ORAL HISTORY PROJECT

CHAIRMEN OF THE DEPARTMENT OF MEDICAL PATHOLOGY

UNIVERSITY OF CALIFORNIA, DAVIS

1967-1996

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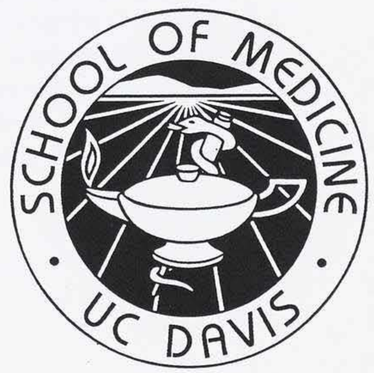
Preface by Ralph Green, MD

Interviews conducted by
Joann Larkey

May 1999



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Preface

Personal interviews of Chairs of the Department of Pathology at the University of California, Davis, were conducted to mark the 25th anniversary of the founding of the School of Medicine. This booklet contains the complete verbatim transcripts of those interviews conducted by Joann Larkey, an experienced parliamentarian and recorder of the history of UC Davis. The interviews represent a comprehensive chronicle describing the development of the Department, as seen through the eyes of the leaders who helped shape the department and its destiny. Much as participants in a relay, the department chairs passed on the baton of leadership, but each left his indelible mark on the department. Often differing widely in leadership style, background, area and emphasis of interest and philosophy, each has made a unique contribution to the phenomenal growth and stature of a department that is so young. The interviews contain candid reflections and personal views that provide a good description of the course that the department has taken during its formative years. Such information is vital to gain a full understanding of the Department of Pathology at UC Davis, its early trials and tribulations, its milestones and spectacular achievements. The six chairs who have provided leadership to the department are a remarkable group of visionaries who have not only helped to build the department, but who have also contributed to pathology and medicine nationally and internationally. Their names carry recognition and respect among pathologists in medical circles that span the globe. Few pathology departments can boast such a cast of leaders. Our department and university can feel justly proud.

Nothing has been edited out of these interviews. This is an unexpurgated rendition of the views of each chair, and their opinions have not been subjected to diplomatic scrutiny. Each has, however, read and approved the publication of their recorded statements. This compendium of oral histories is indispensable to anyone who has ever been associated with the Department of Pathology at UC Davis. The ideas and reflections are a source of inspiration to us all and provide a roadmap that shows us where we came from, where we are and where we may be going. I am honored to follow in the footsteps of these leaders. Anyone associated with the department should feel proud, reading this collection of reminiscences.

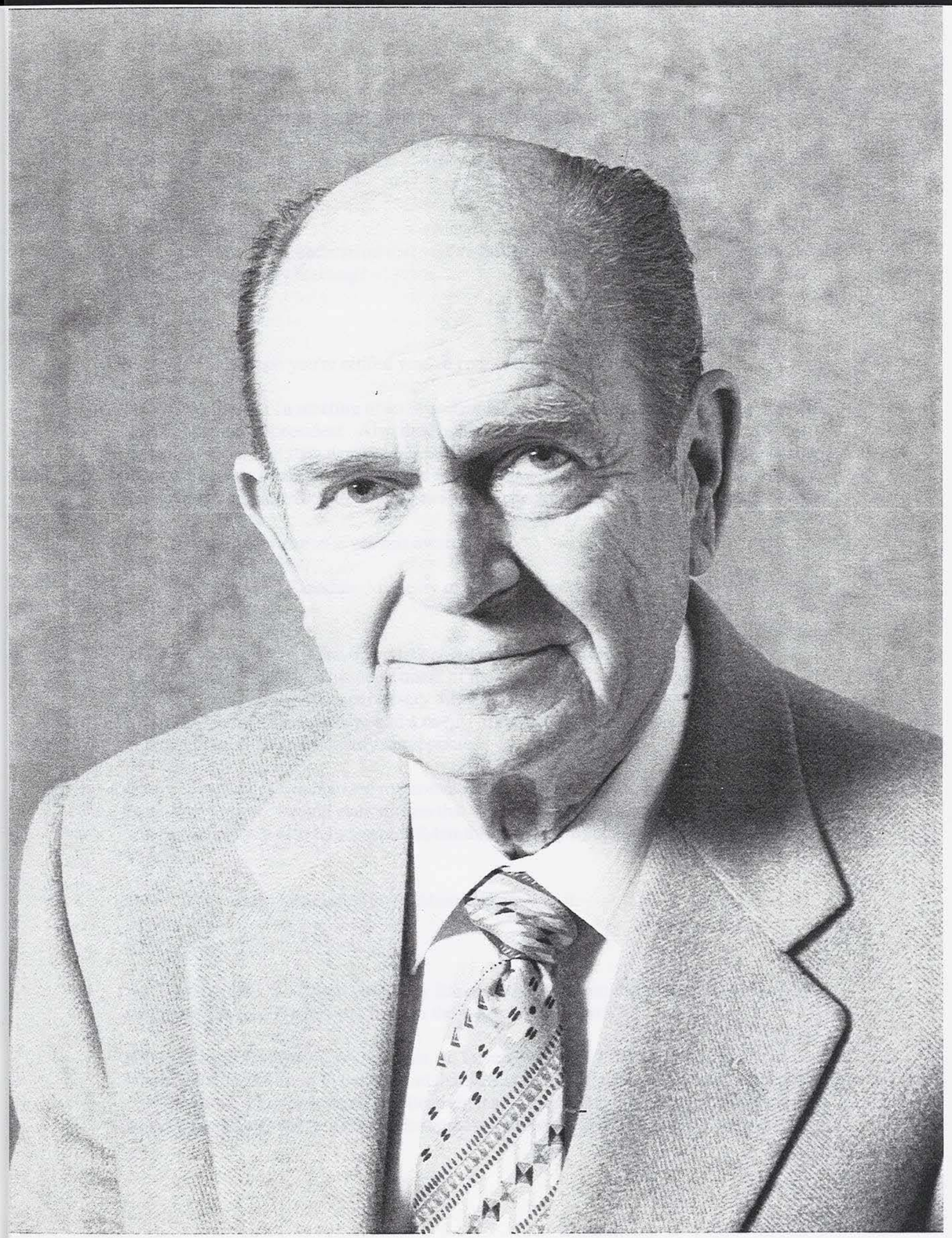
Ralph Green, M.D.
Professor and Chair, Department of Pathology
University of California, Davis
May, 1999

ROBERT E. STOWELL, MD

CHAIRMAN OF THE
DEPARTMENT OF PATHOLOGY

UNIVERSITY OF CALIFORNIA, DAVIS

JANUARY 1967 — JUNE 1969



Joann Larkey Interview with Dr. Robert E. Stowell

April 20, 1995

JL: Dr. Stowell, I understand that you've just returned from a meeting in Toronto. Was this a pathology meeting?

RS: Yes.

JL: Even though you're retired you've continued your interest.

RS: Well, this is a meeting of an organization in which I have been active for over 50 years. I am a past president. Also they established an award for research by beginning investigators, which they named after me and Dr. James Lowell Orbison. When it started there were a few dozen applicants, and this year they had 125 submissions. At each annual meeting I am asked to present the award checks to the winners.

JL: What organization gives this award?

RS: It's the U.S.-Canadian Academy of Pathology now. It used to be called the International Academy.

JL: Today I'd like to have you talk about your experiences with the Department of Pathology at UC Davis. This department of the UC Davis School of Medicine is celebrating its twenty-fifth anniversary this year and people are trying to develop a historical record. Since you've had a twenty-five year association yourself, your comments would be particularly appropriate.

RS: Yes, well, I summarized my comments at the meeting, the Fourth Annual Pathology Forum. I could elaborate on that or on a few other things. I have a copy which you say you also have recorded, but this is a copy of comments which has a few changes.

JL: That would be helpful if I could make a copy of it.

RS: Yes.

JL: First, let's talk about your early life and education and the things you did before you got to Davis. Weren't you born in the state of Washington?

RS: Yes, in Cashmere, Washington, on December 25, 1914. I lived in Okanogan, Washington until I went to college and that was in the midst of the Depression. I was the only one in my class that went to college right after high school graduation. That may sound quite significant, but my graduating class had only thirty students.

First I went for one year to Whitman College in Walla Walla, Washington. Fortunately, I was able to go there because I had a tuition scholarship. I had a job for my room, cleaning the dormitory in which I lived, and I worked in a cafeteria for most of my meals. So I got by on expenses of slightly over $300 the first year. And then near the end of my first year, an uncle who was a college professor died. He left his estate to a scholarship fund for his nephews and nieces, and I was the only one to take advantage of it. It was based on scholastic records, in other words, it required an A minus average in order to get tuition, board and room.

JL: An incentive to study hard.

RS: I found that was a strong stimulus to study, so after the first year at Whitman College, I transferred to Stanford University in California for three years to graduate from Stanford in 1936.

As an undergraduate during summers, I worked in a sawmill in Okanogan. As a supervisor, initially, I was paid 18 cents per hour, and my helper received 15 cents per hour to support his wife and two children. It was hot and hazardous work. There was an inverse relationship between the number of fingers and years of employment. Some veteran operators of band saws had only a total of five remaining fingers on both hands.

JL: At what point in your college career did you decide you wanted to study medicine?

RS: It was probably when I went to Stanford. I had been interested somewhat in law or medicine, and I'm very glad I didn't choose law. I got interested in the biological subjects I was taking at Stanford and that encouraged me to go on to medicine. I went from an undergraduate at Stanford to medical school there.

JL: Was that mostly based in San Francisco at that time?

RS: No, the first year was on the Stanford campus, and the rest of it was in San Francisco at the Stanford Hospital.

After graduating from Stanford medical school, I was considering my future. While a medical student I had worked in the Department of Anatomy during the summers preparing teaching materials and also doing some research. I was considering the possibility of taking training in surgery, but the people in anatomy received a letter seeking somebody to do cancer research at Washington University in St. Louis, Missouri with Dr. E.V. Cowdry. When I talked about this to the head of surgery at Stanford, he suggested that I, by all means get the research experience. He said that I could always have an appointment as a resident in surgery at Stanford, but research experience would be good for me. So I went to St. Louis.

JL: Was this right after your four years of medical school, instead of an internship?

RS: Yes. I did research for two years, and then I again considered what I should do. I still had some thoughts about surgery, but some of the people I was working with strongly recommended I go into pathology. Am I going into more detail than you want?

JL: No, that's interesting.

RS: On a trip, I visited several places, including the medical school at Harvard where I talked to the chief of surgery. He offered me a position but said he took half of his residents starting in July and half starting in January. His positions were all filled for July, but he suggested I might take six months in pathology before entering surgical training at Harvard.

After thinking things over I decided I'd take more than six months in pathology, so I ended up in pathology at Washington University.

JL: You decided you'd rather be a pathologist than a surgeon?

RS: Yes, I thought I would.

I finished my specialty training there in pathology and stayed on the faculty until I was an associate professor.

JL: You had that Fellowship in Cytology at Washington University from 1940 to 1942. You apparently started work on a Ph.D. as well in 1942. What was the subject of your thesis?

RS: It was entitled "The Photometric Determination of Thymonucleic Acid in Tumors." Thymonucleic Acid was an earlier term for DNA. I was probably the first to construct equipment to measure the light absorption of Feulgen stained DNA in tissue sections. I followed the changes in DNA in cells during methylcholanthrene induced tumorigenesis in mouse skin. The DNA change in neoplasia in human skin tumors was also measured. The DNA decreased in irradiated transplantable mammary tumors in rats and mice.

Fortunately, modern equipment and technology represent very significant improvements over the microphotometor I constructed and sampling problems I encountered. Now Fulgen stained nuclear DNA is being measured much more precisely.

JL: You were studying for your Ph.D. at the same time you were doing a residency in pathology at several hospitals in St. Louis?

RS: Yes, fortunately my background met many of the Ph.D. requirements at Washington University. As a Stanford medical student I had taken many elective courses, which fulfilled all of the course requirements for a Ph.D. And my prior and continuing research constituted the basis for my thesis.

JL: Hadn't you also gone to Sweden?

RS: Yes, while I was at Washington University I attended a meeting on the East Coast, and met a very famous and very reputable Swedish professor there, Torbj_rn Caspersson. One of the people from whom I was getting grant support suggested that I consider spending some time in Stockholm with Caspersson. So my wife and I went there for a year. We were married in the fall of 1946 just before we went, so it was sort of a combination honeymoon and research experience.

JL: Had you met your wife while you were at Washington University?

RS: Yes, she was a student dietitian. Her maiden name was Eva Mae Chambers.

That was a very interesting experience in Sweden. We were among the first of the foreign biological scientists to come after the war. The streets of Stockholm were lined with stacks of wood for heating purposes. Hot water was available in our apartment only on a few special holidays, but we joined Swedish friends in weekly trips to the public bath houses. We learned to use food ration coupons. We didn't speak Swedish, but a few people could understand our English.

JL: Did you have a chance to see any of the rest of Europe while you were there?

RS: Yes, we saw parts of Sweden, and then we went to Gotham, a small, ancient town on the Baltic Sea for a few days. Also, we visited briefly in Oslo, Norway. We spent three weeks in England. With a rental car we traveled around Scotland and the Lake District of England. We have gone back numerous times to other parts of England. Since then we have visited over 50 countries on all seven continents.

Shortly after returning from Sweden, I was offered a position as Director of Cancer Research at the University of Kansas.

JL: You went to the University of Kansas in 1948 as the chair of the Department of Oncology.

RS: Yes. The experience in Kansas was very interesting and stimulating. We were successful in developing cancer research programs. A couple of years after founding the Department on Oncology the chair of Pathology retired and they asked me to head a combined Department of Pathology and Oncology.

JL: You took over the chairmanship of that department from 1951 to 1959.

RS: Yes. Initially, the department was quite small. We recruited a good staff and developed an innovative teaching program for the second year medical students. The first year of the medical school was still at Lawrence and the last three years were in Kansas City. Our national research grant support grew to the second highest of any

pathology department in the country, according to NIH.

After staying there for eight years, I was offered a position as Scientific Director of the Armed Forces Institute of Pathology in Washington, D.C., which I thought would be a challenging opportunity. Also, I had not had any military service and I felt I owed my country something. So we moved to Washington, D.C.

While at AFIP I traveled to many interesting places. We developed a program in geographic pathology, demonstrating that the effect of geographic areas and environment were important diseases factors.

JL: This was world-wide?

RS: Yes. Our opportunities for cooperative research were limited, but we developed cooperative programs with institutes in South Africa, Uganda and also in Bangkok, Thailand.

JL: Was the Armed Forces Institute of Pathology actually under the jurisdiction of the military?

RS: Yes, there was a military director and I was the civilian scientific director.

JL: Did the Army hire quite a few civilian scientists?

RS: They had several hundred civilian scientists and other personnel. It was, and still is, the world's largest pathology institute. I am going there next month. They are dedicating a conference room to honor the former executive directors and presidents of the American Registry of Pathology, a position I held after I left AFIP.

JL: Could you describe that registry?

RS: Well, the American Registry of Pathology is sponsored by a group of scientific organizations, not just pathologists. They started out with ophthalmology and they have very active groups in radiology and orthopedics. There are now about twenty-five different scientific societies that sponsor one or more of the registries. The registries collect material from civilian and governmental sources, which is then used for research and educational purposes at AFIP. Also they return a diagnosis on the material submitted.

JL: So if there is a case in a civilian hospital it can be sent there for analysis?

RS: Yes, if it's a particularly difficult case. They have experts in all areas of pathology, and a lot of material is submitted to them. At one time they got most of their specimens from the military services, but now they are more selective in materials submitted. They also put on a series of educational courses for pathologists and other relevant specialists. For example, most U.S. radiology residents receive some training at

AFIP. They publish and sell the fascicles of the Atlas of Tumor Pathology. The registry serves as the most important interface between military and non-government citizens as well as for some other specialty areas that relate to pathology.

I had some interesting experiences with the United States Congress when I was at AFIP. The original AFIP building was on the Mall. Albert Hirshhorn had a large art collection. He was adept in making arrangements. Through the director of the Smithsonian Institute, he obtained the support of Lady Bird Johnson. And he got the Hirshhorn Museum located on the Mall, on one of the most valuable pieces of real estate in Washington, D.C. Of course, to build it they had to tear down the AFIP Medical Museum.

JL: How long had that been there?

RS: It had been in several locations, including the Ford Theater in Washington, before being on the Mall for many years. The building was designated a national monument the year before they decided to tear it down. The earliest specimens were collected from soldiers in the Civil War.

JL: Were you involved in the controversy over this decision?

RS: Yes, the military felt that they could not become involved because President Lyndon B. Johnson was their Commander in Chief, and Lady Bird had taken a position that she wanted the Hirshhorn Museum located on the Mall. So the military director of AFIP asked me to do what I could. He did arrange to get good help from someone who had done a lot of lobbying for NIH and knew his way around congress very well. I worked with that gentleman. We did quite a variety of things. First, we tried to save the building on the Mall. When we couldn't do that, they tried to palm off some very old run down buildings. I went with the lobbyist to inspect one of them, and we found that there were termites all over the floor and the building was in terrible shape. I organized groups to appear before the appropriate committees of congress, and made suggestions for their congressional testimony.

Finally, the lobbyist said we'd lost the site on the Mall, and the best thing we could do now was to get money for a new building. This lobbyist had obtained funding for a number of buildings for NIH and knew the cost of construction. So I called the military director of AFIP and said this is what we're going to have to do. He said, "We'll add a few hundred thousand dollars extra to it, and go for it." Over the signature of a prominent non-governmental pathologist, I prepared letters that were sent to hundreds of pathologists urging them to contact their congressman regarding the AFIP needs. With the lobbying and the action of congress, we did get the appropriation for an addition to the AFIP building which was on the Walter Reed Army Medical Center campus.

This was not a good location for a museum. It was not near places frequented by visitors. They got some at AFIP, but they didn't get nearly as many visitors as they did on the Mall. There they had many visitors, especially on Sundays. And this was

apparently because they had the only lavatories open on Sunday on the Mall.

JL: Is the museum still intact on the Walter Reed grounds?

RS: The museum is. They're going to try to get back on the Mall. They have some very powerful people that are joining forces with NIH, and they are planning a health museum as well as having a museum of disease specimens.

JL: Is that pretty much what the earlier one was?

RS: The earlier one was more of a collection of disease specimens. But they had some interesting things. They had an electron microscope that Hitler had had. My wife and I went to New York and brought back a chicken specimen from Peyton Rous at the Rockefeller Institute. This was the first example of a virus induced tumor, and it was of great interest to many people. Peyton Rous isolated the virus from a tumor in a Plymouth Rock chicken. He produced tumors in other chickens by injecting them with the virus.

The other experience I had with Congress was after I came to Davis. Somebody in Congress decided they would cut the budget of the Primate Centers. With the benefit of my prior experience, I organized groups to testify before committees in Congress. I was fortunate in the selection of those who testified. A former director of the Heart Institute of NIH had prior significant dealings with Congress. When, with him, we went before the house committee, one of their staff people who had tremendous influence with his committee, said, "Oh, Jim, how are you? I haven't seen you recently. We'll have to get together for lunch today." (Laughter) Well, that helped! We were able to save the entire budget for the primate centers.

JL: Were you involved at all in Japan after the war with the atomic bomb fallout? Was that related to the pathology work you were doing?

RS: To a very limited extent. The Atomic Bomb Casualty Commission of the United States collected a lot of materials following the two explosions in Japan. Those were brought for study to the Armed Forces Institute of Pathology. When I went to work at the AFIP, they still had this large collection of materials. It wasn't being utilized much then, although it had been when it first arrived. Pathologists studied it and published papers on their findings. Then the Japanese eventually decided that they didn't want any part of their relatives in a military institute in the United States, so all the materials were returned to Japan. But I did visit the Hiroshima site on one of my trips to Japan and saw some of the things there.

JL: Were scientists able to learn a lot from those materials?

RS: They learned a significant amount, yes. Also, one or two American pathologists used to go to Hiroshima and work with the Japanese who were doing the follow-up work on the people that had been exposed. The U.S. no longer participates with the Japanese in these studies.

In early 1967, I was invited to attend a meeting to discuss medical education plans in connection with the new medical school at UC Davis. I decided to come in June of 1967.

JL: What was the challenge for you to come from Washington D.C. to Davis to start a new medical school?

