



An Oral History of the Linus Pauling Institute, November 8, 2011

Title

“Research at LPISM”

Date

November 8, 2011

Location

Valley Library, Oregon State University.

Summary

In interview 3, Lawson details his initial employment interview with the Linus Pauling Institute of Science and Medicine, describes the LPISM facility at that time, and reflects on the personal and professional characteristics of certain of its critical employees. Among those discussed are Arthur Robinson, Emile Zuckerkandl, Ewan Cameron and Frank Catchpool. Lawson also recalls Pauling's dispute over vitamin C and cancer with researchers at the Mayo Clinic; the funding model that was utilized by LPISM for most of its history; and the Institute's move to a new facility in 1980. Throughout the session, Lawson touches upon various research programs and funding initiatives that were undertaken by the Institute during the 1980s.

Interviewee

Steve Lawson

Interviewer

Chris Petersen

Website

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

Transcript

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

Chris Petersen: Could you introduce yourself and give today's date.

Steve Lawson: My name is Steve Lawson. It is the 8th of November, 2011. We're in a room in the Valley Library.

CP: So in our past interviews we've talked mainly about your relationship with Linus Pauling. What I want to start doing now, at the beginning of this interview and over the next set of interviews, we will be talking about the history of the Linus Pauling Institute of Science and Medicine. So I am wondering if you could recall anything about your initial interview at LPISM.

SL: After I graduated from college, I spent a fair number of years traveling peripatetically around the country, visiting family and friends, and finally came back to Palo Alto, California because I really, truly loved the Bay Area. At that time it was very exciting, terrific environment. I loved everything about California and in particular the Bay Area. Culturally, artistically, intellectually, very stimulating environment. So I wanted to try to remain in the Bay Area if possible, although I still at that point was not very career oriented. I didn't really have a very clear idea of what I wanted to do in the future.

I became aware of a listing for a position at the Linus Pauling Institute of Science and Medicine, which at that time was located at 2700 Sand Hill Road in Menlo Park just opposite Stanford Linear Accelerator, in an office building. Up the street from Addison-Wesley and a number of publishing houses that were located along that road at the time, on property that was owned by Tom Ford. So it was in 1977 that I interviewed for a position with the Institute, and the initial position was part time. And in February of 1978 it turned into a full-time, salaried position.

Initially, I interviewed with Corrine Gorham, I believe, who was secretary of the Institute and head of the HR department for whatever that was called at the time. Fairly small organization still. And I guess she was sufficiently impressed to ask me to come back to meet with Art Robinson, who was the President and Director of the Institute. And Art had recently, in 1976-1977, set up a program of direct mail solicitation, in order to bolster the financial base of the Institute because grants were very hard to come by and in particular the work on vitamin C that Linus Pauling felt so strongly about was very hard to fund. So they turned to the public for a funding source and that resulted in buying and trading mailing lists of magazines whose subscribers might be interested in the mission and the work of the Linus Pauling Institute. Magazines like *Prevention*, which in those days had a much less pharmaceutical orientation than it has today. It was kind of a companion publication to Rodale's *Organic Farming and Gardening Magazine*.

So I think Rodale Press had sort of an orthomolecular perspective on not just farming and gardening but also on health. In those days they emphasized nutrition and vitamin supplements and things like that, so that seemed like a very likely candidate for raising money to support research efforts at the Institute. And that program was very successful, so it resulted in a tremendous amount of mail and the processing of donations. The specific job that I interviewed for at that time was related to this direct mail program, processing the donations, and also initially some fairly rudimentary statistical analyses of the donations. Where they were coming from, the magnitude of donations, what mailing lists were succeeding, which were failing. So that was what I was doing initially.

CP: What was your impression of the Institute when you first arrived, the atmosphere there?

SL: Well, I thought the atmosphere at the Institute was quite collegial. The Institute shared that building on Sand Hill Road with Kemper Insurance so the Institute did not have access to all the rooms in the building. It was clear that what had been built and designed as an office building had been renovated to some extent to accommodate the Institute's research. So it wasn't really laid out optimally as a research facility but nevertheless I think they did a terrific job of shoe-horning the various research programs into the available space. [0:05:15]

CP: How much space are we talking about?

SL: Well, I can't remember how many square feet. I know that the next facility on 440 Page Mill Road was over 20,000 square feet, so this facility was less than 20,000 square feet available to the Institute. And there were programs in

metabolite profiling under way, as well as vitamin C and cancer and some other sundry studies that did not require a large laboratory facility. There was quite a lot of chemical work done on vitamin C done by Connie Tsao under Pauling's direction which didn't really require a lot of floor space in a laboratory. And there were also animal experiments that did require quite a lot of square footage to carry out. And there was, of course, the Volcano Source Field Ionization Mass Spectrometer, which was a lab unto itself, and that was being developed by Bill Aberth who was a physicist formerly at SRI, which was near the U.S. Geological Survey facility in Menlo Park, that originally had some relationship with Stanford University, but by then had become independent. That's where I learned to do cell culture work, when I set up the first cell culture facility at the Institute a while later.

I think that everyone seemed to be very friendly and very dedicated to the work they were doing. I was particularly struck by the dedication of Art Robinson and Rick Hicks, who was the Executive Vice President in charge of fund-raising. And Ewan Cameron, who would come by from time to time. He was a Non-Resident Fellow, and later became Medical Director of the Institute and moved to California. But in those days on Sand Hill Road, he was still, I believe, a Non-Resident Fellow carrying out the clinical collaboration with Linus Pauling in Scotland. And I would say that everyone was extremely enthusiastic and dedicated. It seemed to be a pretty cohesive group and that part of the ambience there really appealed to me. So as far as a working environment, it seemed to be quite good. It wasn't until a little later, until there was a schism between Art Robinson and Linus Pauling, when things tended to become awkward and soured a little.

CP: Was Robinson your supervisor at that point?

