of us would amuse ourselves occasionally by bringing in some of our mineral specimens. We would sit in Pauling's office and try to stump the expert. Sometimes Zelek and I would have forgotten the identification of some of the minerals we had, so they would be presented to Dr. Pauling to see if he could identify them. It was astonishing because even though he had probably not thought about some of these things in many years or even decades, he was infallibly able to identify the mineral. So that was fun. It didn't have anything to do with any of the research going on at the Institute, but it was fun to sit and talk with him in that way.

Later, in the late 1980s, probably around 1988, Pauling came into my office with an idea for a new technique to fabricate superconductive material. This was during a period of interest in ceramic superconductors, but Pauling had a vastly different idea that had to do with the way a material can be fabricated to raise the temperature at which it would be superconductive. It had to do with phonon damping. His idea was that if you could have a conductive metal like tin drawn to an incredibly fine fibril, as he called it, maybe 10 or 20 angstroms in diameter, surrounded by an insulating material like glass, then the superconductive temperature of the tin would be increased. He talked with me about some ideas that he had in how to make this material, which he believed would be important in supporting the patent application that he wanted to file. His goal, I think, was to generate a stable revenue source for the Institute. He wasn't interested in acquiring wealth on his own, but he was interested in looking for revenue sources for the Institute, which had lurched from one financial crisis to another throughout the years. He thought that this might be something that could be licensed and would generate lots of income.

So Zelek Herman and I set up a laboratory at Stanford Industrial Park at a leased facility that the Institute had on Porter Drive that also housed the Sasakawa Aging Research Facility and some storage area. We set up a laboratory, bought an oven, some torches—blowtorches—compressed gas, borosilicate glass, and tin. Zeke had developed a very interesting way to collect the material that we were going to produce—a bicycle inverted on a block of wood. He had taken the rubber tire off the bike rim and we wanted to collect this glass-clad fibril that we would generate with torches above the furnace on the bicycle wheel, using the pedals to turn the rim. We would then collect a long piece of glass that was filled with tin. There was a lot of trial and error involved. Ewan Cameron developed a special interest in this, as well, so Ewan and Zelek, and I spent quite a lot of time in this laboratory at Porter Drive trying to produce this material.

And it was interesting in another way. Pauling was very interested in seeing the laboratory notebooks and wanted to be sure that we were taking copious and meticulous notes on what we were doing. The laboratory notebook page, for instance, had to be dated and then periodically someone who was not associated with the project was asked to read what had been put in the laboratory notebook and countersign it. We asked Emile Zuckerkandl, who was the president of the Institute at that time, to read what we'd written in the lab notebook. He would countersign it and date it, as well, which made me aware of how important it is to maintain good protocol in laboratory notebook entries. You don't erase things; you cross them out. Glue or tape things in, use an ink pen, and date. So that people who are unfamiliar with your work would be able to go back and repeat what you'd done based only on the information provided in the lab notebook.

Linus Pauling was quite happy with it. After months of trial and error and a sophisticated development of this technique and examination of the product through high power magnification, we were able to come up with a material that we thought would be suitable for testing. Pauling was just ecstatic. At that time he was in an apartment in Stanford and Zeke and I were very excited. We brought a sample of this to his apartment, and he was very excited. I can imagine that he

exhibited this emotion many times throughout his life whenever he made an important scientific discovery. He just looked joyful and immediately went over to the mantle over the fireplace and got a big cubic pyrite crystal that he gave to me and said 'be sure to bring your copies of *General Chemistry* in and I'll inscribe them on the occasion of this discovery and invention that we've made.' That work formed the basis for his patent application. I believe his patent for the novel technique for fabricating superconductive material was awarded in 1992. I think there were initial applications submitted in 1988 and 1989, but the final patent was granted in 1992.

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**CP:** Was that technique ever employed on any level?

**SL:** Well, it turned out to be very difficult to measure changes in superconductivity in this material because getting an appropriate contact at the end of this bundle of tin fibrils that was maybe ten to twenty angstroms in diameter was extremely difficult. We sent the material off to General Electric in Schenectady. Pauling had contacted a number of his friends in the industry to find out if they had equipment that might be capable of making these measurements for us, and there were problems in actually making the measurements. The Vice President of the Institute, Rick Hicks, felt that since we had the patent in hand, we should try to license it. So he embarked on a process of trying to identify companies in the United States that might be interested in licensing this technology. He wasn't having much success in this country.