RS: I was challenged by the opportunity to work with enthusiastic, innovative educators. They were establishing a new medical school in an institution that offered unique opportunities for cooperative research. There were outstanding biological scientists in the School of Veterinary Medicine, Agriculture, Letters and Sciences and the National Center for Primate Biology.

I enjoyed an efficient physical move from the Armed Forces Institute of Pathology because my chief administrative secretary supervised the packing of 36 boxes of books and file materials at AFIP. She then came to Davis for a month to supervise the unpacking and organizing my new office.

I was rapidly immersed in many opportunities. I brought with me a few continuing responsibilities, the chief one of which was the editorship of the monthly publication of Laboratory Investigation. Fortunately, my editorial secretary moved with the editorial office to Davis, and subsequently I received assistance by having Drs. Ruebner and Wellings as associate editors.

JL: Had you known any of the initial faculty before coming to Davis?

RS: The professor of

to others later.

JL: The faculty was quite small in 1967, wasn't it?

RS: Yes, I was the eighth person to join the faculty.

JL: And hadn't they already created the divisions or basic groupings within the medical school before you came, so that pathology was under the Division of Surgical Sciences?

RS: That had occurred before. Dean C. John Tupper had been here for about a year, and he moved on a very fast track. He had brought in other people that assisted with the very basic initial planning so that they had the initial plan for three divisions. In pathology we actually had our feet in two divisions, as it were. We were part of the sciences basic to medicine, but we were on the salary scale with the surgical division, so we interacted with both groups.

JL: Earl Wolfman was the head of the surgical division.

RS: Yes, and Robert Bolt was head of the medical division. Lauren Carlson was head of basic sciences.

Not only did I recruit a staff in pathology, but I helped in recruiting staff in other departments also. We didn't have teaching materials that we needed for pathology. We had to arrange to get assistance. We got microscopic slides from the University of California at San Francisco. Since our class was small they had enough surplus slides to help us get started. We got gross pathology specimens from some of the universities in the Midwest and on the East Coast.

JL: Weren't there 48 students in that first class that they accepted in 1968?

RS: Yes, and I was vice chairman of the first admissions committee. As assistant dean, I had a special interest in research development, and so I helped people get established in research.

JL: What were some of the first projects that you undertook?

RS: I was a member of the dean's Executive Advisory Committee. I assisted in initiating activities in medical student financial aid as well as a liaison with the architect's office on the early plans for the Medical Sciences Building on campus. I participated in the earliest planning of the curriculum for the first and second year students.

Although I arrived in Davis in late June of 1967, it was not until January 1, 1968 that I was able to recruit Dr. Thomas Volk from the University of Kansas as our first new faculty member. He was followed in April of 1968, by Dr. Larry McDonald, whose principal activities were devoted to neuropathology and assisting neurologists and neurosurgeons in Sacramento. We were helped in many ways when Dr. Boris Ruebner joined from Johns Hopkins in May of 1968. Another major addition was Dr. Rolla Hill who came on July 1, 1968. He was so good that he left after one year to become chairman of the Department of Pathology at Upstate New York Medical School in Syracuse. As a major addition to our staff, Dr. Sefton Wellings moved with two significant research programs from Oregon and joined us in May of 1969. Dr. Hanne Jensen became a member of the department on July 1 of that year and assisted Dr. Wellings in his breast tumor research.

JL: What were some of your other early projects?

RS: Well, one of the most significant that I became involved in was called a Health Science Advancement Award Program. This was a program in which National Institutes of Health (NIH) made only ten grants in the entire country, but it was particularly good for UCD. I put together a 500-page application which was the biggest one I ever wrote.

JL: Another challenge.

RS: Yes, it was. We were granted the maximum amount that they would allow, which was $500,000 for a period of five years. This was a large grant in those times. We had a series of groups working in certain areas of their own research interest. This was very valuable money because it helped investigators get started. It also helped in cooperative research between not only the faculty of the School of Medicine, but also with Veterinary Medicine, Agriculture, Letters and Sciences and the Primate Center.

JL: Dr. Tupper seemed to have been very interested in that from the beginning, in developing relationships between the medical school, the veterinary school, and some of the basic science departments that were already on the campus. Was this an effort to implement that program?

RS: Well, it did implement that. It was, at that time, probably the strongest project that we had to enhance the development of cooperation between the scientists in the different schools and colleges. It enabled us to get some very good expensive equipment which could be shared. We set up cooperative laboratories, with technical assistants, where researchers could do work in electron microscopy, histopathology or chemical studies. These facilities could be shared by the different people on the projects.

JL: I wanted to ask you about the electron microscope. When in your career was that developed?

RS: Well, when I was at the University of Kansas, we were the first Department of Pathology in the country to have an electron microscope. When I came to UC Davis, one of the things that I was interested in developing was electron microscopy.

JL: It seems to have been a technological advance that really made a tremendous difference to a discipline like pathology. I'm sure it had many other applications as well.

RS: Initially, it was primarily a research tool, but in more recent years it has been a valuable assistance in diagnostic work.

JL: So you were able to get one at the Davis campus?

RS: Yes, we got one, and in fact before long we had several. When Dr.Sefton R. Wellings came, he brought a small electron microscope, and we got a couple of others, so we were fairly well equipped.

JL: What were your facilities like when the pathology department was first developed?

RS: They were quite limited. When I first came I was blessed with two offices, one in the dean's office area, and one in a small what they called "speed space" building, with about a thousand square foot building for pathology. I finally consolidated my offices into just a pathology area. But we had only this one building at first and it was not

sufficient to support any significant research programs.

The main building of the medical school was in three phases. In the first phase, which had the dean's office and administrative areas, we had a couple of labs. And when they added the next larger area, we got an additional laboratory. Later, with the Health Science Advancement Award, we got a couple of additional speed space buildings, which we used for central laboratory facilities. Then we got another one as we got more staff.

JL: Those buildings are all located along Hutchison Drive and south of the old veterinary school?

RS: Yes. And then we continued to add space slowly, but we didn't have enough space that we could bring in many key people who already had a large research program, which was what I wanted. Dr. Wellings brought a couple of research programs.

JL: Those were basically cancer research programs, weren't they?

RS: Yes, and he is a person with really broad interests. He was interested in tumors in fish and a variety of other unusual things.

JL: What about setting up the pathology department at the old Sacramento Country Hospital, which eventually became the UCD Medical Center?

RS: Yes, I was told before I came here that everything there was OK, that even though I was the director of pathology services, it wouldn't take any significant amount of my time. But as soon as the clinicians started arriving from very good institutions around the country, they found that the level of pathology service at the hospital really wasn't what they were used to. So that started to raise some problems.

JL: There was already a pathology staff in place?

RS: Yes, a small staff, with a small amount of space, relatively speaking. And so one of our jobs was to increase the staff and try to increase the space. When we got new staff, they were primarily based in Davis, but we all went to Sacramento and participated in the pathology there. Dr. Henry Tesluk and Dr. Julita Fong were two very good people and Dr. Delores Hardre and her husband Dr. Rene Hardre were also quite capable. This initial SMC staff provided significant professional competence in anatomic pathology.

Before I came to Davis, I obtained a commitment from Dean Tupper that clinical pathology would stay with anatomic pathology. And in order to get that commitment, I had agreed that if there were areas in which some of the departments had more expertise than pathology, that we would cooperate in using their services, which we did. Dean Tupper and Dr. George Snively, Medical Director at SMC, had to referee some of the debates between pathology and internal medicine. They made the administrative decision to keep clinical pathology with anatomic pathology, which is good. It's important for pathology because we now have to train people in clinical pathology as well as in

anatomic pathology. At one time, many clinicians would have training in pathology. Today, they are spending more time on their own disciplines for research and specialized clinical experience.

Initially, the internal medicine faculty made no secret about the fact that they wanted to take over clinical pathology, and they were very aggressive. They even took over space for clinical pathology in several of the areas that were for anatomical pathology, as well as my own office space as director of pathology at UCDMC. I'm not saying they didn't need space since everyone needed space.

But in the areas of infectious disease, we still have cooperation from the Department of Medicine. Dr. George Jordan has a joint appointment there. In hematology, another person has a joint appointment in pathology and in internal medicine. So in some of these areas there is considerable interdisciplinary support. Well, the Department of Internal Medicine, as far as I know, has given up on the idea of taking over clinical pathology, so things are running much more smoothly.

I got side-tracked from pathology to some extent because of the primate center.

JL: Yes, how did that come about?

RS: Well, as soon as I came to the campus, Dr. Leon Schmidt, Director of the National Center for Primate Biology which had been located on the Davis campus by the National Institutes of Health in 1964, appointed me to his national advisory committee. Dr. Schmidt was very cooperative with the medical school, and Dr. Boris Reubner and Dr. Thomas Volk, two of our early recruits, used space there when it was so limited at the medical school.

When I was attending a meeting in Miami, Florida, I received a phone call from Chancellor Emil J. Mrak who said that "as his worst problem in ten years, he had to remove Schmidt as director of the primate center and wanted me to take the job as acting director while they looked for a permanent director." After some hesitation, I agreed to become the acting director.

JL: That meant you had to step down as chair of the pathology department?

RS: I didn't step down when I was acting director. I did both jobs for a little more than a year. After they were unsuccessful in finding a permanent director of the primate center, I agreed to take that job. That's when I stepped down as chair of pathology.

JL: What kinds of problems were you asked to solve at the primate center, and what kinds of research was being done there at that period?

RS: Initially, Dr. Schmidt had two large programs involving research in malaria and tuberculosis. Probably 80% of the research involved his programs. The center was set up as a National Center for Primate Biology with the idea that it would study the biology of

the non-human primates and provide basic information that would be helpful to other investigators. That was one of the things we started to do when I went there.

One of our first big jobs entailed replacing all the animal cages, which were substandard.

JL: How large a population of primates did you have at that time?

RS: It was over 1,000 and it included several different species. Later, we had to reduce the number somewhat, because the people at NIH in Washington felt that all of the primate centers shouldn't have too many kinds of non-human primates even though they were interesting to observe.

JL: So there were other primate centers throughout the United States?

RS: This was the last of seven to be established, primarily from NIH primate center grants. The others were all called regional primate centers with the idea that they would cooperate in research within a given region. Ours is now called the California Regional Primate Center.

JL: These are all under the umbrella of the National Institutes of Health?

RS: They are, but they are probably becoming increasingly independent. Initially, a large part of the money came from NIH, but NIH was not able to increase the funding substantially when it had seven centers, so the centers had to get other research support.

Before I went to the Davis primate center, there had been acute personality problems between some of the staff and Dr. Schmidt. Consequently, they moved Dr. Schmidt out of the primate center. The medical school provided him with some space while he sought a position elsewhere.

JL: Did he continue his malaria and tuberculosis research?

RS: He was in the process of finishing it and he stayed less than a year on campus before he moved to Birmingham, Alabama.

JL: So you were mending fences among the primate center staff. Was this staff recruited specifically for the primate center, or were some former faculty members who were already on the Davis campus?

RS: Many of them were recruited primarily for the primate center. Although I continued to recruit a few, this was one of the things which needed to be changed. In order to improve the cooperation, it was important that these people have appointments on campus. We did get faculty appointments for some of our staff. We did make considerable facilities available to some faculty members who were primarily in other campus departments. Therefore, they could use the primate center facilities for their

research.

JL: Did this include faculty members of the School of Veterinary Medicine?

RS: Yes, they were doing some work there. But they, of course, felt that the primate center should be under the veterinary school, and finally it was.

JL: It wasn't at that time you were the director?

RS: No, it was not. It was directly under the chancellor.

JL: So it wasn't under the medical school, either.

RS: No, it wasn't under any school.

The veterinary school finally started making waves, as it were, to get it under the veterinary school. Then Chancellor James H. Meyer decided that he didn't want to continue to be designated as the principal investigator, and that the dean at the veterinary school was going to become the principal investigator on the primate center grant. Even though I got along reasonably well with the people at the veterinary school, I felt it was a good chance for me to return to the medical school full time.

JL: So this happened when, about two years after Chancellor Mrak retired and James Meyer became chancellor of the Davis campus?

RS: Yes. I returned full time to pathology in 197l.

JL: Was there the opportunity for you to resume being chair of the department?

RS: No, Dr. Wellings, who had been chair at Oregon, became chair when I accepted the permanent position as primate center director.

JL: He says not at his request. He hadn't expected to be drafted for that position.

RS: No, he didn't. In fact, I found a letter in my files in which he said he didn't want to do any major administrative work for at least eight years when he came to Davis.

JL: That didn't last long.

RS: Only about a year.

When I came back full time to the pathology department, I continued my activities with the Health Science Advancement Award Program.

JL: In what areas of research were you working at that time?

RS: I worked with some agents that cause colon cancer in mice, and was studying the affect of hormones and various other factors that would influence the time of onset and the distribution and the nature of these colon cancers.

JL: And you probably also picked up a heavy teaching load when you returned.

RS: I picked up a teaching load and also a service load at the Sacramento Medical Center. Then I was still active in a number of things relating to national advisory committees and panels.

JL: Dr. Robert J. Cardiff mentioned the number of national and international organizations with which you have been involved. That involvement started quite early in your career, as I recall.

RS: Yes.

JL: What was the first one that you remember?

RS: The first one that I became active in became the International Academy of Pathology.

JL: Was that the first international organization of that type?

RS: Well, I would say one of the first. It was probably one of the older ones in pathology.

JL: That was established in 1954, wasn't it?

RS: It actually started in 1907 as the International Association of Medical Museums. Then we changed the name at 2:00 in the morning during at a very long council meeting I attended. But I got involved in that fairly early.

JL: Yes, I see on your CV that as early as 1945 you were assistant editor of the Journal of Technical Methods and Bulletin of the International Association of Medical Museums.

RS: When I was an instructor at Washington University, I was asked to edit one of the issues of their journal. I was told that I could do anything I wanted in rewriting any of the articles, but I couldn't reject any of them.

Later, in 1954, there was a real dilemma as to whether or not they should forget about the museum society. In fact, one of the first meetings that I attended was at Harvard and at a plenary session there were twenty-two people present. So it either had to be radically reorganized, or allowed to die peacefully. I got in on the reorganization of

the International Academy of Pathology and I was quite active with their new journal, Laboratory Investigation, and I was on its editorial board.

JL: I see that you served on the editorial board from 1952 until 1971, and edited the journal from 1967 until 1971.

RS: Yes, it grew to become one of the most important journals in pathology.

And then I was on the council of the International Academy of Pathologists from 1954 until 1961. And I advanced to the presidency and continued to be active on a number of their programs.

JL: You served as president from 1959 to 1960. Was this while you were living in Washington, D.C.?

RS: Yes. We were there eight years and left in '67.

JL: You mentioned that this organization was originally involved with medical museums. What type of museums were these?

RS: Formerly, they used museums of specimens of disease to teach organ disease.

JL: I remember those specimen exhibits from McGill University in Montreal. They had a tremendous display.

RS: Dr. Maude Abbott at McGill was the first secretary-treasurer and a leader in the starting and maintaining of the organization. Much of their meetings were concerned with techniques of preparing specimens and also histopathological methods.

That was the first of the organizations in which I became active. At first it was a U.S.-Canadian organization that had a British branch. That British branch had closed, but later reopened, and now I think the academy has branches in forty-four countries.

JL: Wasn't there an American Association of Pathologists and Bacteriologists? Was that something that you were involved in early on?

RS: I became involved in that after the International Academy of Pathology. And then, subsequently, I became involved in the American Society for Experimental Pathology.

In 1990, I received the association's gold headed cane award. This is given for years of leadership in pathology, research and education. It is considered by some as American pathology's oldest and most illustrious award.

JL: And you've served as president of all three of those organizations.

RS: Yes. Two of them have been combined now. The organization changed its name several times, but it started as the American Association of Pathologists and Bacteriologists. That became the American Association of Pathologists, and the American Society for Experimental Pathology finally merged with it. Then just a year ago they changed their name again to the American Society for Investigative Pathology.

In addition, I was active as a consultant on research and training grants for the National Institutes of Health and the American Cancer Society, and was chair of several of their advisory groups. Sometimes I made fifteen or more projects visits a year.

JL: Were you doing that before you came to Davis as well as after?

RS: Yes, I did a fair amount of traveling, attending these meetings and being on advisory committees.

JL: Which activities did you feel were the most rewarding from the standpoint of your being able to accomplish things for pathology.

RS: I think probably the activities connected with the National Institutes of Health. For example, the Pathology Study Section of the Division of Research Grants, of which I was chair [1954-1957]. We were able to accomplish quite a bit by increasing support for research and research training in pathology.

JL: Didn't you make a trip to China quite early after President Richard M. Nixon's "ping-pong" diplomacy opened the door.

RS: Yes, I headed the first organized small group of U.S. academic pathologists that visited China in 1980. We visited twenty different hospitals and medical schools, and we were very fortunate to get what I regard as a leading pathologist in China to go with us. He would introduce us at the various places we visited.

JL: Was he from a university center there?

RS: He was from the Chinese Academy of Medicine, and was head of the Basic Pathology Institute of the Institute of Chinese Medical Sciences in Beijing. He's been a long-time friend since then. We've had him in our home, and the last time I was in China, he took me out to dinner. He stops by and visits us every few years in Davis.

JL: What's his name?

RS: Professor Ming Peng She. We had a very good visit in 1980. We brought along a lot of pathology educational books and teaching materials that we left with the Chinese we visited. We encouraged Professor She to organize the Chinese Division of the International Academy of Pathology, in which he is still active.

Then I went back to China in 1984. After our first visit, we had two Chinese pathologists come to the United States. We arranged for one to come to Davis and another went to learn electron microscopy at the University of Maryland with a former protégé of mine. I helped find positions for half a dozen or more Chinese pathologists who wanted to come to this country to get research experience. It was one of the ways I was able to be helpful to them. Now they have their own connections. The first Chinese pathologist that came to work in my laboratory has returned for a second time and is now working with Dr. Cardiff and some of his people.

JL: How did you find the study of pathology in China on that first trip, comparatively speaking?

RS: At that time their research technology was quite a ways behind ours. The good equipment was localized in a few areas like Beijing, where they had institutes that were associated with the government. The equipment was rather primitive in some other places. I made a sad mistake while visiting a Chinese pathologist, Professor Chi-Shang Li who had been a fellow with me in Kansas City. He showed me their microtome, and without thinking, I said, "Oh, yes, I know this microtome. I used one just like that thirty years ago."

JL: So much for diplomacy.

RS: Yes. So I made several trips to China, which has been an interesting experience. I went back with another group of academic pathologists in 1984. Since then I have gone to China for several scientific meetings. They've progressed considerably. In many places they are now using quite modern techniques. They've updated their equipment and they have had, literally, dozens of their pathologists come to the United States and to other countries for research experience.

JL: Did you take on any different and new research projects those last few years before or since your retirement in 1982?

RS: I became a consultant to the National Center for Toxicological Research in Arkansas and was on their Science Advisory Board for a number of years. While at the primate center, I was named as chair of an Animal Resources Advisory Committee at NIH, which advised on improving the care of laboratory animals.

JL: Concern for this issue seems to have increased in recent years among animal rights activists as well.

RS: Yes, this committee was concerned with improving the animal care facilities, and elevating the standards for the whole country.

JL: You mean on all campuses and for anyone doing research involving animals.

RS: Yes, for anyone doing research with animals. Most of the people on the committee were veterinarians. I was actively working with them and went on many of their projects site visits.

JL: Do you think those concerns have been adequately addressed, or do you think this is going to be a continuing problem for researchers?

RS: The answer is more or less "yes" to both. I think that concerns have been addressed very well, but I think there are some people that feel so deeply about animal care that it would be almost impossible to satisfy them.

JL: Some people are basically opposed to using animals for research purposes, period.

RS: That's right. You are never going to convince those people to change. So it's going to be a continuing problem, but the care of the animals has improved substantially over the last couple of decades.

Another thing that I was involved in for a year related to Aspartame. This is an artificial sweetener now called "Equal." The Food and Drug Administration got into a big rhubarb with Searle and Company, in which some of the personalities were strongly opposed to each other. Some of the people in the Food and Drug Administration felt that Searle was cheating in some way, in terms of what they were trying to do; and people at Searle felt that the government was unduly meddling in their work and unduly accusing them. Finally, the Food and Drug Administration told the Searle people that they would not get approval to market Aspartame unless they had an impartial, outside review of some of their work. Searle, therefore, came to Universities Associated for Research and Education in Pathology, which was an organization that I had a key role in establishing when I was in Washington, D.C.