SL: I would say he was, yes. He actually hired me - I interviewed with him. I found him to be a very intense, motivated person. I liked him. I thought his personality was quite intense, but he seemed to be very bright, very sharp, very insightful, and I enjoyed working with him and for him in the direct mail program. As the schism developed between Robinson and Pauling and Robinson was no longer physically present in the Institute - I worked, as time permitted, in scientific operations. Alan Sheets was a biochemist who was doing work on vitamin C and also in the vitamin C and skin cancer experiments that were being overseen by Robinson and Pauling at the time. And Alan and I really hit it off and liked to talk about science. And, of course, I had some background in science at Stanford. They were sometimes under staffed in the scientific sector, and I would volunteer to do work with the animals in the laboratory. I think I had an aptitude for that work and some competence, as well. And that was recognized by Alan and Dick Willoughby and other people who were involved in that realm at the time, so gradually my responsibilities and my activities shifted from the direct mail campaign to working as a research assistant or research associate in the laboratory. [0:09:35]

CP: Emile Zuckerkandl arrived in February of 1977 at the Institute. What was his role at that time?

SL: Emile had been a Non-Resident Fellow of the Institute and then joined the Institute to work with Pauling and to continue independent work on the *Journal of Molecular Evolution*, which he founded. He had some pretty interesting ideas that were mainly theoretical in nature. So Emile didn't really need a laboratory to carry out his work at the time. Of course, he and Pauling had known each other for a very long time. Zuckerkandl, I believe, had been at Caltech and the two of them had developed this concept of chemical paleogenetics or molecular evolution in the late '50s, early '60s; it was published in the early 1960s. Francis Crick thought it was one of Pauling's major achievements and I think actually that most of it was probably due to Zuckerkandl. But nevertheless, together they developed this very interesting idea of dating the divergence of species based upon similarities in hemoglobin shared among very many mammalian species, vertebrate species.

Emile is a very interesting, very kind person. He is definitely European in nature and is extremely intelligent. Emile is one of the rare individuals who can hold many competitive ideas in his mind simultaneously and expound upon each of them in turn. So I found him to be a very challenging intellect, and I really enjoyed talking with him.

And Emile and I got along so well that Emile wanted me to do more and more work with him. This was after he became President and Director of the Institute after the departure of Art Robinson, and that led to kind of a long collaboration with Emile that culminated with my appointment as Co-Director of the Laboratory for Research and Gene Regulation in the mid-1980s. Emile at that time was very interested in cancer metastasis and what genes and proteins were involved in the development of that phenotype, because many people who suffer and die from cancer will die from metastatic involvement even if the primary tumor is surgically treated or treated successfully with radiation and chemotherapy. It is often not possible to kill all the metastatic cells that can move through the lymph system or through the blood system

and colonize other tissues. So Emile was very interested in that, and we set up a program at the Institute to use a technique called two-dimensional polyacrylamide gel electrophoresis to profile proteins present in tumor cells, metastatic tumor cells, and also normal tissue to look for proteins, and therefore genes, that might be involved in the development of this phenotype. And I think Emile had an idea at that time that these might be regulatory proteins, which would be present in very small amounts in cells.

So we had to set up a cell culture laboratory, which I did. I bought all the equipment, read a lot about tissue culture and what was important and what kind of equipment would be appropriate. I learned the techniques at SRI, also learned some techniques at UCSD and the University of Colorado in Boulder about defined media, because we were very interested in eliminating or reducing to the greatest extent possible the amount of noise in the system. And we wanted to increase reproducibility of our protein profiling, as well, because if regulatory proteins are involved in this process of metastatic cancer, then we need to be able to detect them. And to detect them, we needed to increase reproducibility and decrease noise in the system. So I spent quite a lot of time with my colleagues, Stuart McGuire, Eddie Metz, Mark Peck, and others in trying to perfect that system.

CP: So Zuckerkandl maintained this pretty active research program even though he was assuming more and more administrative work as well?

SL: He did, that's right. He did assume more and more administrative load. Pauling had plenty of aptitude for administration but was less interested in administration and really wanted to concentrate on his theoretical work, his clinical work, the vitamin C work, orthomolecular medical work. And Zuckerkandl, from a research perspective, was probably a little broader in what he wanted the Institute to become. And as his influence and administration increased and the funding available to the Institute increased, the focus of the Institute also broadened to be much more of a molecular biological approach to health and disease rather than specifically focused on orthomolecular medicine. For instance, the program that I was involved in, looking at gene regulation and cancer, really had nothing to do specifically with orthomolecular medicine or nutrition.

I think Emile had a vision for the Institute as something like the Salk institute in La Jolla or the institute that he had founded in France and left before he joined the Linus Pauling Institute of Science and Medicine, which was much more of a molecular biological approach to science. [0:15:34]

CP: You mentioned his intelligence, how else would you characterize his personality?

SL: Well, he was very kinetic; he is a very tall, lean person, had a physical energy as well as mental stamina and vigor and also has a very European attitude about things. I think in some ways that served him well and in some ways it may not have served him terribly well because the administration in the Institute became a little bit of a black box. And perhaps was not as transparent as it could have been that would have promoted better relations among employees later in the history of the Institute.

CP: And this is an extension of the European attitude thing?

SL: I think so. It's hard to say. I am not a psychologist, and I don't know that much about European political systems, but I think that Emile was kind of a benevolent king.

CP: At a more personal or social level do you have memorable interactions with Zuckerkandl that come to mind?

SL: Well I had many, many interactions with Emile over the years. He had a terrific sense of humor, not so much in a joke-telling way but a good sense of irony. We got along very, very well. And I think Emile depended upon me and Eddie Metz, who was another research associate, and a number of other people within his group to help him make decisions about hiring, for instance. There were a number of people who came to the Institute and went through a review process, candidates for certain positions, and Eddie Metz and myself and some others, I think, played pretty critical roles in those processes at the time, because I think Emile, rightly or wrongly, trusted my judgment about people. And I would give him very candid observations to the best extent I could about potential candidates for positions within the Institute.