He went to Japan and tried to shop it around there also without success. Some months had gone by. Emile Zuckerkandl called me on the intercom if I would come down to his office because we had a visitor. I went down to his office and we had someone there from the Central Intelligence Agency. He was very interested in this particular invention and wanted to know why we were shopping it in Japan and other countries rather than the United States and I explained to him that, well, the reason was, we hadn't been able to find anyone in the United States who was interested. I thought it was quite interesting that the CIA seemed to know about this.

**CP:** And that was the end of that?

**SL:** That was pretty much the end of that. I think there was a miscue later that had to do with the turmoil surrounding Pauling's death and succession of people in charge in the Institute, files being shifted around, and so forth. Somehow the application to continue the maintenance fee on that particular patent fell through the cracks, and it died because the maintenance fee was not paid.

**CP:** Were any other scientific collaborations of note?

**SL:** Not really. The vitamin C and skin cancer experiments and the superconductive work were the two major collaborations.

**CP:** How would you describe Pauling's style as a scientist and what did you observe as being his greatest strengths as a scientist?

**SL:** Well he had an encyclopedic knowledge of physics, chemistry, biology, mathematics, statistics—he had an incredibly good grasp of statistics that served him very well. He was, as I like to say, perhaps the best embodiment of the multidisciplinary or interdisciplinary attack on scientific problems. But it was remarkable that this kind of encyclopedic knowledge was exhibited in one person, Linus Pauling. His memory his ability to extract references, facts, and material from other papers that spanned most of his life was quite remarkable. I think those two things served him incredibly well as a scientist. I saw that in action many times, when he could cite other literature and talk about what other people had found decades earlier, so I was very impressed by his grasp of science and his memory.

As I mentioned earlier, he was very courageous. I witnessed this in the political arena and in science. When Zelek and I were working with this furnace in the late 1980s at Porter Drive, Pauling would come up from time to time. One time, he put on a lab coat, gloves, and goggles and wielded the torch above the 800 degree furnace. So he was actively engaged in the experimental project in his late 80s, which I thought was quite extraordinary. He didn't only sit at his desk and think about things. He also wanted to get physically involved when something really intrigued him.

During the mouse skin cancer experiments, he would often come through the laboratory just to check on things, see how things were going, and ask people questions about the work that was under way. He was not removed from the experimental side of things. He wasn't a stranger in the laboratory. I witnessed that first hand with the work on superconductivity, where he actually was engaged experimentally. I think that was probably the last experimental work in which he was engaged.

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**CP:** Over time, your role at the Institute changed from that of a scientific researcher to more of an administrator and I'm wondering if you engaged in other different kinds of work with Pauling in that context as an administrator?

**SL:** Very much so. I had been asked to become assistant to the President when Emile Zuckerkandl was President of the Institute. At the time, I was co-director with Emile of the laboratory for research in gene regulation, and we had a small research group. We were looking at problems in cancer metastasis using protein profiling techniques that had just recently been developed. We were developing instrumentation that would allow us to increase reproducibility and reduce noise in the experimental procedure. We were very interested in discovering genes and proteins that might correlate with the development of the metastatic phenotype in both murine and human cancer cells.

So I was working administratively with Emile Zuckerkandl at that time and then when Emile retired from the Institute on the occasion of his birthday, there was quite a lot of turmoil at the Institute because there were a number of problems. One problem was that the Institute had a history of financial instability and over the years, under Emile's direction, the Institute had expanded quite significantly to include some projects that probably would not be considered strictly in the realm of orthomolecular medicine. I think Emile may have had desire for the Institute to become a more comprehensive institute of molecular-biological sciences similar to the Salk Institute in Southern California. That's a fine template, but there weren't enough grants or funding to support all the research activities.

So, concomitant with Emile's retirement and the retirement of Rick Hicks from the Institute, there was a refocus on orthomolecular medicine. The Board of Trustees decided that there really weren't enough funds to sustain all the programs under way at the Institute, so the idea was to pare back some of the strictly molecular-biological work that wasn't funded extramurally and refocus specifically on orthomolecular medicine.