JL: This is an association of university scientists that are doing research?

RS: Yes, Universities Associated for Research and Education in Pathology aims to improve pathology research and teaching at universities. This is an association of institutions, although it functions, generally, as a group of the chairs of pathology from the various institutions. This association undertook a review under a contract with Searle, which the Food and Drug Administration approved.

At Davis we carried out an extensive, detailed review on a dozen projects out of thirty that Searle had submitted to the Food and Drug Administration. This work was done primarily at Davis, the University of Maryland, and Northwestern University. When we first met with the Searle people they wanted to know how big a report we were going to have, and I said, "I have no idea, probably a thousand pages." Our report turned out to be slightly more than a thousand pages. We reviewed over five thousand microscopic sections, with the pathologists reviewing them not knowing what diagnosis the Searle people had already made on the slides. Also, we reviewed thousands of pages

of research documents and recalculated all sorts of research data. The upshot of it was that we found that there were minor discrepancies, little things that didn't really affect the overall results that Searle had found in their original research. Our review took a year's time, and I was spending over half-time working on this since I was the main coordinator of all the data that was coming in from Maryland and Northwestern. We also had some of the work done at the veterinary school at Davis. We leased a speed-space building and wrote the total report under my supervision here at Davis. As a result, the FDA accepted our report and approved Aspartame for marketing. But even though that work was done a dozen or more years ago, I still occasionally get phone calls from somebody asking questions about it. Fortunately by now, the research material has all been discarded except for the final report.

JL: Is the report on file at the Medical School Library?

RS: It's not in the main library. One copy was in the pathology library and I have copies of it. And, of course, the Food and Drug Administration and Searle have copies. We made a dozen copies. But timewise that was a significant undertaking and one that was really relatively important in terms of it's effect.

Then I also did a couple of other reviews for UAREP, but they were not nearly as important as the Aspartame study. The others did not take as much time. They did not involve the detailed review of thousands of microscopic sections, and they resulted in reports that were only a few hundred pages in length.

JL: Still, a lot of work. Just getting those projects organized and formatting a research project takes a lot of organizational skills.

RS: Yes. Some of the latter ones were headquartered in Washington, D.C. So I worked on parts of them at Davis and then I'd go back there for several days at a time.

JL: Were you involved when the medical faculty at UC Davis decided to review and change the curriculum for the medical school and establish a new course in pathology?

RS: I was involved in developing the initial courses. Because our staff was so small when I first came in 1967, I was asked to start to organize the first year curriculum. We just got started on that and we said we've got to start planning the second year curriculum. I said it's much more important for me to work on the second year, which had much more pathology in it than the first year. So I did some of the initial planning. We had a lot of participation in the planning for the first two years, not only by basic scientists but also by clinicians. So I had something to do with the initial setup, which has been changed several times, and I have not been involved with the subsequent changes.

JL: Dr. Cardiff and Dr. Lundberg have both noted that in order to update the pathology course, it was necessary because of the academic setup in the medical school to change the whole medical school curriculum.

RS: Yes, the school started out with some ideas that weren't sound. Some of the people thought we could treat the medical students like graduate students, and that we could give them a considerable amount of free time to take elective courses and to do special studies. We thought that some junior and senior students would come back to the Davis campus and take electives in the basic sciences. As it turned out, very few did because as soon as they had contact with patients in Sacramento, that's where most of their interest was focused. The considerable amount of elective time sort of evaporated as departments and clinicians, in particular, were saying they had to have more time to teach their subject matter. So that reduced the elective time very significantly.

JL: The curriculum is probably in constant change.

RS: There have been several major changes, yes.

JL: Now that you're retired and looking back, I wonder what your perspectives are on the current trends of teaching and research in the Department of Pathology.

RS: Well, I think they have progressed very significantly. Their teaching is very good. I think they have one of the better teaching courses in the country. Dr. Cardiff and others have done a great deal to improve the laboratory teaching and, in fact, all of the pathology teaching. I think their teaching is outstanding, considering the amount of time they have. I had about twice the number of hours to teach pathology at Kansas as they have here. But with the time they have, they do a very good job. They have a faculty that is quite interested in teaching.

The research has advanced tremendously, especially through Dr. Cardiff's work and Dr. Gardner's big research program. Many pathologists in the department are doing productive, significant research. You may be aware that they are going to have a $15 million new building for comparative medicine located at the Primate Center.

JL: That's really a major change, isn't it, going back to doing the type of interdisciplinary research that was originally envisioned twenty-five years ago?

RS: Yes, we called our Health Science Advancement Award Program "comparative medicine," and we wanted to continue it as a campus-wide organization, after the core NIH grant expired.

JL: I've always been curious about why that original plan didn't work better.

RS: We had a lot of good equipment and we had space, but some people, unfortunately, were more interested in getting that for themselves than they were in continuing the cooperative program. The cooperative concept was generally accepted by everyone in theory. At the scientist level, it worked quite well. At the administrative level, at times, it didn't work so well.

JL: In view of past experience, do you see a bright future for this comparative medicine research center that they're building on the campus?

RS: Yes, I do because the veterinary school is playing a major role in this and Dr. Gardner is also playing a significant role.

JL: He worked with the veterinary school long before he ever came to Davis. When he was still at USC, he'd come here and do research.

RS: Yes. I think they'll have space for at least a dozen scientists and their staffs, so that will be a big addition.

As I've said, one of the main reasons I was interested in coming to Davis was the opportunity for cooperation between the medical school, the veterinary school and biological sciences and in the other schools and colleges. We have been almost unique in that respect.

JL: Yes, that's what's special about UC Davis. It began as an agricultural school, but now it has all these interdisciplines.

RS: Yes, Wisconsin, I think, has a veterinary school closely associated with their medical school, which is the exception. This is the only primate center in the country that is under a veterinary school administration, to my knowledge. But, yes, I'm thrilled that they've been able to do what I couldn't do.

JL: I'm sure you've helped a lot along the way.

RS: Well, on a few occasions, years after the Health Science Advancement Award Program had finished, some would stop me on campus and say they deeply appreciated the help they obtained for getting their research program going through this award program.

JL: I'll bet that was of benefit to many departments on the campus, in general.

RS: Yes, quite a few in the veterinary school, animal sciences, and the biological sciences benefited in addition to the medical school.

JL: I was impressed at the Pathology Forum when people were speaking about so many inter-relationships now between agriculture, the medical school and the veterinary school. Apparently, people are beginning to think in terms of more kinds of cooperative research.

RS: Yes.

I am presently active in the UC Davis Students First Campaign. It started on July

1, 1993 with a campus-wide goal of raising $15 million, 350 thousand dollars for student financial assistance. I am chair of the medical school committee. Our original goal was $2 million dollars, which by the end of the campaign in December 1995, we will have exceeded by $1 million dollars.

Since my retirement I have been a member of the UCD Preprofessional Health Advisory Committee. Each week for four or five months I review the applications, interview the students and write summarizing recommendations for student applicants for admission to schools of medicine, osteopathy, dentistry, podiatry or pharmacy throughout the country.

Since retirement, as a volunteer, I continue to teach students in the medical pathology laboratory for more time than other volunteers.

JL: The university as a whole has certainly suffered a budget crunch in the last few years. Has the pathology department suffered it's share and more?

RS: I'm not aware of the details, but I think they have probably suffered their share. Dr. Cardiff had a very good arrangement with the dean when he took over as chair, but I think he probably has lost some of the things that he worked so hard to get. But the research program is still good and the future of it will depend upon who they will be able to recruit as young research scientists in pathology.

JL: So there will be a second chapter to the department's history after the next twenty-five years.

RS: I expect so.

JL: Your many contributions to the research and teaching of pathology on the Davis campus and on the national and international levels will be duly noted, I'm certain.

RS: I hesitate to bring it up, but as a way of giving something in return, my wife and I endowed a Lectureship at Davis after I retired. I've also endowed ones at the American Registry of Pathology. At the American Society for Investigative Pathology, I endowed money for a symposium at their annual meetings on "Trends in Experimental Pathology."

Now, we're in the process of endowing a Chair of Pathology at Davis. Then, eventually, our estate will provide a significant sum of money for medical student loans, scholarships and research.

JL: That's certainly needed in today's world.

RS: So there are several things in that direction on which we're working. And one of the things that has occurred to me is that I probably will try to put together a personal history. Possibly twenty-five years from now some of the people who become the Stowell Professors or some of the medical students who get some of the Robert E. and

Eva Mae Stowell funds will want to know who were these Stowells.

JL: It will mean so much more to them if they know who you were and what you did. I was at a meeting last night with some university students who had received loans to help them through school, and they are so appreciative of the help and where it had come from. So I certainly would encourage you to write that personal history.

RS: Well, when I retire I will.

JL: When you retire the next time, you mean.

I also want to thank you for the comments you've shared today. They will be very helpful for the department history that Dr. Cardiff is developing.

RS: You're very welcome.

SEFTON R. WELLINGS, MD

CHAIRMAN OF THE DEPARTMENT OF PATHOLOGY

UNIVERSITY OF CALIFORNIA, DAVIS

JULY 1969 — DECEMBER 1975

MEMOIR OF SEFTON R. WELLINGS

Based on an interview conducted on March 6, 1995

This narrative must begin with personal historical data, which helps to explain why I joined the faculty of the new medical school at the University of California Davis Campus.

I was born on October 2, 1927 in Tacoma Washington. I attended grade school in Tacoma Washington; and high school in Bremerton, Washington. I graduated from high school in June of 1945, just as World War II was ending. I was the valedictorian of a class of 442 students. Some of my high school friends and teachers had already joined the armed services, and some never returned. During my high school years I was helped by excellent science teachers, and this influenced all subsequent events. I believe that I was always blessed with excellent teachers. I don't recall any teacher from whom I did not learn something of great value. This applies equally to my experience in high school, college, medical school and graduate school.

In the span of a few years, I completed two years as a Pharmacist's Mate in the United States Navy, and went on to attend the University of Washington as a premedical student. I was admitted to the School of Medicine at the University of Washington in the Fall of 1947, graduating without distinction in June of 1951. While in medical school, I spent my summers in the study of marine biology and ornithology at the Friday Harbor Laboratories of the University of Washington.

I spent the 1951-1952 year at the Cancer Research Genetics Laboratory of the Zoology Department of the University of California at Berkeley, California, under the direction of Dr. Kenneth B. DeOme and Dr. Howard A. Bern. I completed the last of the basic course work for the Ph.D. degree in Zoology; much of the required work having been completed earlier at the Friday Harbor Labs of the University of Washington during my four years as a medical student. I only needed to fill in the gaps in my prior education. I selected a topic for the Ph.D. dissertation: "Histochemistry and Electron Microscopy of the Mammary Gland of the C3H Mouse During Pregnancy, Lactation and Involution."

During the year 1952-1953, I completed a rotating internship at Highland Alameda County Hospital in Oakland, California. My clinical experience broadened to include the delivery of 153 babies, and the performance of numerous relatively uncomplicated surgical procedures including appendectomy, AK amputation, nephrectomy, cholecystectomy, simple mastectomy, resection of various skin lesions, hysterectomy, salpingectomy, orchiectomy and transurethral resection of the prostrate gland. I assisted experienced surgeons in the performance of a great variety of other more complex procedures for which I lacked the necessary experience and training to function independently.

From mid 1953 until 1962, I worked in the Department of Pathology of the School of Medicine of the University of California in San Francisco. Here I completed residency training in three fields: Anatomic Pathology, Clinical Pathology and Forensic Pathology. I joined the faculty as Instructor of Pathology and by 1962 had advanced to Assistant Professor of Pathology. During these years I continued to work on my dissertation for the Ph.D. degree in zoology. I also worked for the Coroner of the City and County of San Francisco as an autopsy surgeon, and this was the means by which I eventually qualified to sit for the board examination in Forensic Pathology. These years were very fruitful ones. I earned the Ph.D. degree in Zoology, and passed three board examinations: Anatomic Pathology, Clinical Pathology and Forensic Pathology.

The next few years, from about 1961 until 1970, were spent at the University of Oregon Medical School in Portland, Oregon. There I became Professor and Chair of the Department of Pathology, and was responsible for developing a research program in a department that had previously performed only teaching and service functions. I continued to expand my interests in comparative and marine pathology, studying the embryology of marine animals, especially hydrozoans, annelid worms and teleost fishes. It was at this time that extensive studies of skin and liver tumors in flatfish (pleuronectids) were initiated with the aid of large federal grants. At the same time, studies of the quantitative pathology of whole human breasts were begun at the medical school in Portland in order to identify with certainty the lesions of mammary preneoplasia in the human. Both of these lines of research were continued under federal funding when I subsequently joined the faculty at Davis.

I was drawn to the Davis campus because of its excellent reputation in comparative pathology. I wanted to give up administration and chairmanship in favor of teaching and research. Little did I know that this was not to be. Developing medical schools need administrators who can develop teaching and service programs; research is of secondary importance in the early years of an infant medical school. I did not at first recognize that this was especially true in Davis because of the background of Dean [C. John] Tupper and the key faculty members who came with him from the University of Michigan. The attitudes of these few set the limits of the playing field and largely determined my successes and my failures, as chair of pathology at Davis during the five-year interval from about 1969 until 1974.

As soon as I arrived in June of 1969, I discovered to my dismay that the chair, Dr. Robert E. Stowell, an academic pathologist with fine credentials and unimpeachable character, was to resign within the month, and that Dr. Rolla Hill, who was most responsible for my recruitment, was returning to become chair of pathology at Syracuse. Dr. Stowell had neglected to tell me of his difficulties with the dean or of his plan to escape by taking the post of director of the Primate Center on the Davis Campus. Furthermore, I had naively failed to ask the right questions. I agreed to become acting chair, and, subsequently chair of pathology, a post that I held for about five years.

The dean, Dr. Tupper, must receive high marks for his political savvy, which saved the school from closure during a time of statewide money shortage a few years later. I would

give him medium to low marks for his poor understanding of basic science and his tendency to promise a single resource to several different administrative units. This led to contentious displays as pathology competed with psychiatry or other units for lebensraum. Pathology desperately needed laboratory space for everyday testing. Psychiatry needed more room for overstuffed administrators, overstuffed chairs, ping pong tables and TV sets. The dean usually sided with his hand picked divisional chairmen (Wolfman, Langsley and Bolt), who seemed to regard pathology as both a threat and a money machine which they were determined to control. The dean fostered Darwinian competitiveness as an administrative style, and in a fair fight pathology might have won from time to time. Unfortunately, we existed as a subunit of the Division of Surgery, headed by Dr. Earl Wolfman, who could run a surgical service but who knew little else. In any competition between pathology and a unit with higher (divisional) status, pathology was likely to lose because we lacked the clout and the dean usually sided with his henchmen. The dean, personable and likable as he was, simply lacked the necessary academic background and perspective to function well in his high role. Fortunately, Dean Tupper was, on balance, a good politician, and without him the school could have completely failed. Fortunately, his successors were more suitably chosen to match the needs of changing times.

Dr. Loren Carlson was the chair of the Division of Basic Medical Sciences. He was an excellent scientist and administrator who was able to bring various diverse departments together. The Division of Basic Medical Sciences included primarily the departments of anatomy, physiology, biochemistry, microbiology and pharmacology. He recognized that which the dean did not; namely, that pathology should have divisional status equal to that of medicine, surgery and psychiatry. Unable to make his views felt, he proposed that pathology be a dual enterprise, with a unit for experimental pathology and teaching in the Division of Basic Medical Sciences and a second unit responsible for hospital laboratory functions. This would protect the academic and scientific status of pathology from the plebian clinical attitudes which had come to dominate the Divisions of Medicine, Surgery and Psychiatry. Unfortunately, Dr. Carlson died of a non-Hodgkin's malignant lymphoma within a year of my arrival. Pathology had lost the one man who might have been its strongest ally.

The situation at the Sacramento County Hospital was difficult because in those days our status was only that of guests. We had no administrative or fiscal control. The old guard county pathologists, however, were helpful and understanding to a remarkable degree, especially since they recognized that it was only a matter of time before the academic pathologists from the medical school took control. I did everything I though reasonable to assure their future at the medical center. Dr. Rene Hardre, Dr. Dolores Hardre, Dr. Julita Fong and Dr. Henry Tesluk were among those who preceded us and who unselfishly helped us during those early days. Lillie Murphy was the administrative secretary for the Department of Pathology at the Sacramento Medical Center. Without her knowledge of the county system and her generous help and loyalty, I could never have survived. The same was true in the case of Dick Lowe, who was the chief laboratory technician. He was helpful, loyal and efficient; moreover, he successfully defended the laboratory turf against the predations of the Division of Medical Sciences,

whose members used every divisive tool available in their quest to subdivide the clinical laboratory and incorporate it into their individual sections of the Division of Medical Sciences. I believe that their major goal was to control the money which they knew could be made from the operation of the clinical laboratory. One could easily make the case that the patients and the medical students for whose benefit the school supposedly existed, came last in the minds of some clinical faculty members. The War of the Laboratories continued unabated during my tenure as chair, but generally, we were able to hold the laboratories together.

Although I was supposed to be in charge, I had no office space at the county hospital in those early days, because we were simply guests. Dr. Rene Hardre found a desk for me and gave me a corner of his office, for which I was grateful. Lillie Murphy and Dick Lowe could be counted on to warn me of impending dangers and problems that threatened the life of the laboratory. I experienced some difficulty because my time was divided between the teaching and research environment of the Davis campus and the political and patient care world of the Sacramento Medical Center.

George Snively was the Chief of Medicine at the county hospital and was at once the most memorable and the most helpful person with whom I had contact. I was asked to dinner at his house on several occasions. His invitation was verbal, often over the phone, and always began with the question, "Do you like dogs?" To this I always answered, "You bet! I'm crazy about dogs!" You see, George and his wife had two very large German Shepherds that were always in the house, and were present at dinner time as well disciplined observers, but not as participants. George never asked people to dinner more than once if they had problems with his dogs. George also liked motorcycles, and he was very interested in the design and testing of protective helmets. In his garage he had some very elaborate and expensive gear for testing the protective value of the helmets. I often went to see him in his office at the hospital when there was something I needed for the hospital laboratory, and usually he would find the item, or the money to buy it. The hospital made a great deal of money from the laboratory, and so George was usually willing to buy equipment and supplies.

George was, like Gordon Cummings, a survivor. He had strong likes and dislikes for various people in the laboratory and the medical school at large. He seemed to have a strong dislike for Rene Hardre, but he liked Julita Fong and Al Lewis. As far as I was concerned, he was very helpful, and I liked him very much.

Research space was non-existent in Sacramento and scarce on the Davis campus. I in fact bought some of my research space in Davis with laundered money specifically allocated for other research purposes. I was very naive at the time and wouldn't have known the meaning of the phrase "laundering money." However, through creative financing, devised by Mr. Binning Chambers and Mr. Chuck Semple of the dean's office, I was able to move money around so rapidly as to totally confuse potential auditors. The end game was that the bucks were spent as we wished, and, I maintain to this day, in the most efficient manner. We were pretty proud of ourselves, but never talked about it for

fear of getting into real trouble with the funding agencies. They were getting the most bang for each buck, but they didn't know that.

My studies of mammary preneoplasia, based on quantitative morphology of whole human breasts, were funded by the federal government, and I was concerned that I would not be able to meet the research obligations while subtotally occupied in the defense of the laboratories from the predations of the Robert Bolt gang. (Please be aware that I hold no personal grudge against anyone whomsoever in our medical school. There were simply honest disagreements and we fought over them! Furthermore, I feel free to point out the strengths and weaknesses of my opponents, and invite them to criticize me in the same way). Very soon I was fortunate to be joined in the mammary research project by Dr. Hanne Jensen, a very careful and industrious young pathologist. While I was engaged in the War of the Laboratories, Dr. Jensen took charge of the mammary project and kept it on schedule. In time the results were nothing short of spectacular. We essentially cracked the histopathological code of the human mammary gland and had found the basic precancerous lesions. Our views differed from the current dogma in that we had evidence that so called ductal carcinomas did not arise in the larger ducts as formerly assumed, but rather in the most distal components of the mammary tree, which were the lobules and their terminal ducts. We were not ready to proceed with experimental manipulations.