We also shared an appreciation of art. Emile and his wife Jane lived in a penthouse apartment on Arastradero in Palo Alto and I was a relatively frequent guest. We would talk about art, talk about Emile's heritage, his ancestors - his

grandparents, his parents were all very famous in their respective fields in psychology, psychoanalysis, anatomy, art. And I remember one time Emile showed me his autograph book that he had maintained as a kid in his grandmother's salon. And he had signatures in this book from Sigmund Freud and a number of other very famous people, musicians and scientists. I can't recall exactly who now, but it was pretty impressive to see at the time.

CP: Was he a collector of art?

SL: I don't think he had the financial wherewithal to collect art but he had had art that he had inherited from the family, and Emile's family was Jewish and had lived in both Austria – Vienna - and France. And during the Nazi occupation and Nazi infiltration of countries in Europe, his family was forced to flee and often fled without their possessions. So I know many years later he showed me a catalog of German art and architecture in which his parent's chairs were featured. These were chairs that were part of the old Zuckerkanndl home that they had been forced to abandon when the Nazis arrived. And he had remnants of some art that he had been able to take with him, a Gustav Klimt triptych, which I believe he had given away actually. He did have some art and some books that managed to survive that ugly time in Europe. [0:20:09]

CP: Okay, well I want to talk now about Art Robinson and I am interested, first of all, in knowing what your sense of him as a scientist was when you initially met and interacted with him?

SL: Yeah, that's a good question. I would say that I did not have enough science acumen at that time to offer any judgment on Art Robinson. I knew that he was working very closely with Linus Pauling, so I assumed that he must be brilliant because I doubted that Linus Pauling would tolerate fools. And research was clearly the most important thing in Pauling's life - research and science - and I just imagined that Art Robinson must be a terrifically intelligent person to be working with Linus Pauling because otherwise he wouldn't be at the Institute. He seemed to have Pauling's trust, and many people had pretty high regard for Art Robinson, called him kind of the boy wonder. He was quite young at that time, maybe in his thirties, when he was President and Director of the Institute. He had come up from UCSD to take a position in Pauling's lab in Stanford and then together with Keene Dimick cofounded the Institute while the Institute was still located at Stanford. And then when they were able to secure a rental site in a building on Sand Hill Road, they moved the Institute and transferred the grants to the Sand Hill Road location.

I had a very favorable impression of Art. I liked him quite a lot, and he seemed to be incredibly driven. I didn't know at that time anything about his politics, his religion, or anything. I never asked him about those things, I never talked to him about those things, it was all strictly science. But I really enjoyed my interactions with him at that time. I was not high enough up in the food chain, the bureaucracy within the Institute, to really understand the nature of the conflict that developed between Art Robinson and Linus Pauling, so I was completely in the dark as to why that developed and why Art left the Institute. But there was a series of very bewildering memos at that time. I remember after Art left - and of course there was gossip about what was going on and who was in charge and what had happened and so forth. I can't really remember the gossip very clearly but I do remember coming into my office one day and finding a memo from Linus Pauling, a couple of short paragraphs, explaining that Art Robinson had absented himself from the Institute and was no longer serving as the President and Director and Linus Pauling was taking over that position and I should ignore directives from Art Robinson. That was followed the next day by a memo from Art Robinson to the effect that Linus Pauling had no authority to remove him from his position and that I should disregard the earlier memo from Linus Pauling. Things like that went back and forth for a while. So it was a period of tension, to say the least, at the Institute. People were a little bewildered as to what to do but ultimately, because Linus Pauling was chairman of the board and the Institute was named for him, people just inferred that Linus Pauling was in charge and therefore Linus Pauling was the one who had the authoritative say on what the status of Art Robinson was.

CP: So what do you think about Robinson as a personality? Intensity seems to be a word that comes to mind.

SL: Yes, he was very intense, very driven. No question about that.

CP: Any other adjectives that would suit him?

SL: Well, he had a very robust family life. His wife, Lauralee Robinson, at the time was working at the Institute in data acquisition and analysis from the metabolic profiling program, so she was there quite often. She was a very lovely person; I liked her very much as well. And together they seemed like a very good pair. It was quite a shock when she died so

suddenly. Art had small children that he had to turn his attention to and raise, and it just seemed like a very stressful time for everyone. The combination of Lauralee's very abrupt death, Art having these young kids to take care of now and also having this problem with Linus Pauling and essentially losing his job at the Institute. I do remember that prior to this schism with Linus Pauling, there had been several meetings in the Institute's library about the future of the Institute, and the prospect of moving to Oregon had been specifically raised by Art Robinson. [0:25:19]

CP: Was that the late 1970s?

SL: The late 1970s, Art had talked about property in Oregon, if my memory serves me. I think it was near Eugene or Junction City and might have been available at low cost for the Institute to buy and build a building and set up laboratories there. And I think that there was an informal poll taken among staff as to who might be interested in relocating to Oregon at that time; not many people were.

CP: Do you know any more about where this idea came from about Oregon?

SL: I don't. But I should say that when the schism between Pauling and Art developed, it occurred to me that that might have been one of the factors that led to this problem because Pauling had a very close relationship with friends and faculty at Stanford still - Harden McConnell, Henry Taube, a number of other people in the chemistry department there and elsewhere. He had homes in Portola Valley and Big Sur, and it didn't seem to me that he would want, at his age and station in life, to move to Oregon. And at that time I wondered whether that was part of the problem that had developed between them.

CP: So in hindsight, do you have any other thoughts on what led to the schism?