At about that time, Linus Pauling had begun a friendship and scientific collaboration with the German cardiologist Matthias Rath who, together with Pauling, had developed some interesting hypotheses about the relationship of lipoprotein(a), which is a risk factor for atherosclerosis and a major constituent of atherosclerotic plaque, and vitamin C. They published a number of papers, a very significant one, in *PNAS*, and decided to set up a laboratory for cardiovascular disease research that was directed by Dr. Rath. And Pauling was very excited about this work. After Emile had retired and Pauling became, for a short time, the President and Director of the Institute, he asked me to become his assistant. I wasn't quite sure what that entailed, so I had written him a memo asking him what he wanted me to do. I had some ideas, mainly of an administrative nature, that I thought could improve the way the Institute was run and our status—financial status and other issues. But he disappeared to Big Sur for some time.

I felt a little adrift, but by this time, I had gotten to know Linus Pauling, Jr. fairly well because he was in the process of getting more actively involved in the administration of the Institute. He recognized his father was ill and that there were a lot of problems at the Institute, and he felt he should step in to help his father, which I thought was very commendable. Eventually, I became the Executive Officer at the Institute, which was similar in function to being President of the Institute and then the board appointed me Chief Executive Officer in about 1993. So throughout this period, where I functioned in different administrative roles, I worked very closely with Linus Pauling, Jr., the Board of Trustees, and of course, Linus Pauling, who had become chairman of the Board of Trustees and Director of Research at the Institute. He didn't really have an administrative role any longer because everyone felt that it would be best if he were spared the problems that we had to resolve.

There was a zoning issue—the leased property on which our building was situated at 440 Page Mill Road in Palo Alto was scheduled to change from commercial to residential zoning quite quickly and no long-range plans had really been developed to deal with that zoning change.



We didn't have an endowment, had very little money, so the prospect of moving the Institute, trying to keep it intact in the mid-peninsula but paying maybe ten or twenty times more in rent, was not very encouraging. Linus Pauling himself, of course, had been diagnosed with prostate cancer and although he was undergoing a number of different experimental and conventional therapies, it was not believed that he would survive his illness. We were very concerned about what would happen to the Institute after his death. Would we be able to continue to raise money to support the Institute as a viable research organization?

And then, of course, there were lawsuits. There was quite a lot of litigation at that time. Matthias Rath had left the Institute and sued for interference in his business activities. I had to manage a lot of our litigation at that time, as well. At the time, the big challenges were finding a future home for the Linus Pauling Institute of Science and Medicine, keeping it intact and reputable, and also managing our litigation which was very burdensome and took a lot of my time and a lot of the Board's time as well. But Pauling was largely spared from that. He was deposed, of course, a number of times, but I managed a lot of our legal affairs at that time in close association with Linus Pauling, Jr., who was the Chairman of the Board. We had very regular board meetings so the Board of Trustees was kept well apprised of all the activities. I was surprised, when the Board asked me to become Chief Executive Officer because it was so unexpected.

The work that I was doing with Emile Zuckerkandl was not really related to orthomolecular medicine. So I was concerned that my group and my job were in jeopardy as well because there were rumors that the Board wanted to refocus on orthomolecular medicine. I was concerned perhaps I might be laid off from the Institute, too. Emile Zuckerkandl wanted to collect some of the people who were doing work in molecular biology a new institute called The Institute of Molecular Medical Sciences, and he hoped to rent space from the Linus Pauling Institute of Science and Medicine in the same building. I spent quite a lot of time with Zelek Herman and Stephen Maddox, our fund-raiser, analyzing that proposition, trying to figure out whether it was practical and if so, how building expenses should be shared between the two groups.

[0:50:37]

**CP:** One of Pauling's sort of 'calling cards' at Cal Tech any way—during the golden years at Cal Tech—was his abilities to really collect and martial and oversee tremendous talent of scientific researchers, other graduate students or post-docs and I'm wondering if you observed this sort of dynamic going on over the course of your time at LPI with his ability to kind of collect talent and make it efficient and productive?

**SL:** Well, I think Art Robinson is a terrific scientist, and Art, of course, was working very closely with Linus Pauling at the University of California San Diego and then came up to run his lab and collaborate at Stanford. There's no question that Dr. Robinson was an extraordinarily capable scientist and, with Pauling, developed lots of very interesting ideas. A big schism developed between them in the late 1970s, which was very painful to watch. I was not really privy to a lot of the major administrative decisions at the Institute, so it was a little unclear why that conflict had developed, but it was a very difficult and uncomfortable time for everyone.