I also continued my work on a possible viral etiology for the epidermal papillomas of flatfish and had several salt water aquaria in rooms along one of the main hallways. I don't think that this was popular with the dean whose friends and supporters were, like himself, conservative members of organized medicine. One unusual student inhabiting this laboratory was Werner Flueck whose doctoral thesis dealt with reproductive failure caused by selenium deficiency in feral Columbia black tail deer.

My greatest disappointment at Davis was my failure to recruit our resident, Dr. Richard Ikeda, as a faculty member in our department. Dr. Ikeda was intelligent, capable and likeable. I tried to help him start a research program and devised ways for him to attend national meetings as Henry Moon had done for me many years before. As soon as he was appointed to the faculty as an instructor, and later an Assistant Professor of Pathology, a remarkable and permanent Jekyl - Hyde transformation occurred. I was astounded. Dr. Ikeda was now a political animal, bent on self-aggrandizement and on character assassination of those who stood in his way. The story is long and painful and filled with accusations on all sides. What amazes me to this day was his ability to hide anger and paranoia during the residency years prior to attaining faculty status. I was completely deceived. The whole story is very long, involving years of lawsuits against the University and individual faculty members. The outcome may still be in doubt. Enough said.

My happiest time as chair was helping Professor Robert Darrell Cardiff become an outstanding faculty member and investigator in the Department of Pathology. Dr. Cardiff and I went back a long way in time, to 1958 or so. We were both products of the same graduate school, and we had both served at the University of California School of

Medicine in San Francisco and at the University of Oregon Medical School in Portland. We both had functioned in medical capacities in the armed services. Professor Cardiff is on balance the best student I have had in 35 years of teaching, and I have helped many good ones.

All in all, I think that the School of Medicine at the University of California at Davis has survived its most difficult years, and is on the verge of becoming part of one of the best medical centers in North America. The teaching, service and research bases are all there. Support and investment by outside groups such as the Shriners have further promise. Finally, the Davis campus with its strength in biological sciences, agriculture and veterinary medicine assures quality support from the basic sciences.

WILFRED E. TORESON, MD, PH.D.

CHAIRMAN OF THE
DEPARTMENT OF PATHOLOGY

UNIVERSITY OF CALIFORNIA, DAVIS

JANUARY 1976-SEPTEMBER 1977

MEMOIR OF DR. WILFRED TORESON

Based on an interview conducted on February 21, 1995

I was born in McCloud, Siskiyou County, California on Christmas Day, 1916. I was the fifth of nine children. My father, Oscar, born in Sweden, came to California in the 1880s. He attended a one-room grade school in Modoc County, where most of his nine brothers and sisters were born. My mother's parents, William Bidwell and Mary Ann Harrington Bidwell, were childhood immigrants to California, crossing the plains and mountains by covered wagon. Married in Benicia, they moved to Old Shasta, then to Burney, and then to the Rising River Ranch in Shasta County. My mother, who was named Burney Villa after the settlement where she was born, was also one of many children. She attended Chica Normal School just after the turn of the century. She taught in the one-room schools of northeastern Shasta County for several years. In 1909 my parents moved to McCloud, where my father was a sawyer in the McCloud River Lumber Company. My brothers and sisters and I attended the public grade and high schools in the town of McCloud.

There were two physicians in McCloud during the early 1900s, Dr. George Runckel, an early graduate of Stanford Medical School, and Dr. Charles C. Dickinson, a graduate of Rush. Both were greatly respected and admired there and also for many miles around where there were no hospital facilities and no [other] physicians. In those days each employee of the company paid fifty cents per month for all the medical services he might need, whatever they were! The physicians, however, did charge fees for services to the employees' family members and to non-employee patients.

Encouraged by our family tradition, I had always "done well" in school and at high school graduation I was class valedictorian. (There were only 33 in my class!). The principal and several teachers had assisted me in applying successfully for a scholarship at UC. So in 1934, I left McCloud for Berkeley, where I managed to remain for four years. I had declared a pre-med major at entrance and I had tried to qualify in both chemistry and zoology so as to have an alternate path in case I was not successful in attempting to enter a medical school. Those were still difficult economic times — I had scholarship help and a "good job" at the College Women's Club. I had also found time to play my saxophone in the UC marching band, UC Symphonic Band, and six-piece dance band. Although time given to these activities no doubt resulted in some "B" grades where I needed "A"s. I believe these activities were healthful benefits as well as forerunners of my lifelong interests in the symphony, opera, jazz, fine-foods, and gourmet cooking.

Looking back, I can appreciate the guidance and motivation I had from many teachers and professors. Meaning no offense to those I don't name here, I would like to mention Professor [Richard] Aiken's embryology course at UC and Professor Goldschmidt's cytology —both were eye-openers—exciting, stimulating, inspiring. There was also Professor Olga Bridgman with remarkable presentations in psychology. (It was my good

fortune to serve with her, much later, on the Medical School Admissions Committee at UCSF).

In 1938, when otherwise jobless, I spent the summer "pitching hay" for two dollars a day, room and board, in Hat Creek Valley. I managed to save altogether $68. One day I received a letter bearing a Canadian 3-cent stamp and containing an acceptance notice from McGill Medical School! How could I possibly finance attending there when tuition was $360 per year! Leaving many frustrations unstated, I thank our family physician, Dr. C.C. Dickenson, through whose generosity I did put together enough money to scrape by for two years. I rode the Greyhound bus from Dunsmuir, California to Montreal Quebec (at a cost of $47!).

McGill was a wonderful experience—I not only "lasted" there through four years to the MD degree, but also I had a fifth year on a Pathology Research Fellowship. Then came three-plus years in the Army of the United States (WWII). Afterward, I had four more years at McGill University Pathology Institute, during which I earned at Ph.D. and qualified in pathology via the Certificate of the Royal College Physicians and Surgeons of Canada.

I remember the medical school years of McGill with great emotion. For me, Professor of Anatomy, C. P. Martin, was not only a wonderful professional model, but also a true example of a real Christian gentleman. His home was always open on Sunday afternoons and holidays. Many of us students who were far from our families and friends found there the warmth and comfort we might otherwise have had only through memories of home. Professor Jonathon Meakins, an internist, kept his office door open to all students. He was, as far as I know, the last man to write an entire textbook of internal medicine as sole author. But more than any other person there, Professor G. Lyman Duff, my "chief" in post graduate training, a marvelous teacher of pathology in medical school, influenced the directions in which I would move throughout my professional life. It was my good fortune to receive a research fellowship at the institute he directed for one year following graduation.

It was this special year that enabled me to be sent to Johns Hopkins School of Public Health for a three-month "refresher" in Hospital Laboratory Medicine after I entered the American Army and, later to be assigned Chief of Laboratories at Camp Bowie Station Hospital, Texas. In those days, laboratories were just beginning to test for Rh-factor. And we had to maintain a guinea pig colony in order to make our own complement; a sheep, from which to secure red cells, more guinea pigs, for tuberculosis tests; and rabbits for the Friedman test. One day in 1944 we received a shiny new electronic machine and an instruction booklet: it was a Coleman Jr. Spectrophotometer, how exciting! Reproducible quantitative assays were at last nearing reality!

The A-bomb fell; the war ended.

By this time I was Director of Laboratories at the William Beaumont Army Hospital in El Paso, Texas. I had never succeeded in getting an overseas assignment anywhere. (As we

used to say "I got the Texas ribbon with raspberry cluster"). But, I also had over three years of hands-on, responsible clinical and anatomical pathology experience. I also had a wife and daughter. And I had an appointment at McGill, where I might again live precariously while gaining advanced special training in this exciting, and rapidly changing discipline.

When I returned to Montreal in 1946, I enrolled for the MSc/PH.D. program at the Pathological Institute. Professor Duff had long been interested in atherosclerotic plaques and had written a definitive comparison of the human lesions and the experimental ones that result from feeding cholesterol to rabbits. It had recently been shown that rabbits could be made diabetic by injection of alloxan. Several of us who were then with Dr. Duff set to work on various alloxan-diabetes-cholesterol atherosclerosis studies. Some of these had to do with prevention, regression and healing processes.

During my own studies, I had found that a very distinctive lesion called "hydropic degeneration" that had been observed in some diabetic patients' islet's of Langerhans, also occurred in alloxan-diabetic rabbits if they were maintained for a few months in the diabetic state. To my surprise, histologic stains for glycogen showed such cells to be reactive, although all preceding references to human lesions were reported to be unreactive. In checking patients who died in diabetic coma, with "ancient" paraffin-embedded tissue blocks and special care to limit aqueous exposure, I did find several instances strongly reactive. In later studies, I was able to show that some glycogen-reactive cells of alloxan-diabetic rabbits recovered despite long standing periods of hyperglycemia, and that some of these cells did contain beta granules—i.e., were insulin producers. Had these cells escaped alloxan-necrosis, or did they evidence regeneration of beta cells?

During these years I had become deeply interested in technical maneuvers for achieving tissue fixation without chemical exposure and especially without water. Although we had planned to pursue freeze-dehydration and carowax embedding among other ideas, moneys to support these efforts had to be diverted to seemingly more rewarding studies with cholesterol.

For this reason, and others, I decided to leave McGill and, if possible, to return to California (after essentially a 12-year absence). There was to be an opening at Stanford a year or so hence, but in the summer of 1950 I did receive the offer of an Instructorship with Professor James Rinehart at UCSF. I accepted this appointment. It might interest readers to know that my new appointment paid less than $5,000 per year.

At San Francisco Professor James Rinehart, Professor Louis Greenberg and I contemplated a study of atherosclerosis and diabetes in pyridoxine-deficient monkeys. This project never did get off the ground due to the pre-emption of most of the Rhesus monkeys then available for studies of poliomyelitic vaccines. However, after a few false starts, appropriate caging was acquired and I set out to try to make cynomolgus monkeys alloxan-diabetic. With skilled surgical assistance, alloxan was injected into the aorta while temporarily suspending renal circulation. These monkeys became acutely diabetic;

but they soon died from extensive liver necrosis. It was obvious that "attentive nursing care" would be essential for maintenance of such sick animals. As funding for such help was unavailable, the project had to be abandoned.

By this time, I was torn between research and practice. It is worth noting that even in 1950 several distinguished chairmen of university pathology departments were not medically licensed physicians. Dr. Duff, for instance, never ever had a medical license; I did not become licensed until early in 1951. But by 1953, I had tired of writing extra pages in various documents to explain to some organization or bureaucracy that my Canadian Certificate in Pathology was equivalent to the American Board of Pathology. I, therefore, went to Saint Louis that year and somehow succeeded in passing the Pathology Speciality Boards in both Clinical and Anatomical Pathology.

Also, after about a year of work in the San Francisco Coroner's Autopsy Service, I accepted an appointment as Director of Laboratories at the Southern Pacific Hospital instead. This interesting institution had survived the 1906 Earthquake—it was a museum of fascinating pathologic materials, deriving its patients from all along the route from Portland, Oregon via San Francisco and Los Angeles to Houston, Texas. In those days, the railroad also had a Tuberculosis Sanitarium in Tucson, Arizona. My real purpose at that hospital was to develop an additional facility for the residents in pathology at UCSF. For a few years this goal was actually achieved; at least one resident who started there with me, Dr. John Lee, later became a faculty member and distinguished electron microscopist at UCSF before he left the academic arena for private practice in Sacramento.

Meanwhile, the workload at the Southern Pacific Hospital had assumed enormous proportions, keeping me from fulfilling my academic research obligation in appropriate measure. I had found that I liked the practice even more than the research. When the opportunity came to join Dr. Warren Bostick, Assistant Director of the Moffit/UC Hospital Laboratories, I quickly accepted it. By this time, with help of students and fellows, I was pursuing studies of the alpha cells of islets of Langerhans, their probable identity as sources of glucagon secretion, and the "hydropic" changes found in their cytoplasm following administration of Synthalin-A. When federal money was granted, I began studying carcinogenic effects of ethionine on the livers of rats fed this chemical for several months; Henry Trowbridge, then a graduate student in the Ph.D. program, studied biochemical and histochemical changes in these animals. In another investigation Drs. Jerry Grodsky, John Lee and I described very early cytoplasmic changes in the beta cells of rabbits that became diabetic during immunization with insulin as well as "hydropic" changes that appeared later.

During the latter years of these investigations, Warren Bostick left the directorship of the Moffitt Laboratories on sabbatical and then accepted the deanship at California College of Medicine. Consequently, I found myself extraordinarily busy as Acting Director of Laboratories and member or chair of many academic and administrative committees. Those were times when new developments in methodology, instrumentation and quality control occurred with great frequency; and also when sequential analysis and

computerization became more and more important. After about a year and a half of searching for a new director, Dr. George Brecher came to UC San Francisco and I went to New York as the Director of Anatomical and Clinical Pathology in the new SUNY [State University of New York] Downstate University Hospital.

New York was a heady experience in many ways, but it was also frustrating. IBM was at work there installing a "Total Hospital Operations and Medical Information System" (THOMIS), a complex configuration that permeated every aspect of hospital administrative and medical services, including, of course, the pathology laboratories. I can say that eventually the hematology, chemistry and some microbiology services succeeded in functioning "somewhat." However, I became convinced through dire experiences that laboratories should follow a much more gradual schedule in developing "computerized" programs for their critical services.

When not occupied with computer problems as well as professional ones, I also developed a unique academic program in Medical Technology, the only one of its kind in New York State. We designed the program to accept two-year college students via equivalency examinations for university credit and we provided in-depth classes in chemistry, microbiology and immunology while offering employment in the laboratories over a period of three years. Our graduates were able to qualify for the ASCP - MT as well as for the baccalaureate degree.

It is my impression that the State University of New York was trying to grow academically along lines that would somewhat resemble the University of California. "Downstate" was similar to UCSF in that it consisted during the late sixties of the schools of medicine, nursing, and health-related sciences; and a graduate school offering advanced degrees in anatomy, biochemistry, microbiology, physiology and pathology. The faculty organized itself pretty much like any UC Faculty Senate. I was elected president of the faculty there, an obligation that presented many frustrations. In addition, the pathology department set out to incorporate for medical practice, and this was achieved only after considerable awkward, puzzling and time-consuming difficulties. I believe it was the first incorporated pathology practice in New York.

Early in March of 1971, I suffered a coronary occlusion from which I made a good recovery without appreciable physical restrictions. I was then in my 54th year, I had been in New York five years and I was already committed to returning to California, specifically, to UC Davis School of Medicine. I had once visited Davis about an appointment, with Dr. Stowell, but at that time I had barely gotten acquainted in New York. Later on, Dr. Wellings and I had conversations and correspondence about my coming to UCD and we had agreed on a starting date, July 1, 1971.

One of the factors that attracted me was an experience in the early sixties when the Northern Section of the Academic Senate of the University considered the possibility of a medical school at Davis. That proposal envisaged teaching "basic sciences" to medical students by the faculties of the undergraduate and veterinary programs, followed by transfer of the whole class to San Francisco! At that time I represented San Francisco on

the Educational Policy Committee and I was asked to write the committee's response. A few years before then the "basic sciences" had returned to San Francisco from Berkeley, where they had been "temporarily" located for fifty years! The Educational Policy Committee endorsed my response, which favored a four-year school from the outset, preferably with its own hospital at Davis.

When I arrived in Davis [in 1971], I could see that most of my time would have to be spent in Sacramento at the County Hospital. I knew that Drs. Marshall Barnes and Robert Cardiff were also on the way. I no longer had any pretenses about establishing "my own" research program. Dr. Wellings had thought I might work with him and Dr. Jensen, studying serial whole-breast sections to determine distribution and varieties of lesions. It turned out that my guess about the hospital had been correct. I was busy enough there, and, with Medical School Admissions Committee interviews and meetings, I was seldom available to work on the research project with them. I found that developing the Resident Training Program and the clinical services in Anatomical Pathology were challenging enough to require much time and effort; besides, the many conferences at which pathologists' services were needed did much to enlighten and enliven every day.

And so professional life went along until one day we learned that Dr. Wellings had left his chairmanship. Dean Tupper asked me to consider being the Active Chairman while a search was made for a new appointee. With great misgivings, I finally agreed to do so with the stipulation that Dr. Cardiff would be in complete charge of the academic/research activities in Davis and I would be responsible for affairs at the hospital in Sacramento.

Very soon, afterward I received notice of a meeting of the Hospital Staff Executive Committee to discuss transferring responsibilities for various clinical pathology laboratories from pathology to internal medicine. I responded to the notice by refusing to attend. After several weeks a Staff Committee was appointed, chaired by Dr. John Palmer, to consider the question of re-assigning clinical laboratory responsibilities. I did approve of the committee and its assignment. To the best of my recollection, this committee report was never publicized. In any event, no actions were taken until much later, when Dr. George Lundberg had accepted appointment as Chair of Pathology and Director of Laboratories.

Shortly before Dr. Lundberg's arrival the Accreditation Committee of the American Society of Clinical Pathologists visited the hospital to review our residency training program. Although I knew it was my responsibility, I had failed to realize that some essential forms and documents had not been properly assembled. The reviewing committee, recognizing the stress I had been under (from the increasingly severe deterioration of my wife's health, due to the spread of her cancer), did not disapprove our program, but gave it instead probationary status.

Shortly after I relinquished the acting-chairmanship to the new chair, Dr. Lundberg, Mrs. Toreson succumbed to her illness. I then went to Quebec where I took part in a special

convocation honoring Dr. Duff. Almost all of his graduate students attended and presented special papers in contemplation of a Festshrift. Upon returning to Sacramento I found my home had been thoroughly burglarized. I then decided to take a sabbatical "in residence" starting in January 1978. However, about three months into this program, oriented toward immunopathology, my cardiac status deteriorated and I underwent a bypass operation that entailed several weeks of post-operative "indigence." Toward the end of this time I suffered hepatitis-B from the transfusions received at by-pass. This was a very demoralizing experience, from which I never did succeed in returning motivationally to an appropriate level of academic activity. I simply tried to fulfill my clinical and teaching obligations, but made no further efforts to involve myself in research.

At the end of 1982, I began a "semi-retirement" period—really utilizing the months of vacation leave I had accrued through the years.

In retirement I have made no effort to maintain my participation in teaching, research or practice of pathology. I have found a very different new life. After my wife Dorothy died and I had recovered from the by-pass, the hepatitis, and other assorted serious illnesses, I experienced a true conversion to the Christian faith, which I celebrated with baptism and a social reception that friends and some former colleagues attended. Not long afterward, I began to find that many special activities in the church attracted my interest and participation. As time has gone by, I have narrowed these involvements down to only a few. The main one is eucharistic ministry, especially to severely handicapped younger patients at a skilled nursing center. A secondary one has emerged because of my interests in Scripture and teaching; I assist another person in conducting classes for adults, most of whom are just beginning to read the Bible.

In August 1980, I was re-married to Mary; we are about to celebrate our 19th anniversary. An attractive home, new to both of us, delights us; we enjoy my daughter's visits and Mary's son's family. We have two wonderful granddaughters. I have a nice Koi pond with some beautiful fish in a Japanese landscaped garden. I cook dinner for us most evenings with occasional festive dinners for a few guests.