SL: Well, when I became Chief Executive Officer of the Linus Pauling Institute in the early 1990s, I was talking about the Institute before lots of groups quite often and consequently I had to familiarize myself pretty quickly with the history of the Institute and some of the major figures in the Institute. So I really had to learn a lot more about Linus Pauling, his career, than I had previously known. And I had to learn about Art Robinson and some of the activities that had transpired at the Institute over the years. So I looked at some of the depositions, I looked at the court papers that were now available to me as the Chief Executive Officer. I read that material quite carefully because I wanted to represent the story accurately and to the best of my ability. So I thought it was incumbent upon me to learn as much as I could about that problem that developed between Robinson and Pauling so that I would have a good understanding of it and I wouldn't simply be mouthing some other person's opinion of what had happened.

That led me to the conclusion that there were administrative problems that had developed between Robinson and Pauling. It didn't have anything to do with the results from the vitamin C and skin cancer experiments as far as I could tell, which Art Robinson portrayed as the whole problem. Art said that the anomalous results at low-dose supplemental vitamin C actually resulted in increased number of tumors in these hairless mice exposed to ultraviolet light. If you extrapolated that to people, people would be at risk for developing cancer, skin cancer in this case, at the doses of supplementary vitamin C that Pauling was recommending for good health. But there was a significant problem with that argument in that mice synthesize vitamin C. I believe even Art himself at the time had suggested that the exogenous vitamin C given in the diet to these mice might have affected their endogenous synthesis of vitamin C, especially at low dose. And Connie Tsao and a number of other people, Ping Leung, looked into that for Linus Pauling very carefully and found out that, sure enough, when you give them extra vitamin C, you actually decrease the amount of vitamin C in blood and various organs and tissues compared to mice getting no extra vitamin C. And you actually have to overcome that by giving much larger amounts of vitamin C, and then you start to see increases in vitamin C in these various tissues and organs and the blood, and also protection against the development of skin cancer caused by exposure to ultraviolet light. So it is not relevant to people; supplemental vitamin C doesn't interfere with anything we are doing in our livers, unlike the mice. So to me, that argument that Art put forward to explain this problem with Linus Pauling didn't seem to hold water. And I looked more carefully at some of the administrative decisions that were being made at that time, specifically with respect to Richard Hicks and affiliation with a group in Santa Cruz, the Hunsbergers, that Pauling was not very happy about. [0:30:45]

The Hunsbergers were very interested in wheat grass and abscisic acid as having cancer prevention or therapeutic properties. And some experiments along those lines were carried out at the Institute where wheat grass was being fed to mice to find out if it could protect them against cancer. But Pauling, I think - and I never talked to him about this but

inferring from what I read - it seemed to me that he was concerned about the somewhat fringe character of this group in Santa Cruz. He was worried about a close affiliation because the work that he was already advocating and trying to get funded seemed to be very controversial anyway. And I don't think he wanted to encourage even more skepticism about the Institute's activities by getting associated with fringe organizations.

CP: And Robinson had a different perspective?

SL: Robinson had a different perspective, right. And also I think Pauling had set up an executive committee composed of himself, Rick Hicks, and Art, maybe others, too. I believe that Art was asked not to make any important decisions without the approval of this executive committee. Art essentially ignored that and wanted to fire Rick Hicks. And Pauling liked Hicks and thought he was doing a good job. I think that the conflict that developed between them had to do with administrative issues, how to run and govern the Linus Pauling Institute of Science and Medicine, not scientific issues.

CP: Do you have a sense of what the conflict was between Robinson and Hicks?

SL: I don't really. I think that Robinson was unhappy with Hicks' performance and maybe Hicks had not done some things that Robinson expected him to do or asked him to do. They also had very different personalities, and there could have been a personality clash there as well.

CP: How did the legal wrangling play out? I mean, Robinson sued the Institute and that was a multi-year affair.

SL: It was a multi-year affair. It was very distracting to all involved, a very expensive proposition. There were lots of suits, of course, at that time. I didn't really involve myself in any of that because I had no administrative functions, so I was just happy doing my research and not worrying about what was going on in the legal realm or the administrative hierarchy of the Institute. But looking back at these documents retrospectively, it's clear that people were completely consumed with those legal issues because when these sorts of charges are made and suits are filled, you really have to do the best you can to protect the organization. That means hiring good attorneys and getting into the details of all the issues and countless depositions and document analyses, and it's all very tedious and very expensive. In a way, it's also quite fascinating because jurisprudence is a very interesting legal game that gets played, and it's interesting to see how the strategies evolve and how the players behave. But it can be incredibly distracting if your main goal is to carry out research.

Art ended up suing his attorneys because he was unhappy with the settlement that had eventually been reached. Much later, between the time I became Chief Executive Officer and Linus Pauling's death, Art Robinson reemerged. I had had nothing to do with him and hadn't heard from him or about him for many, many years, because after the settlement in the early 1980s, that was the end of the legal entanglement with Art Robinson. I knew that he had moved to Oregon to Cave Junction and set up his own research organization, I wasn't quite sure exactly what he was doing - this was long before the internet of course and it was difficult to ferret out details about people's activities, unlike today. [0:35:06]

So I really had no reason to think about Art Robinson until I got a phone call or a letter - well actually, it was letter, I believe, addressed to Linus Pauling from Art, that Pauling turned over to me that was a request for access to research data that Art had generated at the Institute before his departure. Art described this research data as being many five-drawer filing cabinets, maybe ten or fifteen of those with hard copy paper records that were generated as a result of the metabolic profiling program, and also very large magnetic data tapes that were maybe a foot in diameter. The terms of the settlement stipulated that none of that had to be given to Art Robinson. He had already been given an opportunity to copy his research data, albeit at high cost, before he left the Institute. He declined to do so at that time. But I thought that all these years had gone by, there was really nothing to be gained from withholding anything from Art if he was able to translate this raw data into scientific publications.