Bill Aberth, a physicist and extraordinarily talented individual, was developing the mass spectrometer for metabolite profiling. Emile Zuckerkandl, who had developed the idea of chemical paleogenetics or molecular evolution with Pauling earlier in the 1960s is an extraordinarily bright, capable scientist. He was also the founding editor of *The Journal of Molecular Evolution* and is one of those rare individuals who is capable of sustaining simultaneously many complex thoughts. I saw this in action many times. I have tremendous respect and admiration for Emile Zuckerkandl. He's really an extraordinary scientist and theorist. So yes, there were people of extraordinarily high caliber involved in the Institute.

And, of course, there were extramural collaborations, as well and some of Pauling's collaborators would visit the Institute from time to time. Despite the lack of stable financial resources or an endowment, there were high caliber scientists who were employed at the Institute: John Leavitt, Jim Fleming, Greg Spicer, Ewan Cameron as Medical Director.

But it was difficult because contracts would not be offered to people. It was financially imprudent to offer employment contracts, so there was an implied at-will employment arrangement with people until the early 1990s, when it became an explicit at-will employment relationship.

Nevertheless, there were people who were attracted to the Institute because of Linus Pauling. Dr. Connie Tsao did a lot of really terrific work on vitamin C. There were a number of really excellent people there—physicians and scientists who

did a lot of very good work over the years. In those days, it was very difficult to get funding from the National Institutes of Health or the National Cancer Institute for the kind of vitamin C research that Pauling was especially interested in. You are aware of his difficulty in getting grants to carry out this kind of research. Pauling and Cameron went to the NCI with their clinical results with vitamin C repeatedly, and NCI suggested that they apply for funds to do animal work and cell culture work. They applied for those funds and were turned down repeatedly over seven or eight consecutive years. The money that supported that kind of work at the Institute was sought from the public through the direct mail solicitation campaign.

[0:55:42]

**CP:** Pauling is remembered also, fairly famously, as being a terrific lecturer. I wonder if you could comment on that and your experiences seeing him in lecture and his facility in that regard.

**SL:** I didn't see him lecture very often, but I saw him give lectures at Stanford University. He would give the famous vitamin C lecture where he would stand on stage and pull from his three-piece suit a vial, a test tube with a very small amount of a white powder at the bottom. He would say: 'This represents the RDA for vitamin C. This is the amount of vitamin C that the Food and Nutrition Board believes that people should get every day.' And he would follow that by pulling out a test tube with 13 grams of vitamin C and say: 'This is the amount of vitamin C that a goat synthesizes every day. I think goats know more about nutrition than the Food and Nutrition Board.' And that always got a big round of applause and laughter from the audience.

One lecture that really stands out in my mind was a departmental lecture in Biostatistics at Stanford. That department was chaired by Byron Brown, who was a leading biostatistician. The Stanford Department of Biostatistics is world renowned. Byron Brown was the father of a classmate of mine from Stanford, so I knew him. He had become aware of Pauling's work with Abram Hoffer on a regimen provided adjunctively to cancer patients that included not only vitamin C, but also B vitamins, vitamin E, zinc, and some other substances. Pauling and Hoffer had published two papers on this regimen, and Pauling believed that it was even more effective than the sole use of vitamin C, which was given either intravenously or orally, sometimes both, by Ewan Cameron in Scotland.

Pauling had developed a statistical analysis of that data based on work done by an old friend of his at UC Berkeley by the name of Hardin Jones. Pauling believed that this statistical analysis was better than the standard Kaplan-Meier mortality analysis because with Hardin Jones' analysis you could actually resolve subcohorts that had similar death rates. You might be able to learn more about these subcohorts with similar death rates. He and Zelek Herman published a paper in PNAS on the Hardin Jones analysis of mortality data. He was invited by Byron Brown to give a lecture to the biostatisticians at Stanford. At the time, everybody was using Kaplan-Meier, which is the standard-step staircase type curve that you see most often in papers.

I went to that lecture, which was in a large conference room at Stanford. It was astonishing how little criticism he got from the audience. No one had any serious criticisms of his analysis and said that well perhaps it should be looked at more carefully and perhaps offers a better alternative to Kaplan-Meier analysis in some circumstances. It was quite extraordinary to see some of the world's leading biostatisticians react to something novel so positively. It was quite interesting to see him present that analysis.