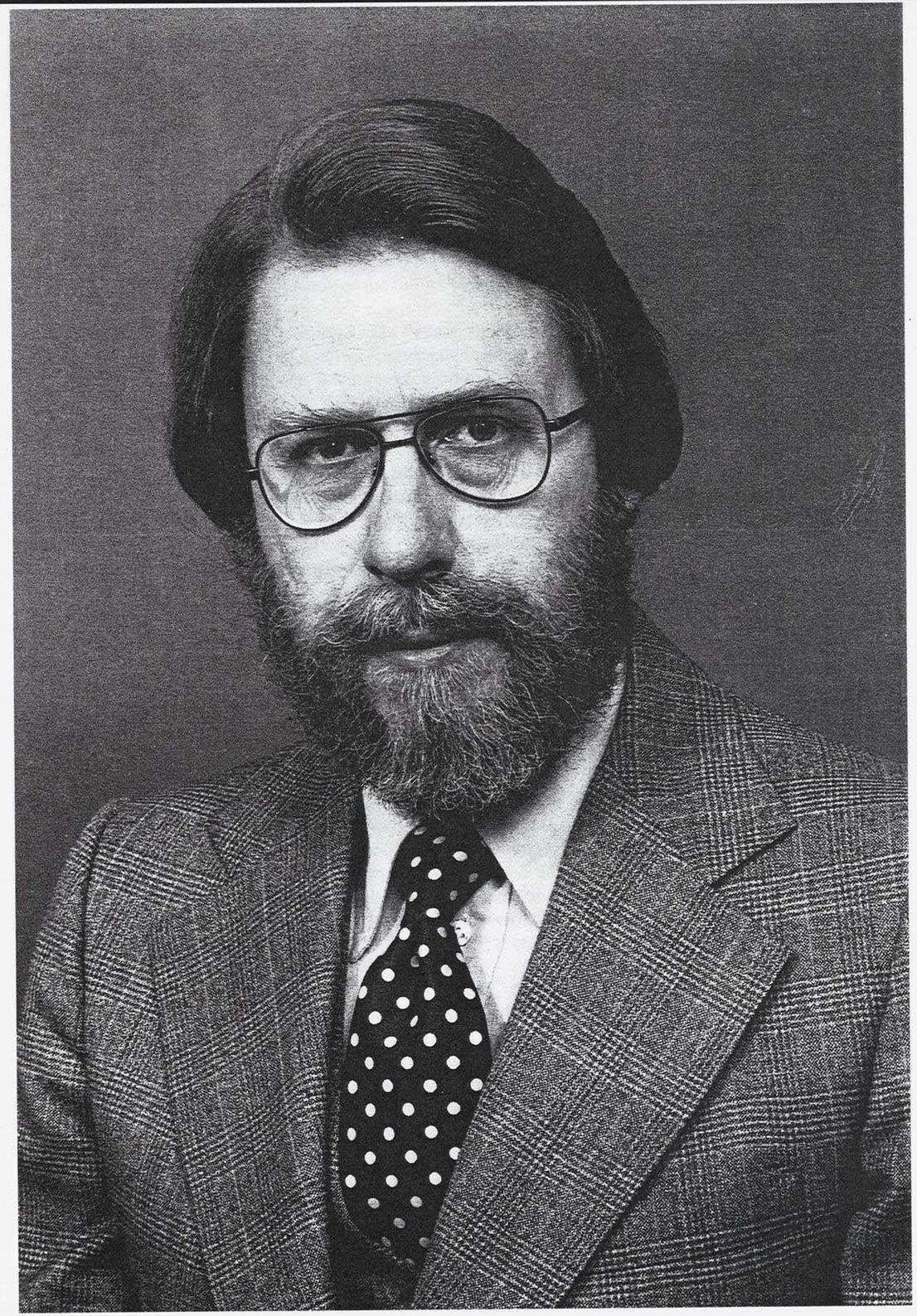
Sometimes I say that I ought to go back "to work" so I might have more time!

GEORGE LUNDBERG, MD

CHAIRMAN OF THE
DEPARTMENT OF PATHOLOGY

UNIVERSITY OF CALIFORNIA, DAVIS

SEPTEMBER 1977-JANUARY 1982



Joann Larkey Interview with Dr. George Lundberg

March 3, 1995

JL: We are going to talk today about your role while you were chair of the Department of Pathology at UC Davis School of Medicine, but first, why don't you tell me a little about your early life: your education and some of the things you did before you got to Davis.

GL: I was born on March 21 in 1933 in Pensacola, Florida at a Catholic hospital, of a mother who had no prior children and who was in her late thirties. My mother and father were first generation Swedish immigrants, having been born in the United States, but of parents who had themselves come from the south, middle and north of Sweden through Chicago. My mother's mother and father were sold land in south Alabama and they went south in 1895, approximately. My mother, Esther Louise Johnson, was a baby in arms when carried by her mother who had been trained as a nurse midwife at the Swedish Covenant Hospital on the north side of Chicago in the 1890s. Arriving by ox cart after ferry, after train, in Baldwin County, Alabama, they proceeded to cut down trees that had never been cut before and establish the beginnings of a community in the wilderness. My mother's father died within the first year, probably of tuberculosis, which he had allegedly picked up while shipwrecked off Madagascar off a Swedish boat. He immigrated to the United States by having been picked up by an American sailing vessel off Madagascar going to New York. He'd been floating on some wreckage from his boat, or so the story goes, in the Indian Ocean around 1890. So he died within the first year in Alabama and my grandmother was left with a one-year-old child and no means of support except as a midwife, and there weren't many people around having babies. Not long thereafter, a Swedish merchant came to what now was a very small town, and he had three daughters and no wife. His wife had died. So they got together and married and he then was my grandfather, whom I knew as I was growing up in far south Alabama, halfway between Mobile and Pensacola.

My father, George David Lundberg, got down there in a different way. His parents had a bunch of kids in Chicago, but one of the parents had asthma and was sent south to get away from Chicago winters. He being the youngest son was assigned to go with them. He was about seventeen when he moved to south Alabama. My mother and father met and married in Silverhill, Alabama in 1920,and earned their living as violin and piano teachers. Later, my mother was a schoolteacher as well, and they were, in a sense, the performing arts culture for that county in south Alabama for about twenty-five years.

I was a latecomer. I was born in Florida, but only because my mother had a problem in her delivery, her labor. There was no hospital anywhere around where they were, so her doctor said she should go to this hospital in Pensacola, where she went. And I was born there. I'm a native of Florida, but moved back to Alabama in a few days. My mother went back across the border, it was just the accident of having a hospital close by. Mobile was hard to get to because there was no bridge across the bay. It was necessary to cross the bay by a ferry and that was tough for a woman in labor. There were automobiles in 1933, but the roads were not paved and it

was difficult, so Pensacola was easier. That's why I happened to be born in Florida. That's a long answer, but it's the quickest one I could give for a fairly complex sociologic situation.

JL: It's interesting about your Swedish background on both sides of your family.

GL: My mother was a powerful, oppressive teacher and pushed me hard, including summer school and pressure to skip three grades. I skipped fifth grade, eighth grade and eleventh grade, and I finished high school at age fifteen.

JL: Did you go right to college?

GL: I went to North Park College in Chicago, because it was a church school and I grew up in a far right, fundamentalist Swedish Protestant church. They had one college called North Park. Since I was so young, my parents thought it would be better to go to a church school where I would be supervised and kept controlled, and that occurred. North Park was a two-year school, so I finished at age seventeen with an Associate of Arts. I went back to Alabama, to the University of Alabama in Tuscaloosa, for the next two years, and graduated with a Bachelor of Science Degree in Chemistry, having been in pre-med for the whole time.

I decided to become a doctor because of the influence of a kindly family doctor around the time I was four or five years old. I was a sickly child. I saw the doctor many, many times and he was a wonderful man. His name was Dr. Henry Jordan, and I wanted to be like him. And so from before I went to school for the very first grade, I was going to be a doctor.

JL: You made the decision that early!

GL: Never did change. But originally I was going to be a family doctor, which I never have been. I tried to go to medical school after three years of college—it was possible in those days—but I was turned down. After four years of college I was turned down, so it was hard getting into medical school back then. It was after the Second World War and right after the Korean War. There were a whole bunch of people who had come back from the military who were on the GI Bill in college and the competition was fierce. I then did a year of biochemistry work at the University of Alabama in Tuscaloosa, during which year I was accepted into medical school at the University of Alabama at Birmingham. At that time it was called the Medical College of Alabama. I went to medical school, then, in 1953. I did not get a masters degree in chemistry then, because I went to medical school instead. I did, however, become a med-tech in the process, and started one of my early careers as a chemistry medical technologist.

JL: So, were you working your way through school part-time?

GL: I did a lot of work. My parents never had much money. Music teachers in the rural south, and a school teacher paid by the State of Alabama, were about as low-paying positions as you can get and be professional. Even though they had no other children, they had little money. They lived okay, but it was important for me to do work, not during my first two years at college, that was okay, but the second two years I worked part-time the whole time.

Then, after I got accepted into medical school, I worked full-time for six months, saving money as a med tech at a new hospital called the Druid City Hospital in Tuscaloosa. It was a brand new Hill Burton funds hospital, at which I was the first chemistry lab tech. In that hospital, because of the virtue of the timing, I set up the first flame photometer in western Alabama so we could analyze blood for serum potassium, sodium, chloride, and things like that, which had never been possible for that part of the state until this new technology [was developed]. So that was interesting. I came to be befriended by a pathologist at Druid City Hospital. His name was Dr. James Simon, called Peter Beck. He had been trained in Worchester, Massachusetts. He had been in Alabama for a long time and he was the pathologist for this hospital. He taught me how to be an autopsy assistant, in addition to being a chemistry tech, and that relationship with him lasted for three years. I would go back in the summer during medical school for at least two years to work as a replacement chemistry lab tech or an autopsy assistant in the summertime when medical school had a break. And he also taught me how to become a pathologist's assistant, cutting surgical pathology, and he began to teach me microscopic pathology between my second and third years of medical school. So I was, in fact, a pathologist's assistant before they ever had such a thing. I learned a lot of pathology, and I am sure that that exposure there was the main reason I went into pathology. It was a mentoring experience that lasted for years.

JL: In reading your curriculum vitae, I notice that you did become associated with the military. Did this occur while you were still in medical school?

GL: I met my wife-to-be, Nancy Ware Sharp, during my sophomore pathologist's assistant time in Tuscaloosa, and we married the next summer, between my third and forth year of medical school. At that time, there was a doctor draft still on, so the likelihood of being drafted into the military for people who had not served in the military was very high. And my wife got pregnant very early in our marriage. I was a senior medical student with little or no money, a wife who was a college student who was pregnant, and there was a doctor draft. So, all those things together caused me to respond favorably when the United States Army sent recruiters to the medical school class to try to talk people into becoming volunteers in the army. I volunteered, was accepted, and became a second lieutenant just before my last year of medical school. They paid me as a second lieutenant on active duty my entire fourth year of medical school.

JL: That helped a lot, I'm sure.

GL: It allowed us to live decently. I then applied for internship at a variety of places in the Intern Matching Plan. I had two or three choices written down, with my first choice being an army rotating intern program. My second choice would have been internal medicine at the University of Alabama at Birmingham with Tinsley Harrison. I matched with my first choice, and within the army you were then also sub-matched. There were several teaching hospitals. My first choice was their teaching hospital in Honolulu, Tripler Hospital, and I was matched with the first choice there also. Finishing medical school, I was promoted to first lieutenant, and

my wife and child and I were then taken to Honolulu where we lived and worked for a year.

JL: Nice experience?

GL: It was a wonderful internship and a great hospital. Great place. Marvelous living experience. We lived in Waikiki in a small apartment. The hospital was an Army-Navy-Air Force-VA and Public Health Service Hospital for the entire Central Pacific, and it was a great place. It was an outstanding year. During the year, however, my father had a heart attack, and southern Alabama is a long way from Honolulu. There were no jet airplanes yet that you could fly on, so it was slow. The Red Cross helped me maintain contact with my father. I did not go back to Alabama. He got over his heart attack. But because of that heart attack, I gave serious consideration to not staying in the Islands after that internship. The choices were to either stay in the army to pay back three years, two years for draft time plus one year for my fourth year of medical school, and then leave the army and go on about my life. The other option was to go into a residency out of internship and stay in the army but incurring additional payback time by doing a residency. Because of my father's heart attack, I opted to apply for residency, because the army was going to assign me to a standard general medical officer job just out of Honolulu had I stayed there, had I not done more training. So I chose to do more training and chose hospitals back on the mainland United States, and I got matched with Brooke Hospital in San Antonio, Texas, which was actually the closest one to Alabama anyway. So I chose to go back there.

JL: That is also a very fine teaching hospital.

GL: It was a good teaching hospital and an interesting place in a nice town. My idea was that I would go there and do one year of pathology, which at that time was applicable to surgery, medicine or pathology as a year of training. If I liked it I'd stay in it, and if I didn't I would go on and move into something else. But I liked it a lot, and I stayed in pathology at Brooke for four years, during which time I also got a Masters Degree in Pathology at Baylor University for doing coursework, a thesis project, writing and defending a thesis during the residency program.

JL: What was the subject of your thesis?

GL: The thesis subject was "Mixed Erythrocyte-Leukocyte Agglutination and Its Role in the Serologic Adhesion Phenomenon." It was part of a larger project being done by Bob Greendyke from Rochester, New York, who had been drafted, or perhaps he was in a Berry Plan after residency in Rochester. He worked with me and with another of my fellow residents, Bob Brierty, on erythrocyte phagocytosis. Fairly fundamental immunology—how antigens and antibodies work around red cells in erythrocyte phagocytosis and the mixture of how they agglutinate and whether compliment was or was not required for that agglutination. It was a nice, finite piece of fairly basic science, written as a thesis. It was published in a larger paper in the Journal of Laboratory and Clinical Medicine, a very good journal.

JL: Was that your first publication?

GL: No, my first paper was in the New England Journal of Medicine in 1961 on "Mucormycosis in Severely Burned Patients." Erwin R. Rabin who was from Washington University in St. Louis, and I, and I think one other person, T. M. Mitchell, had discovered mucormycosis, a major fungus infection, in a burn patient that I had autopsied, and we went back in the files and found another one, wrote them up with photographs, and the New England Journal of Medicine published them. The paper in the Journal of Laboratory and Clinical Medicine came early, but it was a few papers later, after the thesis had been finished.

JL: So you received this advanced M.S. degree from Baylor in 1964?

GL: The work was finished by 1962, but then we had to go through getting the thesis done and approved and all that stuff. I think the actual date on the diploma was 1964, although by that time I had been in San Francisco for a couple of years.

JL: And I noticed your degree was given with honors, so you must have done well.

GL: Honors, right.

JL: You also took time out to take the American Board Exams for both Anatomical and Clinical Pathology in 1962.

GL: I had my full four years of anatomical and clinical Pathology at Brooke, and I took the boards as soon as I was eligible, which was in November of the year I finished, 1962. I passed the first time.

JL: That's always an achievement.

GL: It was significant. Of the various steps in one's professional life, the occasion was a significant one.

JL: And I see that between 1962 and 1966 you took a number of interesting courses: Histochemistry, Forensic Medicine, Personnel Management for Executives, and one on Computer Research.

GL: Two of those courses had a major influence on my life. After serving two years on pathology staff duty at Letterman Army Hospital in the Presidio of San Francisco, living in San Rafael, Marin County, the army assigned me to William Beaumont General Hospital in El Paso as the chief pathologist. So I was only two years out of residency and I was in charge of a 400-bed teaching hospital laboratory with a residency program, a med tech program, the whole bit.

JL: That was a heavy responsibility.

GL: Two years out of residency. That was one of the good things. Again, a lot of good things have happened to me in my life. In the military I was able to take an awful lot of responsibility very early in a career, much earlier than one would in most academic careers. I did that. I didn't

really want to go to west Texas, but when the army says "go," you go. You don't have any choice. I would have preferred to stay at Letterman, but.... So in those three years I was at Beaumont, the Vietnam War was beginning to heat up. We built the residency program. We tripled the number of residents. We put in the first electron microscope of any army teaching hospital, and we began to computerize the lab for the first army hospital to have computerization. In those three years we did all that. I also became friends with a lot of leading pathologists whom the army had the money to bring to town to be teachers for my residents. One of those leading pathologists ended up recruiting me to go to his place after I left the army.

I left the army a Lieutenant Colonel after eleven years service for two main reasons. The first was that the Vietnam War was getting going and I didn't like the war at all—I don't like war anyway—but I specifically didn't like that war.

JL: You weren't alone there.

GL: But early on not many people knew much about it. Later on it was very unpopular. I served an extra year in the army because of the Vietnam War. They weren't letting anybody out. But I was not assigned to Vietnam.

The second reason I left the army was that I'd moved several times, and by that time had three children. Two of them were in school, and I wanted to control my moving more myself rather that have the army do it. My next job in the army was already determined by 1967, and that was to be the commanding officer of an army area lab in Camp Zama in Japan, near Tokyo. That lab became the lab that gave support for the entire Vietnam enterprise. So I would have been responsible for the blood program and all lab support for Southeast Asia.

JL: You would have been there for a long time, no doubt.

GL: A long time. Out in Japan, but in charge of everything. It was one of the biggest jobs they had, and it would have been enormously valuable in an army career for advancement and so forth, hugely important. But I said, "No, I've served eleven years. If I can get a good job at an academic institution I'll take it and leave the army, because I'd always wanted to be a professor or teacher somewhere.

JL: That had been a long-term goal for you?

GL: Long-term goal, in general. Specifically, to be a path department chair became a goal about half way through my residency program at Brooke Hospital. Probably when one of my fellow residents said I ought to be a chairman somewhere. I said, "Really?" He said, "Yeah, you have the ability." And from that moment forward I thought maybe I should. That was about 1961, probably, when that goal was set. So when it was time to leave the army and I could get out, I wrote around to pathology departments in decent cities in warm weather climate areas. I don't know, I sent out ten or fifteen letters. I got invited for interviews to five places, and I was offered jobs at all of them: Emory in Atlanta, Alabama at Birmingham, Baylor in Houston, Southwestern in Dallas and USC, University of Southern California in Los Angeles. I

considered all of them and accepted the one in L.A. for a lot of reasons. My family and I moved to Pasadena to live for the next ten years, starting in 1967.

JL: Who was in charge of recruiting you for that job?

GL: Dr. Hugh Edmondson.

JL: He was chair of the department?

GL: He had been at USC since 1933, and he had been chair for many years. He was an old man from Oklahoma and/or Arkansas, right on the border, a marvelous man who influenced me greatly at that time in my life, as he did Murray Gardner whom he also recruited in 1963. Edmondson managed to come up with buildings and money and all kinds of stuff for Murray. For me, he came up with an associate professorship with tenure for my first academic appointment.

JL: That was rather amazing.

GL: I never was an instructor or assistant professor. I insisted on that as a condition of coming and he agreed. I had published twelve papers by that time in the army, and he had been with me in El Paso and watched me.

JL: Was he one of the instructors you brought in?

GL: Yes, he was one of the professors I brought in. We got along well. He saw how I ran a lab and how I taught. He got to know me and he was very responsive to recruiting me and very effective at it. He had the personal touch to recruiting. For example, for my second visit out there, he made sure it was during the holiday time and had me stay at his house in Pasadena and got Rose Bowl tickets, Rose Parade seating spots, and it was marvelous. He was a wonderful man—a very human man. Damned good pathologist, and an interesting administrator. I learned a lot of stuff from him. We had very different styles, but he was awfully good. So I worked with him for five years at SC. I went to SC to do mostly teaching and research.

JL: What were your areas of responsibility were when you first came?

GL: When I first started I went there to do teaching and research. Research in drug-induced diseases, primarily. He got me a half-time position at the county hospital so I could get pay from the county as well as the university. That was an autopsy position. But after a year, the assistant director of Laboratories and Pathology at the LA County-USC Medical Center resigned from his position to take a job out at Burbank as director of labs at St. Joseph's Hospital, and that position became available. Dr. Edmondson had seen how I had functioned in El Paso, and he asked me to become the assistant director of Laboratories and Pathology under his direction. He was chair as well as director, but he mostly did chair work. He didn't do much laboratory directing work. For practical purposes, I ran the labs at the "Big County" medical center from that day forward for the next nine years.

Dr. Edmondson retired after five years and Dr. Nancy Werner became the chair of pathology at USC. I stayed in my same job. She gave me a new title of Associate Director of Laboratories and Pathology, and I basically ran the labs while she was the chair.

JL: Was the medical school at UCLA involved in the labs at the county hospital?

GL: UCLA had responsibility for Harbor General Hospital, and USC had responsibility for the LA County-USC Medical Center. There were several other county hospitals—about nine of them actually—and some of them had no tie with any medical school. When Martin Luther King Hospital was built, USC and UCLA shared responsibility, but the pathology part of it was USC, so I helped to recruit Eli Amador to come out there and run the labs at the Martin Luther King Hospital, and he's still there. We brought him from Cleveland.

The time in Los Angeles was a fabulous time in many, many ways. I got into toxicology in real depth. I had come there to do work in drug-induced diseases in *in vitro* settings in animals, but found that there was a most natural human experiment going on every day at the LA County-USC Medical Center—people admitted with drug-induced diseases. We would get thirty to fifty people admitted every day with drug problems, so the thing from our standpoint was to build a laboratory that would do the clinical lab support base for all those patients with overdose. We did that. We got the money from the board of supervisors because of Art Linkletter's daughter's death. Remember Art Linkletter's daughter?

JL: Yes.