So I decided to look around and see what I could find that resembled what he had asked for. And in looking around the Institute I could find nothing that resembled the volume of material that he had requested. There weren't boxes and boxes or filing cabinets full of data. There weren't dozens or hundreds of magnetic data tapes; these things just didn't exist within the building. So I queried a number of people, including Dick Willoughby, who was our Facilities Manager; Emile Zuckerkandl, who had left the Institute at that time; and a number of other people who I thought might have some knowledge of whereabouts of this material, and no one really seemed to have a clear idea of what had happened to it.

But I reconstructed what I believed to be its disposition and sent this in a letter to Art Robinson in what I considered a very collegial and cordial letter. It was unhappy news for Art, but, nevertheless, it was, as far as I knew and as far as I still know, the truth about disposition of this material. The Institute had a satellite facility on Porter Drive in Stanford Industrial Park that was used for Sasakawa Aging Research carried out by Jim Fleming and others. Also, in the 1980s, we had carried out some work there on fabricating superconductive materials to support one of Pauling's patent applications.

We also used a number of rooms there for paper storage. We had had problems with the roof leaking in that facility that actually led to a lawsuit with the landlord, and a lot of the paper that had been stored in that facility was ruined because the roof leaked and the paper was damaged by water and just thrown out. And because it was damaged, there wasn't any attempt to really examine it carefully. I am reconstructing that, I don't know that for sure, but I do know because I did see it with my own eyes, a lot of paper destroyed by water. That was a reasonable explanation of what had happened to Art's research material. My understanding was that it had been stored up there since there wasn't any room to really keep it in the 440 Page Mill Road facility, if indeed it had been kept that long. If it was at Porter Drive, it was probably destroyed by a leaking roof that, as I said, led to litigation with the landlord because the landlord wasn't quick in repairing the roof, unfortunately. So Art did not take that news very well and in a way I can sympathize with him because that work represented a significant percent of his life's work, but that's the way it is. The material was gone, it was probably destroyed, and there was nothing to give him. But he was unwilling to accept that explanation and believed, for whatever reason obscure to me, that I must be hiding this material or lying to him. [0:40:03]

I think that he placed so much emphasis on the importance of this material that it was beyond comprehension to him that it could have been thrown away, even having been damaged. So he was unwilling to accept that explanation and that led to kind of a proxy lawsuit against the Institute by a colleague of his, Jane Orient, who I think is associated with him in Libertarian politics. She is a physician in Arizona, at least at the time, who had made, rather strategically, a small donation to the Institute and then later wanted access to research records. She called me on the telephone, and I had a nice conversation with her about research. At first I thought she was talking about the protein profiling work, and then I realized that she was talking about the metabolic profiling work. I talked about some of the work that had been ongoing until the mid-1980s and so forth and the development of the apparatus and the data collection and analysis and loss of funding. And then it became clear that what she was really interested in was this old Art Robinson material. I think at the time I told her the same thing that I told Art, which was I don't know what happened to it, we don't have it so I can't offer it to you. So that led to a suit that she filed against the Institute for access to this publically financed research data, and that of course involved a lot of time and energy and resources but eventually was dismissed by the court for lack of standing.

CP: Do you have a sense of - was there ever any reconciliation between Robinson and Pauling before Pauling died?

SL: Well, I think that Art's letter to Linus Pauling and Linus Pauling's response to him, if there was a response, were cordial. Art's letter was pretty cordial, so I don't know that they communicated much from the time the lawsuit finished until that letter in the early 1990s. My guess is that they probably did not because Art, on many occasions, had lots of bad things to say about the Institute and Linus Pauling. So I can't imagine that they really communicated much. And I don't know if that was Art's attempt at reconciliation or whether he viewed me being in charge of the Institute as an opportunity to get access to research data that he thought we had been sitting on for all those years.

CP: Okay, well we switch to Ewan Cameron. He arrived in 1978 in perhaps a full time capacity, I'm not totally sure, but you recall your first impressions of Cameron?

SL: Cameron was a wonderful person, he was very charming, he had a great sense of humor. He also had the Scottish brogue and was shy. He didn't like to talk in public and sometimes it was hard to understand what he was saying because of his accent and a tendency a bit to mumble sometimes. But he was very intelligent, very compassionate, clearly a research-minded physician who was interested in finding new ways to help his patients live with cancer. I think he and Pauling initially thought that vitamin C might be able to turn cancer into a manageable disease, a chronic disease rather than something that would kill you in short order. And that led him to consider vitamin C as having potential value in treating cancer patients. I really enjoyed him. I would go to parties with him quite often. He loved to drink single malt scotch, which he would bring from Scotland. Every time he went to Scotland, he came back with an armload of single malt scotches for everyone to try. And he liked to smoke cigarettes, too, which was a bit ironic. Sometimes it's the old 'do as I say but not as I do' motif you see with physicians or other people. Cameron was not pious, he didn't preach to people

about what they should or shouldn't do, and he was well aware of his own bad habit of smoking. It was hard to reconcile with his career as a cancer surgeon, removing diseased lungs and other tissue from patients, but nevertheless, he found it very difficult to quit. I know he tried to quit smoking a number of times but didn't succeed. I liked him because he had some weaknesses, but he was extremely bright and a very compassionate person. [0:45:23]

CP: And in these party settings he was more social?

SL: Yeah he was very social. Very, very social. My girlfriend and I loved his tales about his work in Burma. I remember one story he told about lancing a boil on an elephant, which was quite extraordinary. And he told me about the therapy available to the cohort of cancer patients he was treating, which was called Brompton's cocktail, a mixture of, I believe, alcohol, cocaine and heroin that Scottish surgeons and physicians very freely gave to their terminal cancer patients because they weren't concerned about these people becoming drug addicts. They just wanted to make the end of their lives as comfortable and pain free as possible.

CP: Did Pauling ever comment on Cameron to you in terms of his personal feelings towards him or his professional feelings?