[1:00:30]

**CP:** I wonder if you could speak a little bit more about Pauling as a mentor as well, perhaps to yourself or to others that you saw.

**SL:** He was very encouraging, and as I said, he was not aloof. He was a very easy person to speak with. Everyone was extraordinarily respectful to Linus Pauling. No one at the Institute called him by his first name, for instance. It was always Professor Pauling or Dr. Pauling. Even Dorothy Munro, his secretary and assistant, called him Dr. Pauling, and Zelek Herman called him Professor Pauling. Emile Zuckerkandl called him Dr. Pauling, as did Rick Hicks. I think that was a measure of respect that all the people at the Institute had for him. Sometimes he invited me into his apartment for a vodka—typically Stolichnaya.

Pauling was very interested in people. He had decided not to drive anymore from his apartment of Stanford to the Institute, which was just a mile or two away. People volunteered to drive him from his apartment to the Institute and then take him back to his apartment at the end of the day. I did that quite often because I enjoyed talking with him during an uninterrupted period where he was free from telephone calls and appointments and other distractions. It was rare to have an occasion to talk with him with no distractions. He would occasionally ask me about my long-term aspirations and make suggestions, and he would often tell me stories. He loved to tell stories about things that had happened to him.

During one of these trips when I was taking him to the Institute, he told me that he had been over at the Stanford library to look at an old issue of *Nature* or *Science* from close to the early part of the twentieth century, when David Starr Jordan was President of Stanford University and Leland Stanford was Chairman of the Board. David Starr Jordan was an ichthyologist who had written books on fish. Pauling had been looking through an old issue of *Nature* for a specific article, but this other story caught his eye that had to do with the controversy that had developed at Stanford between David Starr Jordan and Leland Stanford. Leland Stanford, apparently, had become unhappy with David Starr Jordan because during trustees meetings, Jordan did not refer to the trustees by name. Stanford thought that this was not civil and perhaps even rude. He had implored Jordan to learn the names of the trustees and in subsequent meetings refer to them by name. Jordan's retort to Stanford was: 'Well I'm afraid if I am forced to memorize the name of a trustee I'll forget the name of a fish.' Pauling thought that was hilarious. It gives you an idea of what he valued. I think he was definitely on the side of Jordan in that argument.

**CP:** That's funny. Do you recall any other stories from the drive time?

**SL:** Oh, there were lots of other stories. When I was negotiating a contract with Unilever and Elizabeth Arden, who had decided to develop a line of cosmetics that included some antioxidants and wanted to get the Institute to get involved—not in a research and development capacity, but to support some research. I talked with Linus Pauling about this prospect he remembered one time that he and Ava Helen had gone to visit Lord Lever in England. They had brought along a bottle of Scotch whiskey as a present. It was in their bag, and they got to Lord Lever's home, and they were met by servants who immediately took their bags. The Paulings were ushered in to see Lord Lever. Pauling and his wife were chatting with him, and Pauling remembered that he had a gift, so he wanted to excuse himself and go to his room to get the gift. He went up to his room, and the servants had already unpacked their bags and put clothes in the drawers or in the closet. They'd also taken out the bottle of whiskey and uncapped it and had two jiggers there ready for Pauling and his wife because they believed Pauling had brought this along just for themselves. So he thought that it was really funny that he had to present Lord Lever with a bottle of whiskey that had already been opened. But he explained to him why it had been opened.

Another story that I think is quite indicative of Pauling's character was told to me by Ewan Cameron. Cameron, of course, had been collaborating very closely with Pauling, and Pauling had decided to visit Cameron at his home in Loch Lomondside on a trip that he was making to England. Cameron told me that he was a little bit ambivalent about this visit by 'the great man of science' to his humble home in Scotland, and he was worried that it wouldn't be presentable enough. So he and his wife made elaborate preparations to welcome Linus Pauling and his wife to their home. Half an hour before Pauling arrived, the toilet broke and Cameron hadn't had time to do anything about it. Pauling and his wife arrived and they came in and pleasantries were exchanged and Cameron showed them around the home and the view and so forth. Pauling asked to use the bathroom, and Cameron said 'I'm very, very sorry but there seems to be a problem with the toilet.' And Pauling said, 'What's the problem?' And Cameron explained to him that he hadn't had time to do anything. Pauling said, 'well let's go in and fix it.' So Cameron was a bit aghast but the two of them went into the bathroom. Pauling got on the floor with a wrench, and Cameron had the plumber's helper. Cameron realized that he didn't really need to be too careful about what he said or how Pauling was treated after that because he recognized that Pauling was not an aloof, cerebral person who didn't get his hands dirty.