GL: She fell or jumped off a high place down there when she was high on LSD. And out of the response from the public, the board of supervisors wanted to do something about drug abuse. So we said, "Well, build us a lab. Give us the money, we'll build a tox lab so we can identify what these people have been taking when they come in to the hospital," and they did that. That lab is still there. It quickly became the best clinical toxicology lab in the world because we had good people, huge amounts of patients to have to do work on, and a lot of administrative support. So for those nine years, I worked very actively in clinical toxicology, in addition to computer applications to all the labs.

I wrote a book on laboratory management around that time, which was my first book.

And then, of interest, the county needed a committee to help advise the director of hospitals for all county hospitals in what to do about his laboratory problems, so they founded the Laboratory Standards Committee. Actually, my predecessor, Dick Horowitz, founded that committee, but I succeeded him as chair of that committee. For several of those years I ran a committee that advised the Department of Hospitals on what to do about labs for the whole county, which was nine hospitals. We also consulted with and advised the chief purchasing agent for the county of Los Angeles regarding what labs to send overflow lab work to, and we created the concept of competitive bidding for laboratory overflow work and ran that for years.

All times are interesting—those were very interesting times. I could easily have stayed there for the rest of my life. There was plenty to do.

JL: Weren't you also director of the Medical Technology School at USC.

GL: I directed the med tech school. We had thirty-six students. I ran the pathology residency program most of the time. We had more than thirty residents. And I directed pathology education for the School of Pharmacy. I created the whole program in pathology and lab medicine because they were going to do a Doctor of Pharmacy Degree instead of a Bachelors of Pharmacy Degree, so we had to have pathology.

JL: That was a new program?

GL: It was brand new. Edmonson asked me to do it because I was interested in the drug interface. So I got to be real friendly with the dean of the pharmacy school and I used pathology residents as the teachers of pharmacy students.

JL: Did that work well?

GL: It worked beautifully. And we were able to pay them a little supplementary money for lectures. It gave them the opportunity to learn how to teach more. I brought some of them along so that they could learn how to administer teaching programs and supervise other residents. It was a perfect laboratory, again, for teaching people how to teach. It worked very well.

JL: Were you involved as part of the research team for the Virus Cancer Center?

GL: No. I had nothing whatever to do with that. It was Murray Gardner's entire game and he worked with Bob Huebner, from the National Cancer Institute, on contract money, primarily. Infectious disease, cancer research and so forth, I had nothing whatever to do with. That was Murray's game and a lot of peoples who worked with him. Mine was the clinical lab, the residency program, the med tech program, toxicology and computers.

JL: So you were using computers pretty much in all labs by then?

GL: We set up the first ones in the labs and ended up spending many millions of dollars as new systems would come by to try to make them ideal. They never were ideal, but we accomplished a great deal in the process.

JL: Didn't you make a change in your career in 1977? What prompted you to consider the position as chair of the Department of Pathology at UC Davis? You've said that being a department chair was a long-term goal for you, but had you considered other medical schools as well?

GL: I was tired of not being chair at USC. I thought I ought to be chair there, because I was doing most of the work. I was sure I could do it, but the committee process had not done that.

Instead it had appointed Dr. Nancy Werner, who was a fine person and did a very good job. And she was healthy and looked like she'd be staying there for a while, so it looked unlikely that I would get the chair at USC. That meant it would have to be somewhere else. I was approached from time to time, but not often, at that point, but I was about to the point of becoming probably generally available.

I was then approached by people at Davis. I had been up there once before. Bob Wellings had tried to recruit me to leave USC and come up there to run his clinical lab at the Sacramento Medical Center. I don't remember what year that would have been, but it was probably something like 1974. So I knew the Davis scene. I had met Bob Cardiff and I liked him, and I had met some of the other faculty, and seen the hospital. I had rejected that opportunity to come and do that, but the chair was a different question. It was the University of California, which is a great university with many campuses. And, although this was a young one, it was in a relatively small city, and it seemed open for a lot of opportunities for growth and development. So I looked at it seriously. They obviously liked me and we struck a deal rather quickly after I had come back from a sabbatical in Sweden and England.

JL: You had gone overseas then, to do a sabbatical?

GL: I did a sabbatical from USC in Sweden and England. It wasn't long thereafter that this opportunity came up and we struck a deal. In addition, the chair of the Search Committee, Earl Wolfman, was a very convincing person. He was chair of surgery at the time and John Tupper was the dean. "Tup" was a great salesman, very enthusiastic. I'll have nice things to say about him this afternoon at the Pathology Forum. He had talked a lot of people into coming to help start this medical school and it was still in that mode, although it was well into being a school.

JL: This was 1977 that you were considering coming?

GL: Yes, the consideration occurred in 1977, and the move occurred in '77. It happened fairly quickly when it happened. So I came in September of 1977.

JL: Were there things that you asked of Davis that they didn't have in place at the time that you came? I was curious to know when the Department of Pathology was given a status on its own rather than be part of the Division of Surgical Sciences.

GL: Pathology was always a free-standing department, but it was in a division. The division concept did not mean it wasn't a department. It just meant that "Tup" had set the school up to have divisions, which had superheads. So there would be a division director who was also department director. There would be multiple departments within a division. It didn't in any way impede the concept of being department chair. The departments were still free-standing departments; they just happened to funnel through a division.

But, frankly, the division system wasn't working all that well, and I didn't see it as either a plus or minus at that point. I think probably "Tup" set it up in a division system because it was all faculty and he wanted to have division leaders who would be magnets for faculty recruitment,

in general, while he would be out raising money and trying to keep the politics of the school going. It was probably a wise strategy to have divisions to start with, but shortly after I came, they really amounted to nothing. The relationship was the chairs to the dean. The division director control was a low order of magnitude.

JL: So you reported directly to the dean?

GL: Right. I felt like I always did, although Wolfman had recruited me and we were friends and still are. I did whatever I had to with the division, but it seemed more window dressing than a serious administrative arrangement by that time. Earlier it was probably a serious administrative arrangement, but shortly thereafter it really wasn't.

JL: Well, all the departments had grown considerably in that space of time.

GL: They had.

JL: How many faculty members were in pathology when you came?

GL: I can't give you the answer. Catalogues for those years would have that answer. That would be historically valid. My memory would not be. Answering a question like that is always very hard anyway, because you have all these different categories of faculty. They're tenure-tracked, and there's political faculty and there are volunteers and there are joint appointments.

JL: You also have lots of highly trained technology staff in pathology, particularly.

GL: It was a large group of people. On the other hand, it was a third as large as what I had left at USC. In fact, at SC I at one time, I supervised 750 people. It was my goal from then forward to always supervise fewer. The best job is the one that has enormous scope and importance and no supervisorial responsibility at all. That's the best job. But I've never quite gotten there.

JL: Does that exist?

GL: It's hard. It does exist. There are things like that. But they're pretty rare. So I can't tell you numbers, but catalogs would. If they haven't thrown out the catalogs. They've thrown out all my progress reports.

JL: I was impressed that you said you'd made an annual report to the Dean's Office. We're going to try to find those, but they haven't been located yet.

GL: I found one. (Looking at report.) Here are faculty numbers from something....Actually, Anita Moore may have sent me that. Here's a reference to an open house that my wife at that time and I gave honoring Dean Hibbard E. Williams when he came as the dean. We had 82 people at either 4:00 or 5:00pm at the open house. They were all pathology department members in one form or the other and their spouses. So that was in 1981, right at the end of my time here.

JL: When you came to UC Davis, you took over from Dr. Wilfred Toreson who had been the acting chair of pathology for a year and a half. He had also managed the clinical laboratories at the Sacramento Medical Center. That med center was still in great transition when you arrived, wasn't it?

GL: The best I can tell, it's always been in transition in one form or another, and probably still is. Yes, it was in transition. I think that I was recruited primarily because I had a long history of solid efforts in running hospital labs. In the army for years, and then at USC, that had been the main thing I did. The people who had been recruited here previously had been strong in teaching and research, but had not been particularly strong in service pathology or administration of hospital labs. I think that was seen to be a weak spot by the powers that be at that time. That was probably why I was recruited. Also, a new hospital director had been recruited. His name was Robert Smith. He had come from UCLA's Harbor Hospital, but before he had been at Harbor, he had been a top administrator at the L.A. County Medical Center for a short time, and I had known him there. So my guess is he had seen the work I had done there and he had influenced the Search Committee and the dean here to go after me because of my experience level in running complicated hospital labs in a university setting. So it was probably Wolfman, Tupper and Smith together that decided I would be the right person. I saw this as an opportunity in the hospital lab setting which had not been well developed up to that point. As an opportunity to apply the skills I had learned at prior jobs and bring it around rather quickly into a solid, good and well-performing clinical laboratory, which the hospital deserved. It not only deserved it, it had to have it. It couldn't survive without that. So I saw that as an opportunity to do something I knew I knew how to do, and to have fun doing it.

JL: Were you able to get new laboratory facilities to accomplish these goals?

GL: When one is recruited somewhere, it gives a person the best opportunity they'll ever have to get concessions from the administrators who are recruiting them. Among the things that I required as a condition for coming was a lot of new space.

JL: Adequate space for pathology at the hospital had been a long-standing problem.

GL: It had been a terrible problem for a long time. And it wasn't unique to here. As I went around the country, many hospital labs were very cramped. Because there was a 10-15% increase in laboratory productivity in most hospital labs in the United States, even 20% per year in teaching labs, and labs couldn't keep up. The first thing you have to have to build a lab is space. They had run out of space a long time before. Nobody figured out how to deal with that. So we looked around and came up with a whole building which was being used for other things at the time. It was two stories and a basement. We got the first floor right away, then the second floor later. It was committed originally. Then we never did use the basement. It was for storage. But that relieved the acute space problem very well. It allowed us to do what we had to do to grow. That was critical. Had that not occurred, had we not gotten that kind of space, number one I wouldn't have taken the job. Number two, if I had, I wouldn't have been able to

do anything in their service lab area because there just wasn't any place to do it. So that was critical.

JL: Could you describe your first administrative offices?

GL: The first office I had was on the Davis campus—I always had two. There was one in this Medical Sciences Building, which the chair, Dr. Cardiff, had designed. It was fine.

JL: The Medical Sciences Building was brand new in 1977.

GL: It was brand new, and we were just moving in when I was arriving. We were in a one story temporary type structure when I first got here, but within a few months we had moved into Medical Science One, which had been planned long before and constructed with its completion just there. I had nothing whatsoever to do with MS1 except to occupy it.

JL: Your timing was good.

GL: It had all been done before. The good part, to occupy it, that was the easy part. And then try to use it well. I think we did, because the use of MS1 enhanced the research activities of the department. Although I was here to emphasize service and teaching, research flourished. We did very well in research. In articles published, number of post-doc fellows, grants, awards, etc., all increased very nicely during my five years as chair. But I had little to do with that except not messing it up. I provided background support and symbolic support and relatively smooth administration so that people could do their research without being interfered with by anybody, which is the chair's main job, as I see it. If you have the good investigators who go out and get their own money, which we had, I inherited those, I didn't recruit those, that was fine.

But in the hospital lab area, you can't do anything unless you have space. And they didn't have space. We got space. And that helped a great deal. I tried to figure out where to have an office in the medical center. I had to have one, obviously, because I decided I'd be the director of labs as well as chair.

JL: Yes. That was a heavy responsibility, to take that over.

GL: Yes. But it had to be done. There was a model at SC where the chair had been director of labs and it worked. I thought it was the most sensible thing because that's where the largest number of people were, the largest amount of money, the largest amount of responsibility, and so if I was going to try to assure that the clinical and anatomical laboratories worked, I wanted to be as close as I could to the action and not be able to slough it off on anybody. So I did that. I wanted to have a nice office, because when people come to see you from elsewhere if you are recruiting somebody, they look and see what your facility is like. They do an instant take. A lot of people say that the first five minutes of an interview is the decision maker. The first five minutes.

JL: Yes. I've heard that.

GL: It works both ways. So if I'm recruiting world-class people to come in and they come in and I've got a sloppy, lousy office stuck out somewhere, they're going to think, "Well, who is this guy, anyway? What's he got going? He can't even take care of his own needs, so how's he going to be able to take care of mine?"

JL: Wasn't the building you acquired for pathology previously a prison facility at the old county hospital?

GL: It had been a hospital prison ward. We revamped it. At first, we had the crisis clinic upstairs. People would use the same entrance as we had if they were having an acute psychotic emergency upstairs, until we took over the upstairs too and it was phased in. They moved the crisis center somewhere else. But the building had been a prison ward. It had bars, which meant it was safe.

JL: Yes. You had no security problems.

GL: We basically didn't. We built a very nice office [in the former warden's office], kind of rounded room on the end of the place. It had a private entrance, a private parking place five feet away to start with, drapery, nice wall paneling, a private bathroom. It was perfect. It was very nice, and I'm quite sure that Bob Cardiff is still in that office. In fact, I was there a few months ago.

JL: It is an attractive looking building, at least from the outside.

GL: It was fine, a lifesaver for the department. Without that we couldn't have done it. There wasn't any money to build a new building. It would have taken a long time. This one was there. We moved in, remodeled gradually....

JL: I believe it's slated for eventual demolition for some other building project.

GL: Isn't everything? Nothing is forever. Always is a lie.

JL: There was some interest in doing interdisciplinary research with members of the School of Veterinary Medicine and also with the National Primate Center that was established on the Davis Campus. Was that a program that was still in process when you came?

GL: The Primate Center was still going. We had a little stuff going there. Dr. Stowell was primarily involved in that area, and I did a little with him. I didn't hurt it. I didn't help it. I saw it as a substantial resource that didn't need much of my attention or time. My basic opinion is that if the Davis medical school is ever going to be truly great, and I don't know if it is or not, it isn't now, it hasn't been, it's good, it's not great. But if it's going to be truly great, it's going to be great because of the location of the Davis campus with the agricultural school and the veterinary school. That cluster of people that can be brought together with thinking, research,

collaborative efforts, etc. would be the kind of thing that could make the school unique. It's already unique in that sense, but it could allow development.

JL: That's what attracted Dr. Tupper here in the first place. He saw that possibility of interdisciplinary research prospects, which haven't really materialized to a great extent.

GL: They haven't materialized and I'm not enough on the inside to indicate why. But I know that people build walls. Even if you have two people they'll build a wall between the two of them. It's hard to break down the walls. We established and promoted some good relationships with the veterinary school and the pathology department of the medical school during my time here. A number of our faculty did interact. But the interaction was not profuse. It didn't develop as much as I thought. On the other hand, that's one of the main reasons I went after Murray Gardner, to recruit him to come here, because he had always worked so well with veterinarians in southern California. He'd gotten along perfectly with them.

JL: He'd also been up on the Davis campus doing research with the veterinarians prior to his coming to Davis.

GL: He had all that going. So it made really good sense for him to come, because he had so much to use as background for his work in a collaborative sense across multiple schools. That was my main thinking in going after Murray.

JL: When you first came, wasn't there some heated discussion about the reassignment of some of the laboratory work at the hospital? Had that happened before you came, or was that something that occurred after you got here?

GL: The threat that was ongoing here, and at a lot of other places in the middle '70s and early '80s, was for the entire discipline of clinical pathology to be decimated and all of the activities that involved clinical laboratory testing to be disseminated and distributed out to a host of other kinds of doctors who had highly specialized areas of interest. This was an attractive notion to some because, at that time, it was possible to charge large amounts of money for laboratory tests in those areas and create profit centers because insurance companies would pay large amounts of money.

JL: So it became an economic factor.

GL: It was probably largely an economic factor more that than anything, because the faculty members in those disciplines wanted to have research money, wanted to have space. They figured that if they could get the hospital to give them space, and the hospital to give them technologists to do service work, then the leftover time and space and money could be used for doing research in their field. That's a sound concept for generating research money. It's dishonest because it's ripping off service money used for research, although traditionally that's been called "excess profit for a good cause."

And so there was rampant interest in doing that here at UC Davis and other places. I, as a pathologist, had a territorial turf feeling that that was the wrong thing to do because clinical pathology was mine. I mean, hey, that's my field. And second, I thought it would be administratively unsound and a nightmare for the medical school and the hospital to administer if you had all these little labs scattered all over the place. Who's going to draw the specimens; who's going to guarantee quality; who's going to put the results back on the charts; who's going to do the consultations; how's it going to be entered in a cost-effective way; how can you assure it's cost effective; how are you going to respond to inspections from outside agencies who want to see all the labs; how are you going to assure safety of all the employees; and on and on and on. All the things a clinical pathologist does that all these small, superspecialized content experts knew nothing about. They knew a lot about the content of their little, bitty areas, but they knew nothing about running laboratories.

So it made no sense to me at all that that be allowed, and the administration of the hospital agreed. They didn't like that idea. They agreed with us. So that was, again, one of the main reasons Bob Smith wanted me up here, because he knew that I ran a fairly strong program and I took strong positions on things and usually won. I think he thought that the hospital would be run a lot more efficiently if we could keep the labs together and not have them broken out completely. Now, a few had broken out.

JL: Already, before you came?

GL: Yes, or had been founded without being within the Department of Pathology. There were a few scattered around, so early on I took them on. We closed some of them, we assumed some of them, and we made joint arrangements with others. Also, one or two were left that were politically strong enough that I couldn't do anything about.

JL: Which ones were those?

GL: I'd have to work from memory, and I don't want to do that. I don't think that would be fair. Those are the categories. But we stemmed the tide. We turned it back the other way, and the hospital's interests and the patients' interests were served, in my opinion.

JL: Wasn't there also a problem with accreditation during the time that you first got here? I suppose that has occurred other times.

GL: It was interesting to me that when I was at the L.A. County-USC Medical Center, I was off in Princeton, New Jersey receiving an award at Princeton for the quality of services we were doing in the laboratory at USC. At the same time, the Joint Commission on accreditation was out threatening to close the hospital.

JL: That is interesting.

GL: At the very same time. In Davis, the same kind of stuff was happening. The laboratory was getting kudos and national marks of excellent quality by those organizations that inspect

laboratories, while the medical center was at risk of being closed.

JL: It was actually at risk of being closed?

GL: It was. It was. It wasn't at grave risk, but it was at risk, because there were some very serious discrepancies that were coming to the fore which, had they not been recognized and addressed and some kind of corrective action been set in place, the Joint Commission had the right, even the responsibility, to withdraw its approval. Now, that doesn't close a facility, but it stops federal money from flowing to a facility. You take Medicare and Medicaid money out of an academic medical center and you don't have much left. So it would have, in effect, closed the facility down. The Joint Commission hardly ever closes a facility. What they do is they threaten.

JL: This is the Joint Commission of what?

GL: The Joint Commission of Accreditation for Health Care Organizations. At that time it was called the Joint Commission of Hospitals. It is a national volunteer agency that accredits hospitals and other health care organizations for the last sixty years and functions in lieu of government accreditation in many instances. It has deemed status from the federal government. So it was very important. And hospitals have to be inspected every so often. This particular one during my time here had one of those inspection times with a lot of trouble found. Among the problems, the public media in Sacramento had managed to find access to people who were willing to talk openly to it about the troubles, but off the record. So that the newspaper could have a headline every day about the trouble at UC Medical Center.

JL: Yes, I recall that occurring.

GL: There was one particular woman named Diane Divoky who had moles at the medical center who leaked things to her. So we were front page stuff for quite some time. A lot of that was going on at exactly the same time as the regular visit from the Joint Commission was going to happen, so it was a mess. It was a mess.

JL: Someone wrote a book about those problems.

GL: I've heard about the book. I haven't seen it. It's likely she did, or someone with her. She was right in the middle of it.

JL: I take it that problems were addressed and things got back to normal.

GL: I don't think that's a legitimate assumption. Problems were recognized. Problems were addressed. A long, long effort at solving problems and healing wounds ensued; the scars of which are still evident on many people and on the place. It was a very serious time of trouble and it did not get fixed quickly. It got fixed slowly, after the expenditure of huge amounts of money for lawyers, for paying families of patients who were able to bring suits that, had they gone to trial, could have resulted in huge payments by juries. It is my understanding that they

were settled without trial, but at the cost of many millions of dollars to the institution. Faculty members were suing each other. And the university had to figure out, "Hey, they're all our faculty." The general council's office is supposed to pick which one, you know? Then they have to get their private lawyers to figure out who's who, and it was a big, big mess.

JL: Did that affect your department particularly?

GL: I'm going to say this afternoon that our department could have prevented it from happening if we'd been doing our job right, especially by focusing on the autopsy for quality assurance with feedback and diagnosing.