SL: Pauling really liked Ewan Cameron; I think he had a very deep feeling for Ewan Cameron. And despite the fact that Cameron smoked, Pauling was apparently willing to completely overlook that, and Pauling felt very strongly about smoking. I remember seeing letters that were unopened in Dorothy Munro's office because they smelled of cigarette smoke and Pauling refused to handle them. That didn't seem to bother him so much about Ewan Cameron though and they were great colleagues and I think they enjoyed each other's company quite a lot. And Cameron regaled us with stories, my girlfriend and me, about Pauling's first visit to Scotland, I didn't mention this in the last talk did I?

CP: The toilet story?

SL: The toilet story, did I mention that?

CP: Yeah.

SL: Cameron was very down to earth and very much aware of Pauling's reputation and stature in the scientific field, too, so Cameron himself was a little bit in awe of Pauling. But Cameron had medical knowledge that, of course, Pauling didn't; so they provided kind of a nice complementary relationship for each other. And I think as a result of working with Cameron for so long, Pauling became even more medically minded. I know that Abram Hoffer, another of Pauling's old friends who initially got him interested in niacin and orthomolecular psychiatry back in the early 1960s, told me that he had been working hard towards the end of Pauling's life to get him an honorary medical degree, which apparently is possible in Canada - being part of the United Kingdom, you can appeal to the Archbishop of Canterbury through the Duke of Windsor or something like that to get a medical degree conferred upon someone. Of course it wouldn't allow them to practice medicine, but it was a very prestigious honor and Hoffer was working on those channels because he knew the English royalty that would help him with this process. But then he dropped it once Pauling died, of course.

CP: How about Cameron's family, did you know them at all?

SL: I did. I did not meet Cameron's first wife. I knew his second wife, Connie Cameron, quite well because she had been working for Rick Hicks at the Institute and that's how Ewan Cameron got to know her, through his visits and tenure at the Linus Pauling Institute. And she also is a very charming, very funny, very attractive person. Ewan had a son and a daughter, and he was always a bit concerned about his son because his son seemed to be having a bit of a hard time being independent, finding a career, being happy with his life and so forth. Ewan was also conflicted a bit in taking the position at the Institute in California because it would take him away from his children, and he was concerned that his children still needed some guidance. I met his children, and they are very nice, very affable, very intelligent people. [0:49:40]

CP: So how did Cameron's research program evolve over the course of his time at the Institute?

SL: Well, I would say that when he was physically at the Institute he, of course, was not treating patients in Scotland but was continuing to collect and analyze data from those cancer cases in Scotland and continuing to work with people who were doing that for him in Scotland, developing ways of analyzing the data, automated data collections, programs and

things of that nature. So he was very involved in that while he was at the Institute. He also became increasingly involved in what today is called outreach or public education. And as the work with Cameron and Pauling on vitamin C and cancer became more well known, there were many queries from the public, particularly patients afflicted with cancer who wanted advice: 'how much vitamin C should I take,' 'where can I go to get it' and so on. There had been a clinic operating at the Institute on Sand Hill Road for a very short period, but it became overburdened by poor patients very quickly, so that was abandoned. That was set up by John Catchpool and it didn't last very long. We were struggling at the time with giving sensible, practical, and non-medical advice to people because Cameron wasn't licensed to practice medicine in the United States so he couldn't give medical advice to people. We had to provide patients and their physicians with research papers and allude to responses of similar patients to a certain therapy and so forth. Cameron was working very hard to perfect our strategy of responding to the public for these health-related questions so that we didn't incur legal liability and make sure that we were acting ethically and morally and trying to be good citizens and compassionate. Many of the people who contacted the Institute in those days were in dire straits for whom traditional conventional therapy was not successful. They were just waiting to die and they weren't willing to accept that fate. So they were turning to other modalities, which included things like vitamin C. And once they started to consider that, naturally the tendency was to get into touch with the Institute and talk to Ewan Cameron. So he spent an enormous amount of time on the phone and writing letters to cancer patients and other people with questions about vitamin C.

CP: Well Cameron's and Pauling's research on cancer attracted a great deal of criticism. Do you have a sense of how they responded to that internally?

SL: Yeah, I think they felt - and this has been written about many times and I think you can infer their emotions from some of the letters that appeared in journals and elsewhere - that they had been treated badly by the Mayo Clinic. And both Cameron and Pauling were astonished to find out and angry that the Mayo Clinic researchers, Craigen and Moertel, had never even read Cameron's relevant papers. They were ostensibly charged by the NCI with replicating Cameron's work, but they knew virtually nothing about it. Had they read Cameron's papers they may have noticed that Cameron was giving high-dose vitamin C both orally and intravenously. And that, as it turns out, may have been a very crucial feature that was completely ignored by them because they had perhaps too much hubris to read this Scottish surgeon's papers on vitamin C and cancer. I think that the fact that the studies were published without the courtesy of showing the study before publication to Pauling and Cameron did not sit well with them because once the study was published, the media immediately wants responses the same day. It put you in an awkward position because Cameron and Pauling hadn't had the time to really read the study and think about it carefully and come up with a cogent analysis and critique of it. They were angered by that as well because they felt that professional courtesy was not extended to them in that regard. They felt that the work that the Mayo Clinic had done was, to use Pauling's words, somewhat fraudulent. And they were worried that this would end serious consideration of vitamin C as adjunctive therapy for cancer. They felt that that would be unethical, immoral, unscientific, and ill-conceived. They were upset and it was clear that they were upset. You know Cameron - neither Cameron nor Pauling revealed anger, they didn't shout or break things or storm around or anything like that. But voices were raised definitely and Cameron was not very pleased by the work that had been done by the Mayo Clinic, and it showed. [0:55:17]

CP: How about amongst the staff? What was the reaction to the criticism amongst the Institute staff?