[1:08:25]

**CP:** That's funny. You mentioned one of your—maybe the first experience of Pauling that you had was of him delivering a talk related to peace and politics and I'm wondering if you could reflect a little bit more on what you saw out of him in terms of his peace activism and his political work in the course of the time you knew him?

**SL:** I was very much opposed to the Vietnam War. My brother was killed there as a fighter pilot in 1970, which almost interfered with me resuming my studies at Stanford because it was emotionally a very difficult time. I had never understood the logic of the war. I didn't buy that domino theory of communism. It seemed to me that the war had been going on for a very long time and was resulting in a lot of misery. I couldn't see how it was constructive. I couldn't see a good outcome. So I think I had sentiments that were very similar to those of Linus Pauling.

And Pauling, as I learned, had been an old hand in political activism, originally stimulated by his concern about proliferation of nuclear weapons and medical concerns about the effect of widespread fallout from atomic bomb testing in the atmosphere, and that really resonated very strongly with me. There were some people at the Institute who were even further to the left. I consider myself in some ways conservative and many ways liberal, but there were some people at the Institute who were, I would say, probably extreme leftists. I remember after one of Pauling's talks at Stanford, there was some unhappiness among some people because Pauling had made the statement during this talk that he believed that the retention of some nuclear weapons by both the Soviet Union and the United States and the so-called 'mutually assured destruction' principle had probably served as a deterrent from having a nuclear war. They were surprised to hear him say that because they had hoped that he would espouse the opinion that we should destroy all weapons unilaterally, but that's not what he said in his lecture. He said that he believed that having some of these weapons by opposing forces had probably resulted in a satisfactory stalemate because either side was inhibited from using them, knowing what would result from that first use of an atomic weapon.

**CP:** Right. That was an opinion that I think he developed pretty early on maybe in the sixties—late fifties early sixties, as I recall...

**SL:** I would say he wasn't a pacifist because he said many times that World War II was well justified. He worked closely with the military to develop armaments and instruments that helped the Allied effort during World War II, and he was certainly opposed to Hitler and Nazism. I always saw him as a great patriot, and it never occurred to me that patriotism and opposition to the Vietnam War or opposition to atmospheric testing of nuclear weapons couldn't be reconciled by some people. They didn't seem to be mutually exclusive to me.

**CP:** Were politics something that came up around the Institute very often? Did he talk about that with people in casual conversation or with his colleagues or was it primarily more business oriented?

**SL:** Pauling was not someone who engaged often in small talk. He wasn't someone who liked to talk about pop culture, movies, music, literature, or anything of that nature. He loved to talk about science and especially about things that he was interested in. He loved to tell stories, and most of the stories were related to things that interested him scientifically. So I don't recall ever talking with him specifically about any political activities and, unfortunately, I never thought to ask him more closely about some of his political activities. We were always talking about current things or what we were going to be doing in the future. But no, he didn't generally talk about political issues with staff. Conversation was mainly related to administrative and scientific issues.

**CP:** Well, what was he like in larger social settings?

[1:13:49]

**SL:** Well, in larger social settings, he was often the person to whom people gravitated. He had a charismatic, magnetic personality. He always had a big smile. He was very engaging, and very warmly greeted people in a social setting. He shook hands, put his arm around people. He was very engaging, and people responded to that. That's why so many people loved him over the years. And when he died in 1994, the Institute collected hundreds and hundreds of cards from people that didn't know Linus Pauling personally but had been deeply affected by his death and were grieving and sharing their grief with us. Everyone at the Institute felt the same way too. When he died in 1994, it was a very emotional moment for all of us because we loved him and respected him.