JL: Well, I'll wait to hear your speech at the Pathology Forum.

GL: Right. It didn't affect it directly, because we were the pathology department, and we were doing service teaching research pathology for everybody. The fact that the surgeons and internists were having this giant fight was disruptive and unpleasant—they were our colleagues and our co-faculty members and we all shared in the grief—but there was no direct affect on us because we were doing our job and our job was going fine. So in the narrow sense, it didn't affect us at all. In the broad sense, we were all part of a larger picture and that picture was the whole medical school and in that sense, it affected us a great deal. By that time, I had been named the chair of the Quality Assurance Committee of the medical center.

JL: Was that an outgrowth of addressing these problems?

GL: That was where they had to be addressed. The Quality Assurance Committee was not an outgrowth, it had existed right along, but this became its problem. I had the joy of chairing the group during much of that time. That was in addition to chairing the committee that changed the curriculum for the whole medical school.

JL: Yes, I understand that you brought up the suggestion that the pathology curriculum be changed. Did you feel there was a real need to address that?

GL: The curriculum had to be changed. It was clear from the first day I was up here in '77 that it was inadequate. It was fine for the way it was to start with, but it hadn't developed right. The original thinking of the first years of medical school was to have integrated teaching of cross organ systems where you didn't have the disciplines responsible for their own areas. In an ideal world, where all colleagues get along, that's wonderful. To a certain extent, it worked pretty well, but there was no course in pathology for the medical student.

JL: Not an organized course per se?

GL: No, it didn't exist. I said, "You have to have a course in pathology." They said, "We can't have it." "Why not?" "Because the faculty has to vote on that. It's a curriculum change." "Oh, well, how do you do that?" "There's a committee that works on curriculum change and they try to get it changed, and then the whole faculty has to vote on it." I said, "Okay." So I got

on that committee. At first we had representatives on that committee and I had them tell me what was going on, and for a year or two nothing was happening. Then I got on the committee and I saw the problems. Everybody on the committee—this was all the departments—they were all unhappy with the curriculum. There wasn't anybody happy with the curriculum.

JL: So you were really suggesting a revision of the curriculum for the entire medical school, not just the pathology department.

GL: Only because there was no other way to do it. I would have been perfectly content with a change in curriculum for the path department, and doing it right, and not messing with anybody else. That would have been fine. But you couldn't do that. It wasn't allowed. It had to be voted on by the entire faculty. The members of the path department had been trying to change the curriculum within the structure for years, and had gotten nowhere, absolutely no where. And it was for a whole lot of reasons. Some were personalities. Some were bureaucratic. Negativism. Some was turf. A bunch of tradeoffs. It was fairly obvious, after being on the actual Committee on Educational Policy, which is why it's called CEP, that the only way to change the path curriculum was to change the whole curriculum for the whole school. So I started trying to do that. And so the faculty organization put me in charge of the Committee on Educational Policy, the whole committee for all of the school. We changed the whole curriculum. We did it in just a few months.

JL: It went that quickly?

GL: From summer to fall. One year.

JL: Didn't you have a weekend workshop. Wasn't it kind of a think tank?

GL: Yes. We had a weekend workshop with the path department. Then we had all kinds of other things. We had every trick of participatory management I could figure out.

The first thing was to get the Committee on Educational Policy restructured so the people on it would be people who had real influence in their departments. You see, in academic administration, some times you assign people to committees to give them something to do so they don't mess up something else, or because its a penalty, or because you don't want to waste time. My attitude is the opposite: you assign the best people you have to committees and make things happen. With the Committee on Educational Policy, I was able to work with the chair of the faculty, the total faculty, or to get the best people, the most respected people from each department assigned to that committee so that what they said might have weight with the rest of their department. Without exception, we got those kind of people, the movers and shakers who were respected and who cared, and who were good. That was the first thing.

The second thing was to develop a questionnaire that asked the faculty what it thought about its existing curriculum. And get the committee to approve that questionnaire. That included pool parties of the committee by my pool in Sacramento in late afternoon, and breakfast meetings at sunrise. Whatever it took to create camaraderie, the feeling of movement, and to

meet tight deadlines. I had a secretary assigned for that purpose; she was very good. So we created the questionnaire. And the questionnaire went out to every faculty member in the Academic Senate, because the power base is the voters in the Academic Senate.

JL: This was the Academic Senate of the entire Davis campus, or just the School of Medicine?

GL: Medical school only, but all the voters. (That includes faculty at Martinez and Sacramento.) There were hundreds of them. I believe it was 380, but several hundred. Of course we had meetings to let them know this was happening. We'd send them a memo saying it was going to happen, and then it happened. We made them vote. We took roll on the votes. We didn't all vote, but we had 70-80% response rate.

JL: That was good.

GL: Very high. So we knew how we did. We tallied the votes. We looked at them, analyzed them, and then we sent out the votes so everybody knew how the votes were. We didn't tell anybody else who voted what way. We told the faculty as a group how the faculty as a group voted.

Then we set about doing what the faculty said to do. And since they had already told us what they didn't like and what they wanted, all we had to do was create the curriculum they wanted. [For that] we used committees and, again, meetings on all three campuses. I personally would go to Martinez, Sacramento or Davis and meet with departments and so forth to get their feelings and to seek their support. And then when we created a curriculum we had to have, we got a vote on that, the actual curriculum. That wasn't binding because it wasn't put in that proper form. Then we had to have a faculty meeting, with a quorum there. That happened. It was voted to send it [the curriculum proposal] to a mail ballot. The mail ballot got 70% approval. That was for the first two years. Then we did the second two years as well.

JL: That was a big job.

GL: It took another two or three months. The third and fourth years were less difficult because they weren't that badly screwed up. The third year was in good shape. The fourth year needed to have some of it brought back from being a fully elective year and make some of it required and start teaching out in ambulatory clinics. That was the big thing at that time. We did that too.

Then we set a mechanism into place, and got the faculty to approve a way to do small changes in the curriculum without having to go through this process. And I hope that has been going on. It should have, because we got the law basically set that you could made small changes without having to do this hullabaloo. That was the whole problem. We couldn't make small changes without the hullabaloo. If we could have done that we could have changed the pathology part without all of this fuss. But there was no mechanism in place to do that. So we had to fix that too. I hope it worked. I don't know.

JL: As a result of these changes, I understand that you were able to develop a first-class course in pathology.

GL: A wonderful, wonderful pathology curriculum. We had general pathology taught the first year. We had systemic pathology we taught [in the summer,] between the first and second year, and we still taught organ system interrelated interdisciplinary teaching the second year as well. We had a little course in lab medicine as well, in addition. We did a little forensic teaching and we did drug abuse and alcohol teaching in the third and forth year. And elective activities in basic sciences for fourth year students. We had what I think was the best curriculum of pathology in the country and it was written up by Bob Cardiff and Joel Lanphear in an article in the *Archives of Pathology* in1987. Faculty developed and exchanged scripts on how to do it.

JL: You mentioned the affiliation with the Veterans Hospital in Martinez. Had that been in place when the medical school was started?

GL: It had been in place for some time. I don't know the answer. John Tupper had seen this as a great resource. The only problem was that it was so far away. I saw it as a resource. They had their own residents. Medical students rotated down there. We had departmental faculty there who were paid mostly by the VA. They did research. They came to meetings. They were loyal. It was a nice small subset of the department. I got Sam French to be the vice chair at Martinez. We had been friends. He had run the labs at Harbor Hospital when I was at L.A.County-USC. He and I knew each other.

JL: It's interesting, all the interconnections at various levels of people's careers who had touched bases at UC San Francisco or at other research institutes.

GL: It came out at the dinner last night.

So Sam and I got on famously and Martinez was a great resource. It, of course, has been closed because of the 1987 Loma Prieta earthquake.

JL: Yes. What about the David Grant Hospital at Travis Air Force Base near Fairfield?

GL: We had nothing to do with it. They had no relationship whatever so far as the path department was concerned.

JL: It seems like you were very much involved in building the pathology department as a service unit at the medical center in Sacramento as well improving the teaching program. From that aspect, it must have been an interesting period of time for you.

In addition, you've done a lot of writing, and that led to a new career when you decided to make a change from being chair of pathology at UC Davis. Had you been involved with editing for the AMA before you took on the new job?

GL: I was asked by Robert Moser, when he became the editor of JAMA—the Journal of the American Medical Association—around 1974, to be a member of his editorial board. That happened because Bob had been an army colonel as an internal medicine expert. I had first met him in San Antonio and later in Honolulu, and he and I served together in William Beaumont Hospital in El Paso. He ran the Department of Medicine and I ran the Department of Pathology between 1964 and 1967. I left the army in '67 and went to USC. Bob went to Walter Reed as the Chief of Medicine for Walter Reed and then he stayed until he retired as a colonel after twenty years of service. After retirement, he got tapped to be the editor of JAMA. I don't really know how that happened. JAMA had never had an editorial board in ninety years, but he thought it should have an editorial board, so he set about to create one. And he asked me to be his pathologist on the editorial board. That was when I was still at SC.

So I was on the editorial board at JAMA from the beginning, and in that role I did reviews, wrote a few editorials and had my own column called "Toward Optimal Laboratory Use," which I set up as part of the original editorial board. The column, "Toward Optimal Laboratory Use," started in 1974 and it is still going twenty-one years later. It runs about every two or three months as a special, all about guiding doctors in how to use the laboratory correctly. I've been back to editorial board meetings annually from 1974 right on. Moser only lasted for a year and a half in his job. He had a confrontation with his boss, threatened to resign and was fired. He went back to Maui to practice internal medicine for about a year, and then he was tapped to become the executive vice president of the American College of Physicians in Philadelphia, a job he took for ten years. He retired again and became the medical director of the NutraSweet Foundation or Company, you know, the little blue artificial sugar packets? He was the guy in charge of that for several years later on. He's now retired in Santa Fe. But he's had a lot of careers as well. So Moser got me in JAMA.

I stayed on the board when he left and continued to work with his successor. When it came time for the successor to retire, under pressure, the administration at AMA asked for the CVs from the editorial board members as part of their national search for the new editor. They liked mine and went about to recruit me to leave UC Davis to take that job. That's how that happened. It was an active recruiting from the AMA. I wasn't looking for a job at all. I was fully consumed with the job at Davis and had no likelihood of leaving unless I'd become a dean someplace. Then maybe I would have been interested. I was fully occupied and doing all kinds of interesting things and having success with a lot of them. I had a beautiful house on Hopkins Road in Sacramento, with a big yard, nice swimming pool, big trees. My kids were pretty much grown.

JL: You eventually had five children.

GL: Five children. Three are biological children; the other two are step-children with my second wife, Patricia Ann Blackridge Lorimer. The are: George D. Lundberg III, Charles W. Lundberg, Carol T. Lundberg, Christopher Lorimer and Melinda Lorimer Roberts.

So I wasn't looking for anything, but they set about trying to recruit me. The AMA's idea was to convince me that if I took that job, worked hard, had decent support and had some luck, that that could become the most important job in medical education in the world. They convinced me of that, mostly because of international opportunities. So I resigned tenure at Davis and took the job in Chicago. I started there January 1, 1982.

JL: Did you leave before a successor was appointed to chair pathology at Davis?

GL: Probably. I think Bob Cardiff was named acting chair.

JL: Yes, he was for six months or so.

GL: He'd been acting chair before, because I had had a sabbatical in Chicago for three months the previous year and he had been acting chair at that time. So he had experience in it, and I believe Hibbard Williams named him as acting chair. Then they had a search and Murray Gardner was made the chair.

So what AMA said could happen has happened. We have forty-nine medical journals worldwide in thirteen languages. We have more readers of JAMA outside of the United States than we have inside the United States. We have our nine specialty journals in multiple languages as well. And so what we decide to publish acts as an education mechanism for more doctors in the world than anything else anybody else does. It really has turned into that.

JL: It has a major impact throughout the world.

GL: It's a huge impact, our operation. When I went there, there was a French JAMA and a Japanese JAMA. We now have eighteen international JAMAs. When I went there, there were nine specialty journals and two international specialty journals. Now there are still nine specialty journals, but there are twenty-seven international specialty journals, or something like that. I lose track of the numbers—they slip and slide a little bit. But we have a huge impact.

We are also central to the whole process of medical editing and how it works. A week from tomorrow I'll be going to Italy to be one of the leaders of a small group funded by the Rockefeller Foundation teaching editors from developing countries how to do medical editing better. We've been able to have a salutary impact on how journals work, the quality of how journals work, and a significant impact on setting the agenda for health care research delivering education for this country and some other countries in the world as well. So it was clearly the right thing to do, although at the time it was a big chance. I have no tenure and they could fire me at any time they want. And they doubtless will. They always do.

JL: You went as editor of the Journal of American Medical Association, but didn't you also become Chief Editor of all their Scientific Publications?

GL: It's a misleading CV. It actually is right the way it is said, but it is misunderstood. I went as Vice President for Scientific Information, which gave me editorial responsibility for all

these journals the day I went, January 1, 1982. That didn't change. The change was the name, the title. They changed it from Vice President for Scientific Information to Editor in Chief, AMA Scientific Publications. Most people interpret it as you do, that that was some kind of a promotion or change. It wasn't at all. It was just a change in the title. But it was a good thing to do because it's a unique job in the world, and it's a unique job in that organization. It's better not to be slotted with people called vice presidents, because every so often there's a reorganization thing and they wonder about all the ranks and they shuffle things around, so having a new title I'm lateral to all that trouble. In that sense, it's very helpful. But what it means is that I have editorial responsibility for all those journals, forty-nine at the moment. I delegate enormously.

JL: I would think you'd have to.

GL: The key to my job now, as the key to the job in Davis was, was to select the best people you can, give them reasonable resources, trust them and come back and see how they're doing once in a while. And if they're not doing well, try to help them do better. And if they're doing great, leave them alone. That's what we do in all the journals as well.

JL: I notice you were involved in a number of television programs.

GL: I've had a lot of television experience in the last three years, which is over at the moment because the network died, as I mentioned last night.

JL: Yes, sorry to hear that.

GL: I started doing a weekly video editorial in May 1991, and every week until this last Christmas I had a video editorial, on the Discovery channel to start with and then on NBC—ninety seconds on anything I wanted to talk about. My CV has a listing of all the topics we dealt with.

JL: There are lots of them.

GL: That grew from just being an editorial to being a co-host of a program called "Medical Rounds." Then I became host of the program when the host resigned and took a full-time TV job in New York. That would have been in like late '93. Then for all '94 I was the host, the on-camera host, the anchor, for a program called "JAMA Medical Rounds," which was a full hour program on what's new in medicine this week, every week. It was great fun. I think it went very well, but the owners of the network kept fighting with each other. They weren't making enough money off of it. We didn't make any money. AMA lost money. The journal didn't lose any money. And then in the summer of '94, the hour program, "JAMA Medical Rounds," truncated to a half-hour program, which I still had total editorial control over, but it had a different anchor, a woman named Sarah Jean Childers. And I still had my video editorial on that half hour. The other half hour was made into a brand new program called "Second Opinions with Dr. George Lundberg." I was the on-camera host. We'd have four guests each week and we'd take on the most controversial topics in medicine that week for half an hour of MacLaughlin Group-type

discussion. We were not Crossfire because we were more professional. But it was a very lively...it's kind of like "This Week with David Brinkley" on ABC TV Sunday morning. A cross between that and the MacLaughlin Group. We dealt with malpractice, gun control, abortion, euthanasia, with practice guidelines, with a host of interesting things, and got very good ratings.

JL: Were you dealing with the proposals for health care reform?

GL: Every one of them. In the journal we've been the showcase for them since 1991, and my talk this afternoon at 4:30, the Highman Lecture, will have a lot to do with that. We've been the principal promoter of health system reforms since May 1991, and among the saddest of the people since nothing happened at a federal level. But we're right in the middle of it. Our TV program was very much in there. We analyzed every plan there was. We rated them. I'd always try to have a fairly far right Republican, a fairly far left Democrat, and then the centrists if it was a political issue. With guns I had the research director of the National Rifle Association. We did tobacco. We did "Should We Legalize Illegal Drugs?"

JL: That's another controversial issue.

GL: We had a thirteen-week run. We were doing great. The network was making money. We had good ratings. It was really a great show. Then it died. The network died. So we have people out there looking now to see if they can put together a business to bring it back up. Probably syndicated. I should know within a month or so whether there is support out there to create enough money to create a business and come back up with either one of those shows. So that was fun. It was difficult because it was yet another skill to learn, although actually I started doing radio when I was a child because my mother used to have a program on radio. See, I started writing because my mother wrote columns for the local newspapers.

JL: So she was the influence in your life?

GL: Always. Very much so. She had a radio program for a little dinky radio station and it was called "The Story Lady." She would go on every Saturday morning and told stories to children. She was good. She was really good. And every so often she would have music as part of the hour and I would sing on the radio, when I was twelve or fourteen or something like that. I did violin and piano when I was a child because my parents were teachers. At one time it was thought that I would be a professional violinist and my teacher in Chicago wanted me to stay there. I was about eight or nine. But my parents said, "No, come back home and be a doctor instead." So I did. I haven't done anything with music since, probably, medical school. Except for singing. I've done some of that.

JL: Is there anything else that you'd like to talk about? I was curious to know...just in reflecting back, if there were things that you wish had happened while you were on the Davis campus as chair of the department?

GL: There are two things, and I have them in my talk. Two things in particular that I felt were missed opportunities. Rather than spend the time on your tape, you can just take it off the talk.

I suppose the biggest thing was we accomplished a great deal. We set into place mechanisms to accomplish more after my leaving. But there are a lot of things we didn't get done that we wanted to do. Bob was kind enough last night at the dinner to indicate that he had found a goals list of mine that had written out 107 goals.

JL: That was impressive. Do you still have that list?

GL: I left it. I left it because it was a Davis issue, not something to take to Chicago. I created a new list in Chicago.

JL: And how many of those 107 goals did you accomplish?

GL: Bob said I accomplished 100 of 107. Now I don't remember the numbers. I have to trust him. He may have been exaggerating; you can ask him directly. He may have the list, I don't know. But we accomplished a lot. A whole lot. And put into place the mechanisms to keep the thing going. The thing about bureaucracy, which most of us work in, is bureaucracy tends to be very resistant to change. And so if you want to change something, it's hard. You have to fight against all this stuff that's in place. That's a weakness. It's a strength, however, once you've created change and put into place things that are good, they also resist change. So that things tend to stay. They resist people mucking them up. So that was always one of my strong feelings about the benefits of bureaucracies. If you could work your way through it, and produce things you think are good, and you have to have objective measures to prove they are good, you then can probably rest assured that a lot of what you started, once it's solid, will stay there no matter what anybody does. Even if they aren't good, they may stay there. That's the bad part.

JL: As we've seen on the political scene, on occasions.

GL: I'm seeing that in the army at William Beaumont Hospital, where I was chief for three years in the '60s. I'll be back there this month for three days as a Visiting Professor in the United States Army, teaching and consulting and stuff. I haven't been back there for about, I suppose, ten years. I went back two or three times after I left as a consultant. And one of the things we did at Beaumont was to design the new hospital and its lab. And then they built that hospital and its lab. They're still in it.

JL: That must be a satisfaction.

GL: A big satisfaction. One of the things we did in my time at USC, in the L.A. County Hospital, a big county building, was to add 50,000 square feet. We developed it, and it's still there, still working. That's a great satisfaction. Here at Davis, much of what we developed is not still there because they got better places. But they were able to leapfrog. That's one of my basic principles. When you can get space, get it. It doesn't make any difference where it is, or if it's any good, you try to get the best you can in the right location, but if you can't, you take whatever you can get. Then you can trade it off. So that was useful, and now they've got that

whole new lab building. Unfortunately, it isn't anywhere near the hospital. You have to use an umbrella to get stuff over there.

JL: Someone said they're wearing out a lot of tennis shoes going back and forth.

GL: That isn't the way I would have done it. Then again, maybe they didn't have a choice. I don't mean to sound critical, because unless you're at a place at a given moment, you really don't have any way to know what was the better way to do something.

JL: That new cancer center has been a big addition to the hospital.

GL: Well, very much so. I'm very pleased with the way this institution has developed. I'm proud to tell people that I was here before I took my current job. I often do. Many people worked hard to make the place quite good. I think it does many things very, very well, and that's a source of substantial pride. And it did come out of its serious troubles. Not unscarred, but it came out of it, and I don't think there are problems at this time, although some of the people are still paying.