SL: I think everyone was unhappy and angry as well. I think everyone shared the anger that these studies had been published and they weren't carefully done and there seemed to be lots of problems with them. They didn't replicate Cameron's protocol. I think there was also concern among the fund-raising staff that this might bode ill for continuing efforts to raise money to support research at the Institute. And sure enough, there were some fund-raising problems after that work was published. So it had a big impact on our work at the Institute as well as sort of shelving serious consideration of vitamin C and cancer for many, many years, which I think was a real disservice to the research community and to the huge population of cancer patients out there who might have benefited from this had the research been done more carefully early on.

CP: But the staff continued to believe in the science and believe in the validity of the idea?

SL: Yes, partly because the arguments that both Cameron and Pauling leveled against the Mayo Clinic researchers were very cogent and sensible. They weren't just fringe arguments that had no validity. They were valid arguments, and I think

that everyone was well aware of them. Cameron, I think, said one time at first he thought they were just dealing with fools and then he realized they were dealing with knaves, as he put it.

CP: You mentioned very briefly in 1978 the Institute set up a clinic for diagnosis and treating disease. You said that it didn't last very long; can you talk a little more about that?

SL: Well, it pre-dated my arrival at the Institute, so I don't really have a lot of firsthand knowledge about the clinic. I know that the concept there was clear, that some people might benefit from orthomolecular medical treatment, but I think the funding was too low and the liability issues were too great to really turn that into a practical, functional concern. I do remember Corrine Gorham and others telling me that there were people who showed up knocking on the door, deathly ill, had spent their last money on a bus ticket to get there and had no money and expected to be cured. That was not uncommon and, of course, was just not possible. So I think rather than risk having the tail wag the dog, they decided to dissolve the clinic rather quickly.

CP: Where was it?

SL: It was located within the building, as I understand.

CP: And Frank Catchpool was part of that?

SL: Frank Catchpool was part of that. I got to know him a little because even after the clinic finished he was still loosely involved with the Institute. I think he was practicing up until the end of his life in Marin County, maybe Sausalito, California. And he really liked Pauling, and I think Pauling really liked him quite a lot as well. He spoke at Pauling's memorial service at Stanford Memorial Church.

CP: Did you ever talk to him about his life? He lived quite an interesting life.

SL: I probably talked to him about his work in Africa, where I think Pauling first met him - Albert Schweitzer's camp there. I can't remember the discussions very clearly now though.

CP: We've talked about this kind of off and on throughout the interview, but I wonder if you could reflect a little bit more on the chronic money problems that the Institute faced for a very long time?

SL: Well, the Institute was, I think, founded and moved off the campus of Stanford University initially because there wasn't enough money nor was there enough space at Stanford to carry out the work that Pauling really thought would make a big impact on public health, namely research in orthomolecular medicine and specifically research on vitamin C. I think Pauling and Robinson felt that this had great potential, as did the metabolite profiling program. [0:59:52]

So they founded the Institute and moved to the location that we talked about in Menlo Park and then set up about trying to raise money to carry out this research. Government grants were not forthcoming for the vitamin C work, although the metabolic profiling work was supported to a large extent by government grants in those days. The development of the apparatus, data collections, and analysis, sample collections, things like that. As the Institute started to grow, the need for more financial resources became pretty clear and that's when Art Robinson and Rick Hicks got involved in this direct mail solicitation program because they correctly perceived that the public had responded to Pauling's message and thoughts about vitamin C and health much more quickly than the medical and scientific community. There were lots of people out there who believed that Pauling was right about vitamin C and wanted to support David versus Goliath, Pauling versus the conventional medical establishment. So they were willing to give small and large donations to support research at the Institute.

In a way that was very good because it allowed the Institute to hire staff scientists, without tenure of course, but to hire them and provide them with money to carry out research that was not dependent on government grants or corporate sponsorship; although throughout the history of the Institute, corporate sponsorship and contracts were quite variable. Hoffmann-La Roche gave money, the Japan Shipbuilding Industry Foundation gave money, so there were corporate and organizational sources of funding for the Institute. Sometimes they were restricted and sometimes were non-restricted. Luckily the money from some of those large donors was unrestricted and could be applied across the board to support whatever Linus Pauling and Art Robinson or Emile Zuckerkandl felt was important. But stock market performance and

the vagaries of the national economy over the years affected the ability of individuals and to some extent corporations to give money - it affected their philanthropic abilities. When there were down times during the recessions, people gave less money.

Nevertheless, at the Institute, the costs were constant or increasing. There was always a pinch and that would often be resolved transiently by decreasing contributions to pension plans or salary reductions; they were temporary and they would be lifted once the economic picture brightened somewhat. It was always kind of a roller coaster depending upon what was going on in the national economy. When you're a charitable organization depending upon philanthropy, the economic climate affects the ability to give, and we certainly were aware of that. Much later, as I said, when the Mayo Clinic studies on vitamin C and cancer were published, that also caused us transient loss in funds because people felt now that vitamin C had failed to show anything. Therefore, why support that kind of research? And still later, when people became aware of Pauling's interest in work on HIV and AIDS and vitamin C, that also caused a transient loss in support because some people were opposed to that work on moral grounds and didn't want to see that kind of research funded at the Institute.

CP: It strikes me that there couldn't have been many organizations like LPISM that were doing scientific research but were depending mainly on donor funds.

SL: I don't think there were. In the early 1990s, John Leavitt, who had been the director of the molecular carcinogenesis laboratory, calculated the impact factor for the institution. Impact factors can be calculated for journals, they can be calculated for scientists, they can also be calculated for institutions. And I believe the result of John's analysis at that time ranked the Institute in the top fifteen non-profit, independent, biomedical research institutes in the country on the basis of impact factor. I should add that that was not all related to Linus Pauling's productivity. Lots of papers from Institute staff figured in the calculation of the impact factor. So despite the fact that there were financial problems throughout the Institute's duration, I think ultimately, in terms of productivity and influence, it succeeded, surprisingly well given its size and lack of resources. [1:05:14]

CP: In 1980 the Institute moved from Menlo Park to 440 Page Mill Road in Palo Alto. What precipitated this?