He was sometimes a very jovial person. He treated everyone fairly and equally. I remember one time when I was in the back parking lot of the Institute, Pauling was leaving the back door and the janitor was arriving. It was after hours—maybe 5:30 or 6:00 in the afternoon. The janitor was quite a ways from the door—maybe a hundred feet—and Pauling stood there holding the door open for probably three or four minutes for the janitor. And then Pauling greeted him. He

was also holding a big box because he often used a box to ferry books and papers back and forth from the Institute to his apartment or ranch. So he had something in his arms, and he stood there holding the door for the janitor. And I think that was kind of symbolic of the way he treated people. He was just very fair and courteous to everyone.

Sometimes, I think, his excitement about scientific ideas caused him perhaps to overlook what issues some other people might consider problematic personality issues. And I think that this happened to him at least twice in his later years at the Institute. Both times it resulted in personal clashes, very unamicable departures of personnel from the Institute and litigation, which was very difficult, expensive, and painful for everyone involved. But I think he was so excited by scientific ideas that he tended to overlook some personality characteristics from time to time. But he was a very warm, engaging person. I went to birthday celebrations that occurred every five years, typically at a hotel in San Francisco. Pauling was always very charming and people were very happy to see him. He always had a big smile and greeting for everyone.

**CP:** Any specific memories about those birthday celebrations?

**SL:** Well, the birthday celebrations were terrific. The last one I remember was at Stanford Court Hotel in San Francisco. The Mistress of Ceremonies was Rita Moreno. I believe she was approached by Rick Hicks to host that party because she had gotten an Emmy, an Obie, a Tony, and an Academy Award. So Rick thought it was appropriate that someone who had been so honored in so many of the performing fields would host an event for someone who had been so honored in so many medical and scientific and social fields like Linus Pauling. She was a real firecracker, and I think Linus Pauling really enjoyed listening to her talk. He had a lot of his old friends there. He always really enjoyed interacting with old colleagues.

We also had the twentieth anniversary celebration of the Linus Pauling Institute in 1993 at the Rengstorff House, which is a Victorian house that had been renovated in the salt marshes of Mountain View, California, adjacent to Palo Alto. That was an opportunity to display a lot of Pauling's awards, medals, honorary degrees, and also invite supporters of the Institute, Institute staff and friends, and Pauling's colleagues and friends who were still in the Bay Area. It was probably the last time that some of these people had an opportunity to meet him, to see him again. Owen Chamberlain was there. Harden McConnell from Stanford chemistry department had been a long friend of Pauling's and, I think, instrumental in bringing Pauling to Stanford back in 1969 was there. Byron Brown from Biostatistics was there. Pauling was happy to see old friends. Pauling still had a very interesting way of speaking and a very characteristic way of moving, as well, both of which were captured quite authentically by John Astin when he performed the part of Linus Pauling in "The Nature of the Marital Bond," which was a play put on in association with the Linus Pauling Exhibition years ago.

[1:20:03]

**CP:** Sort of on that note, outside of Lloyd Jeffress and Ava Helen, we don't really have a good sense of who Pauling's friends really were. Do you—can you comment on that? In the time that you knew him, who would you say were his friends?

**SL:** Well, he spent quite a lot of time with Rick Hicks and Emile Zuckerkandl. Rick really went out of his way to take care of Linus Pauling, especially after Ava Helen died. He really looked after his welfare and his personal health, as well—I think even tried to get Linus Pauling to exercise, which is something that Pauling refused to do. He talked quite often with Emile Zuckerkandl, mainly about administrative issues.

He saw quite a lot of Harden McConnell, who was someone he could talk about chemistry with, and Henry Taube, as well, at Stanford. Zelek Herman was a colleague who Pauling could talk with about theoretical chemistry and structural chemistry and physics. Herman and Pauling co-authored quite a few papers during the 1980s and 1990s. I think Pauling relied on him to do a lot of the—heavy work in many of those papers. So he spent a lot of time with Zelek Herman. There were a lot of people who visited him who he'd known throughout his life, but he didn't really seem to have what I would call a strong social life.

Of course, if he was entertaining people at his home in Portola Valley or at his apartment in Stanford, I wouldn't necessarily have known about that. I went to his home in Portola Valley a number of times. He and his wife used to host

Christmas parties, and one of the last Christmas parties I went to there I think was probably in 1979. Ava Helen then died in 1981.