SL: Well, my understanding at the time was that the building owned by Tom Ford, in which the Institute had been located, was primarily an office building and it was on loosely packed sedimentary soils. And over time, because of seismic activity related to the San Andreas fault, there had been some structural problems with the building. I believe that the owner wanted to evict everyone to fix the building structurally and probably did not want to invite the Linus Pauling Institute back because he could make much more money by renting that space to paper companies or people who weren't doing lab research and whose financial outlook was sometimes sketchy at best. That precipitated a search by Institute administrators - Rick Hicks and Emile Zuckerkandl in particular - to find a facility in which the Institute could carry out research, which led them to this old battery factory, cinderblock construction, at 440 Page Mill Road that didn't require a great deal of renovation to accommodate the Institute's work. There was some renovation in laboratories; we had a clean room that was designed for large-scale mouse work; a vivarium where we were housing guinea pigs and other animals for some vitamin C research. So the move was made to that facility. We were on very good terms with the new landlords, who increased rent based only on the CPI every year or couple years. They weren't out to make a killing by escalating the rent to the point where we would have to move and they would get some other tenant in there. They seemed to have some feeling for the Institute's mission and admiration for Linus Pauling and wanted to make it as easy as possible for the Institute to remain in that building.

CP: So it sounds like, in financial terms, it was probably a positive move. In terms of the space itself what was the impact?

SL: Well, there was much more square footage available to us in that building. The building itself was pretty shabby compared to the nice office building that we left in Menlo Park. As I said, it was a cinderblock construction, kind of an ugly building, really nothing great to look at. Definitely not a showcase building, but it was functional and the offices were fine and we were able to carry out the scientific work, so that was fine. It suited us at the time. And we moved everything ourselves. Alan Sheets, who was a biochemist working at the Institute, his father had been a professional mover in Idaho and Alan had spent a lot of his boyhood years and college years doing summer jobs moving for his father's company, so he really knew about moving equipment. Alan really coordinated our move from one facility to the other.

We rented a bunch of big trucks, hand trucks, and blankets, and all the other material we needed to move and did it ourselves. Saved a lot of money.

CP: Were there any special considerations, I would assume, with the scientific apparatus or with experiments that were in process?

SL: Everything was moved very carefully, of course, and we had to close the experimental work and then lock down all the instruments. It was a challenge to move some of these things. But nevertheless we did it, and vendors came in to recalibrate our centrifuges and other equipment. At the time, we had quite a few reasonably young men on staff, so we had quite a lot of muscle power. [1:09:58]

CP: In 1982 the Institute gave out the first Linus Pauling Medal for Humanitarianism, can you comment of the background of that?

SL: Well, of course, at that time I was not in administration, I was just in research, so I didn't really have any insight into the nature of this award. I mean I was aware of the awards and the ceremonies and so forth. And the candidates seemed to be uniformly deserving of the award, but it always seemed to me to have a bit of a fund-raising component to it. If you ask the people who had set up these awards and the occasions, I think they would probably acknowledge that there was a fund-raising component.

CP: What was the nature of the laetrile controversy in 1983?

SL: Again, I didn't have a lot of familiarity with that. I know that Pauling believed that there may be some validity to laetrile, and he thought that there was enough biological plausibility that studies should be undertaken. I don't think he had a firm opinion that 'yes, laetrile is definitely valuable in treating cancer.' But I think he was calling for more research to really get to the bottom of whether this was going to be useful in cancer therapy. Other than that, I really was not very aware of the controversy at the time.

CP: So nothing actually went on within the Institute in terms of investigation?

SL: Not that I can remember, no. One program that I got involved with - just backtracking slightly - Ewan Cameron was very interested in whether or not vitamin C would affect chemotherapeutic drug activity in the body. He was concerned that cancer patients who were taking chemotherapeutic drugs that were designed to be effective in halting the spread of cancer or attacking the primary tumor - he didn't want those people to take simultaneously high-dose vitamin C if the vitamin C was going to neutralize the effectiveness of the chemotherapeutic drugs. Vitamin C has some detoxifying capabilities, and it can interfere with certain drug actions. And he was very concerned about that and wasn't sure whether vitamin C would or would not affect chemotherapeutic drugs. So we set up an experiment - Alan Sheets and I with Ewan Cameron and Len McPherson - to look at the effect on fingerling trout of chemotherapeutic drugs and vitamin C. You can monitor the movement of fish, which relates to their physiology and biochemical activity and how they are feeling. The idea was to measure movement in fish that had been exposed to drugs like methotrexate with and without vitamin C to see if vitamin C was interfering with the drug action in the fish.

CP: Okay the last thing I want to ask you about today is Pauling's relationship with Ryoichi Sasakawa. We've talked a bit about his foundation, the Japanese Ship Building Foundation I believe, and my understanding is that they donated money to the Institute. Sasakawa is something of a controversial figure - is that something that you know was ever discussed within the Institute? Or did they identify mainly the source of funds because there was a pretty close tie there it seems?

SL: I think some people were aware of Sasakawa's controversial history in Japan; I was aware that he had made some fortune from motorboat racing in Japan, that didn't really bother me much. I wasn't that familiar with some of the other accusations levied against him at the time. I think that the magnitude of the gift from the Sasakawa foundation affected what we were able to do at the Institute so dramatically that most people chose to just accept the money, which I believe was about \$500,000 a year for ten years. A lot of that focused on aging research, and we had a Sasakawa Aging Research Laboratory set up at Porter Drive. When Sasakawa and his entourage would visit the Institute, it was always in a long line of black limousines and many, many people in dark suits would always accompany him. So my casual observation was that he seemed to be a pretty important figure and very wealthy, but I didn't know much about him. I met him once and

had some subsequent communication with his son and other functionaries at the foundation much later. But I wasn't privy to any interactions that took place between Pauling and Sasakawa.

CP: Okay thank you. [1:15:06]