I remember thinking about the eggnog that Pauling made, which was extraordinarily good. He wasn't a chef, but having a Nobel Prize winning-chemist make the eggnog and spike it with vodka was pretty extraordinary. It was a really terrific treat. When he was with his wife, he was very affectionate. After his wife died, of course, there was no one for him to be that affectionate with so he was a bit different in social situations.

**CP:** Do you have any other recollections of Ava Helen?

**SL:** She was small in stature. She was very well-spoken, very articulate. I enjoyed speaking with her. She and I did not talk about science much as we talked about other issues. She, as I found out later, was particularly fond of their association with some of the celebrities in Los Angeles and Hollywood during the era in which Pauling was trying to educate them about radioactive isotopes and nuclear weapons. We talked—I think, talked about those things sometimes, but typically she was very close to her husband physically whenever they were in a social environment and he always liked to steer the conversation to science. So the conversation would typically end up being about science and not so much about some other activities.

One other thing about Pauling that I enjoyed was when I found out that he enjoyed reading mystery novels. I was a big fan of Sherlock Holmes, not so much Agatha Christie, but also Rex Stout. I asked him if he had read all of Sherlock Holmes and Rex Stout and, of course, he had. There wasn't anything by Stout I could loan him, but there were other books that he was not familiar with. One was called *The Red Right Hand* by Joel Townsley Rogers which I think was written in 1945. It's a great, hard-boiled detective story. I loaned that to him, and he really enjoyed it. He also loaned me mystery books that he had finished reading.

One time, he broke from that orthodoxy and loaned me a book called *Good Night, Ava* by Richard Bissell. This was a 1950s pulp paperback with a painted cover. He said 'I think you'll enjoy this.' I read it and it reminded me a little bit of Erskine Caldwell, even Mark Twain. The cover of the book showed a desultory group sitting on a porch of a ramshackle cabin on the banks of the river. There were a couple of guys lounging around, looked like ne'er-do-wells. There was a woman strumming guitar with not much on. She had her foot up on a barrel I thought it was amusing that he had loaned me this book. It was actually an entertaining read. When I gave it back to him, his eyes kind of sparkled a bit and he said 'Did you notice anything particularly funny about the book?', and I said yes. Both of us burst out laughing. He had taken a photograph of his wife and cut out her face and pasted it over the face of the woman playing guitar on the cover of the book. I thought that was very funny, and very appropriate, since the book had Ava in the title.

[1:26:53]

**CP:** We have that book now.

**SL:** I'm sure you do. Pauling had a terrific sense of humor. Zelek Herman likes to tell the story of the time that he was standing at the urinal in the men's room at the Linus Pauling Institute of Science and Medicine. Linus Pauling came in and stood next to him and said 'Ah, we meet as peers.'

**CP:** That's funny.

**SL:** He was not prudish. And, of course, Ken Hedberg has a story of when Ken was a graduate student at Caltech and was looking through one of those little plastic view finders of a woman *au naturale* in a stream, standing on a rock. He was holding the viewfinder up to the light, and he heard Pauling's footsteps outside his office and thought 'uh-oh.' Pauling came in and wanted to see what the viewfinder picture was. He held the viewfinder up to the light and said, 'ah, basalt.' He'd identified the rock that she was standing on. He had a great sense of humor.

**CP:** Just backtracking briefly, you mentioned Ava Helen talking about the connections that they had with some of the Hollywood types. Do you recall any individuals that she was particularly fond about in her reminiscence about that?

**SL:** No I don't, but I remember a number of people who came to the Institute and to some of Pauling's birthday events and other events such as Eddie Albert. I gave a tour to Eddie Albert and his son, Eddie Albert, Jr., one time at the Institute. Carl Sagan came by for an event.

**CP:** Pauling, by all accounts, had a very difficult time dealing with Ava Helen's death. I'm wondering if you have memories of that time period and how that presented itself?

**SL:** Well, as I recall, he was gone quite a lot after her death. I think he took a trip to Oregon and visited some of his old boyhood haunts: his home, maybe the family cemetery, Condon, and Oregon State University. He didn't exhibit the same sort of levity that I was accustomed to. He was happy in the late 1970s when I saw him. There was a more somber demeanor that he exhibited after her death, and a lot of the sort of levity seemed to evaporate from him at that time. That slowly returned over the course of the 1980s and by the mid to late 1980s he seemed to be pretty much his old self with a good sense of humor. But there was a period there where he didn't seem to be very happy.

[1:30:22]