The saddest part about being here was the death of one dean abruptly, and the death after that, with a lot of lingering, of another dean. I had four bosses in five years.

JL: When was that?

GL: Well, John Tupper, he retired voluntarily, and then Morton Levitt dropped dead. His successor, Dr. Ernest M. Gold, had a heart attack. Then Hibbard Williams came. I didn't think Hibbard would last a year, but he lasted more than ten. It gets hard to predict.

Exactly how many years was Tupper dean?

JL: He was appointed dean in 1966.

GL: He was dean in 1977, because he recruited me, but he stepped down on graduation day in either 1978 or 1979.

JL: It was in 1979.

GL: So he served twelve years.

JL: According to the records, Morton Levitt served as dean from April 1, 1979 until his sudden death in January 1980. Then Ernest Gold was only dean from January until August of 1980. Hibbard E. Willams became dean in September 1980 and served until September 1992.

Did Dr. Williams come to the Davis campus as dean, or was he already on the faculty?

GL: He was being looked at for the position of professor and chair of internal medicine. He

was at that time at Cornell in New York, having gone there from San Francisco, where he had been for a long time. But when the chancellor, James H. Meyer, was talking to him about being chair of internal medicine, the chancellor got the idea that maybe this guy could be the dean. So he was named dean. So he was dean for twelve years also.

Jerry [Gerald S.] Lazarus has been the dean since when? This is 1995. 1993?

JL: Yes, he succeeded James J. Castles who served as acting dean from September 1992 until February 1993.

One thing I didn't ask you was about your relationship with the chancellor of the Davis campus and the vice president of academic affairs at the time you were so involved in revising the medical school's curriculum. Were there a lot of dissuasions at that level?

GL: Curriculum revision was a function of the academic senate of the faculty of the medical school. There was support and comment by the dean's office, with essentially no interaction with Davis administrators outside the medical school. It was very much internal.

As a matter of fact, I personally had very few interactions with Davis campus faculty or administration except for the Department of Personnel, where we had many personnel actions of various kinds and good support, and except for interactions with one of the vice chancellors who acted as the governing body for the veteran's center at Martinez during some of it's troubling times, a relationship which was fuzzy and not satisfactory. Relations with the chancellor were confined to an occasional workshop or some kind of campus-wide personnel action, or something like that, almost nothing else.

JL: Thank you for sharing these recollections about your career and the time you spent at UC Davis.

Now I will look forward to your talk this afternoon on "The Future of Medicine" at the Pathology Forum. You have been selected to give the Benjamin Highman Lecture, which is quite an honor. This is an honorary lectureship which was established *in memoriam*. Did you know Dr. Highman?

GL: I don't know anything about him. Presumably, Bob is going to honor his memory....

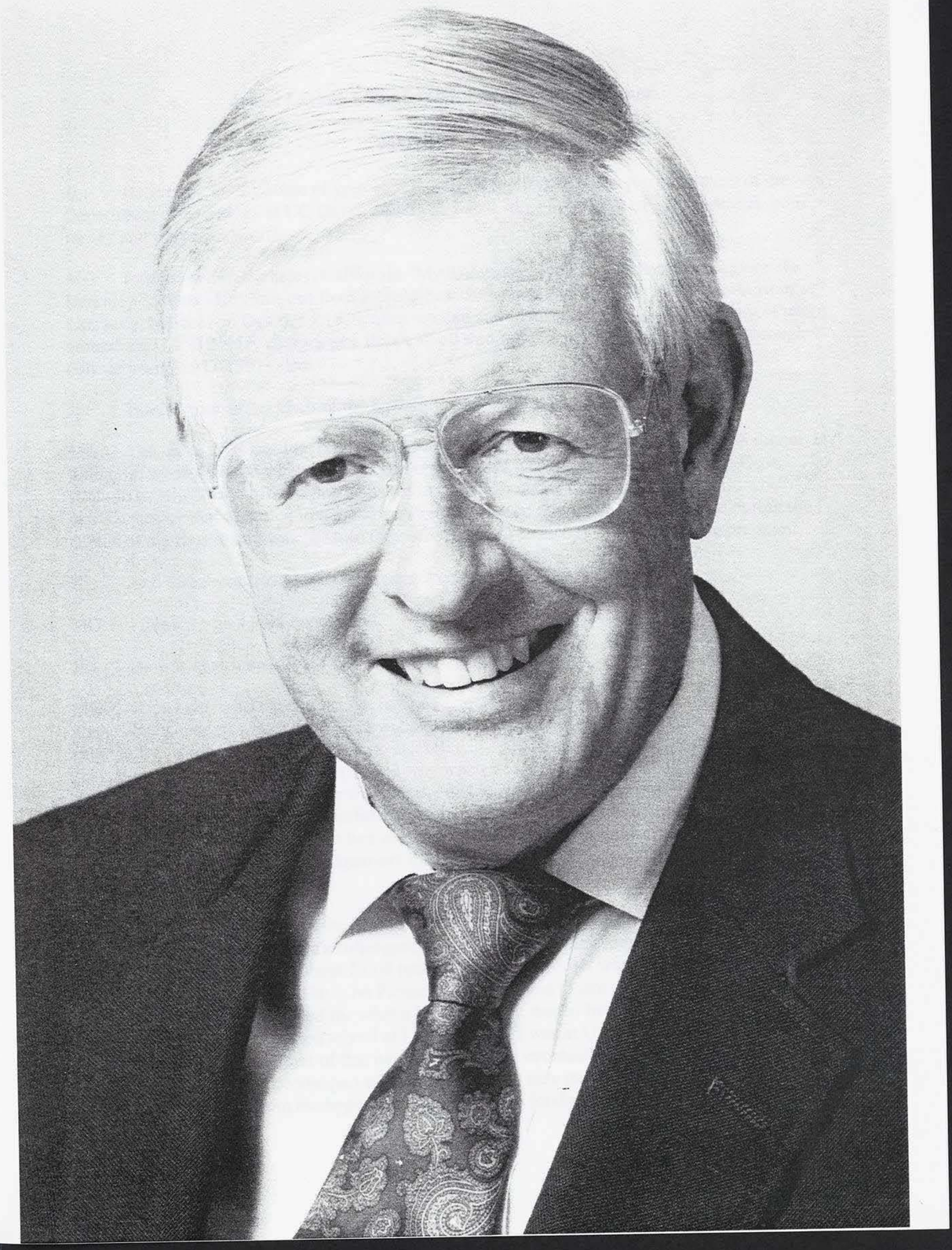
(End of Interview)

MURRAY B. GARDNER, MD

CHAIRMAN OF THE DEPARTMENT OF PATHOLOGY

UNIVERSITY OF CALIFORNIA, DAVIS

JULY 1982-JUNE 1990



Joann Larkey Interview with Dr. Murray Gardner

March 17, 1999

JL: Before discussing some of your experiences of the 1980s, when you were chair of the Department of Pathology at UC Davis, could you recall some highlights of your early life, your family and your education?

MG: I was raised in Berkeley, California. My dad was a professor of plant pathology on the Berkeley campus. He came out from Indiana in 1930 when I was one year old. I was born in Lafayette, Indiana, on October 5,1929, after my dad and mom were at Purdue University. I was around the U C Berkeley campus as a little kid, all through the 30s and 40s, eventually through college years (1947-1951) also.

JL: Wasn't your father chair of the Department of Plant Pathology?

MG: Yes, from 1930 until he retired in 1958. One of the reasons he had come to this school, I found out when I visited where he had done his graduate work at Wisconsin, was that Dean [Claude B.] Hutchison offered him a chance to build a graduate program in plant path, which was something that appealed to my dad very much. One of the real reasons he came during the depths of the Depression was because they offered him a chance to build a graduate program.

JL: And that department was a joint department with the Davis campus?

MG: I think it was a joint department.

JL: He was chairman of both?.

MG: I don't know how it was set up, but I do know that he had a friend at Purdue named [James B.] Kendrick who led the show in plant path at Davis for a number of years. Kendrick came to Davis in 1927. My dad came to Berkeley in 1930. And I think they sort of debated at first about who wanted to go to Davis and who wanted to go to Berkeley. The interesting thing that my mother told me years later was that my dad wanted to make the headquarters [of plant pathology] at Davis, with the beautiful opportunity to work with plants up here. But my mother said it was too hot. She wanted to live in Berkeley for that reason, and he gave in. So they lived there and made Berkeley the headquarters for their operation, while Kendrick lived happily here in Davis for many, many years.

Dr. Kendrick was often the host of Picnic Day activities, which allowed me as a little boy to come to Davis, meet my dad's colleagues here, to play with their kids and go to Picnic Day when it was a small town of about 2,000 people. The town border was Russell Boulevard. It was those fond memories of Davis on Picnic Days in the mid to late thirties and early forties that stuck in my mind and reminded me what a lovely place it was to live. Years later, when they were about ready to start a med school at UC Davis and I was at USC, I kept thinking, "Gosh it would be wonderful to be part of that new school." And eventually, when the time became right (in 1981), I was able to become part of the still relatively new medical school at Davis. It was as if it had been scripted from the beginning, because of my fondness for that part of the world.

JL: When did you graduate from Berkeley High?

MG: I graduated from Berkeley High in 1947. I had gone to Willard Jr. High, Berkeley High, as a pre-college student. I entered into UC undergraduate school in 1947. I didn't know what I wanted to do. I was studying and planning to be scholarly, to keep peace in the family. I was really interested in sports, and became interested in truck driving shortly after that.

My dad had introduced me to a fellow named Ray Wood, a great guy. He had developed the university's fleet of cars that still is used by faculty for field trips, etc. He was an entrepreneur, an Oklahoma cowboy who came to Berkeley in the early 30's . The university saw a need for professors at UC Berkeley to have cars to go on field trips, so Ray Wood provided those cars. Then University President Robert Gordon Sproul said, "Hey, let's make this part of the [statewide] system." And so Ray Wood developed a central fleet of cars and furnished all the university buses. I had a job working for him, driving trucks and buses for university business, and going to Detroit or Pontiac, Michigan, with busloads of kids to bring out the cars for the university fleet. In those days, which would have been the early 50s, there were no assembly lines on the West Coast. All the cars came from the factories in Michigan. So there was a need to get them out to California, and we drove them out. The same went for the buses.

JL: Did you organize these excursions?

MG: I organized them. I took the long trips. And the deal was that we would let student drivers go in the bus I drove anywhere they wanted on the way East, to have sort of a tour. We went to Carlsbad Caverns, New Orleans, fun places. And then in return, they would drive the new cars out West. We would give them a credit card to pay for their gas expenses. So the university saved thousands of dollars doing this. I drove the buses full of these kids. I had a lot of nervous energy in those days, and I didn't want to stop. I didn't want to stop! So we would go for hours and hours and hours. Eventually, they would pee in cans. I just drove non-stop because I was having fun. We had no place to stay anyway, except on the bus. Eventually, when exhaustion overcame us I would park. We got to New Orleans and parked on a rainy street at 3:00 A.M. and slept two hours. I took them to West Lafayette [Indiana] to try to find the street where I was born. I'd never been there, but I thought they ought to see where I was born. We wandered around West Lafayette for the morning, and couldn't find it. We just had a big adventure. And it was exciting.

But that was a "sign of the times." That was a young kid driving a bus with no chauffeur's license. Nobody worried about lawyers. We just did the job that had to be done. Ray Wood said get it done. It was a university you could relate to, because it was small enough and we felt a sense of family. It's gotten harder to do that as the university has been growing to nine branches and 89 chancellors and 16 presidents, and all this. It's very hard now to know where you fit in such a huge bureaucracy. But in those days, one had a sense of loyalty. And the great thing about working for Ray Wood was that it made me aware of a whole new walk of life and gave me confidence. I could do a man's job, and do things my dad never taught me or encouraged me to do. I could be my own person.

All that time I kept studying, keeping my nose in a book. I didn't drop out of school for two reasons. One, to keep peace at home, and the other reason was, had I not kept going to

school I would have been drafted and sent to war, because that was the height of the Korean War. And there was a mandatory draft, which eventually did take me when I completed my M.D. At least I was deferred to go to medical school, which I preferred to do rather than become a foot soldier.

JL: At what point did you decide to become a doctor?

MG: My sister, who was a good, solid student, preceded me. She had gone through Cal and had been the number one scholar in her 1944 class. I remember how hard she would study, partly because my family had their thumb on her. She got into Cal med school six years ahead of me. And in 1947 she told me that it was a great education, a great experience, and I'd be wise to do it, sort of as a way to do something worthwhile. I said I'm much more interested in driving trucks and doing other things, but that I'd go to the trouble of applying to Cal. I didn't apply to more than that one school, because if I didn't get in there I'd do something else.

JL: You'd be driving a truck.

MG: Driving a truck and exploring the world. Go to Alaska and all the other things. After three years of pre-med, they took me, and I don't know whether that was my dad's influence, and I don't like to think it was, but it probably didn't hurt, although he wasn't really known to the medical school. Anyway, I don't think it was my sister's influence, because we were so different. At any rate, my first year of med school was combined with my senior year of college.

JL: Did you remain on the Berkeley campus during that time?

MG: Yes. The first year of medical school was on the Berkeley campus in the Life Science Building. Anatomy, physiology, biochemistry, the basic science courses were then taught in the Life Science Building. That was near the university garage, so it allowed me to keep my truck driving job. During the first year of med school, I drove trucks and buses, and just went to medical school during the day. I took my early 6:30 A.M. run driving people to the Radiation Lab. The university provided transportation for people who worked at the Cyclotron. Then in the afternoon I went and picked them up after work. And then I worked at the Radiation Lab at night on-call for scientists who needed rides up and down the hill. So I got paid to study, which I thought was great, and managed to combine medical school with my first love, which was doing these other things. I was rebelling in that way, in that I didn't want to end up like my dad and just be another professor. I wanted to prove I could be independent, my own person.

JL: As I recall, you also were rebelling a bit with your classical music training by pounding out Dixieland jazz on your mother's grand piano.

MG: My mom wanted me to play classical, and indeed I did and I did enjoy it. My teacher thought that popular music was bad, and I should only play classical. But in many ways my heart lay with jazz, so I would play boogie woogie and Dixieland and other things I had picked up on my own. I would annoy my piano teacher when she came to give me my lesson. I'd see her park her car, and as she'd come up the stairs I'd pound out some boogie and she'd come through the door with, "No, no, no. Don't do that." So it was always sort of a fun thing to do. As long as my mother was alive, until several years ago, I always felt when she was at our home I should pay Beethoven, Bach, etc. Now, at this other end of the life's spectrum, I'm taking jazz

lessons. But I found out, unfortunately, that my roots are in my way. It isn't as easy to play jazz as I thought it would be. All the technique doesn't do it for you. You've got to have some innate ability and background that I didn't have from my classical training. I missed a golden era when I should have been out doing jazz, listening to jazz, taking part in it. And now to try to catch hold, I see I suffered from that. But at any rate, that was part of my rebellion, as was the truck driving and some other things. I didn't want to conform.

I joined a fraternity because it was the thing to do, but I didn't want to be like everyone else, wear a white shirt because everybody else wore a white shirt. I didn't want to take part in some of the sorority exchanges because everybody did that. I wanted to do my own thing. On the other hand, I sort of kept my toe in the water, but I never really joined in. I had a chance to drive trucks and buses, and make money. Because I had no time to spend it, I just saved it up, bought a convertible. But I was pretty intent on not being a clone of my dad. Not that he was bad; it was just that I wanted to be my own person.

So that sequence of rebellion influenced my training. As I went to medical school, I went through the paces and I didn't mind. But all those years my heart still lay in doing other things—truck and bus driving, and playing basketball and tennis. And then in 1955, after my internship, I got drafted as part of the Barry Plan.

I had interned at UCSF Moffit Hospital in medicine from 1954 to 1955. I should have taken a rotating internship, but they talked me into staying there. But afterwards I never really felt that I was a complete doctor, because I had so little practical experience.

When I went into the military, I joined the navy. They assigned me to the marines, sent me to El Toro and soon dispatched me to Japan. I ended up at a naval base called Iwakuni, which had a support group for the marines over across the sea in Korea. I was lucky to get stationed in Japan rather than being sent to Korea, so I spent over a year of duty there.

I loved every bit of it, because, first of all, I was getting away from home, being independent, and I didn't have to study all the time and pass exams like I had been doing all my life. And I became interested in the Japanese people and their culture. I saw there was a need to do a lot of things that weren't getting done to help the Japanese people. Marines are not allowed to take their families overseas because they have to be ready to fight on a minute's notice. But needless to say, they were having families with the Japanese girls. But there was no place for the women to have their babies because they weren't officially part of the base. But the American servicemen and their girlfriends wanted to have their babies delivered at the base hospital, which was not legal. I volunteered to do this job, which was beyond the call of duty. I formed a clinic, on my own, to take care of the Japanese wives and girlfriends of the American marines. And I did that for a year. I followed these women pre-natally, right up to the moment of delivery. I came in on my own time and delivered their babies in the base hospital, sort of incognito, and provided them the best American medicine that I could give, rather than have them have to go to a local clinic. I built up a tremendous clientele of patients and their children, several of whom named their kids' middle names after me because they were so grateful that I had delivered them.

I became close friends with a number of the Japanese people in the community and was "adopted" by a family in Hiroshima, a doctor's family who were instrumental in forming the

Atomic Bomb Commission; they had lost many of their relatives during the bomb. I lived in their home on weekends, they dressed me in their clothes, with fans and things, and took pictures. I was sort of like a toy, a funny thing in their life. But they treated me like one of them, not like an imperial conquering soldier. And I learned to speak Japanese, which helped, because during all that time that I was caring for the pregnant women, I spoke their language. I practiced Japanese while palpating their swollen abdomens and trying to figure out where the baby was and when it was going to get born. And I learned to ask them if they had pain, little things. And eventually they would talk to me and I would learn to speak more Japanese.

I had a very good friend during those years named Tommy. That's a whole other story, but to make a long story short, when I came back from the marines, Tommy, who had been born at Tule Lake, in this country, wanted to come back. So I brought her home to Berkeley and helped her start back in life in this country, and introduced her to her future husband in a Japanese church in Berkeley. They ended up living in Rocklin. Her husband died and now she lives in Sacramento. We have reunions with our respective families once every year or so.

JL: Did the military approve of this ad hoc clinic you conducted?

MG: The military just looked the other way. It was not legal, but the military should not have been living with the local girls. I mean, some of these men had wives at home, yet they were over there a year or more and they ended up living with Japanese girls. So their consciences hurt, particularly when they got VD or fathered a baby. I learned to really enjoy family practice this way. I said, "Hey, this is what medicine is all about. This is what I would like to do when I go back to the States, perhaps have a family practice, because this is a good feeling you have, really caring for people, being appreciated." And that was a good experience.

I also took care of the military. I took the sick call for marines and treated their athlete's foot, non-specific urethritis and like problems. But the most fun was doing something totally outside the realm of what I was supposed to do, and that was building this family practice of Japanese wives and girlfriends and their children. And this allowed me, then, to get to know the whole community much better through their eyes. I identified with the Japanese.

I was so rebellious of the military system that I would not go to the Officer's Club. Never did go. I wouldn't eat with the officers in the mess hall, or at the hospital cafeteria. I would eat with the Japanese cooks in the kitchen because I enjoyed their company more and could talk Japanese with them. I just ate what was left over and wouldn't be part of the establishment. Eventually, the base captain called me in and said, "You know, if you weren't a doctor, we'd be getting you out of here. You're just not behaving like an officer." I said, "Well kick me out then. I'm only here because I have to be. That's why I'm doing what I'm doing." But he couldn't make me conform. So I was still sort of rebelling against the establishment. But on the other hand, I was doing some good things, so they let me stay. It was a great experience, and I came back home after the my tour of duty ended in 1957.

JL: So you were in the military for two years?

MG: Two years. It was mandatory. I was discharged honorably and decided I wanted to finish my family practice training, general practice, it was called then. Again, I was interested in not

specializing. I thought that all this new trend that was then going on was a mistake because you lost track of the whole patient. I said, "Nobody's going to force me to specialize in one part of the body or another. I want to be able to take care of the whole thing."

There were very few training programs in family practice in 1957. There were only a handful, but one of them was in Santa Rosa, at Sonoma County Hospital. The residents ran the hospital, and they were coached by the local physicians who came in when asked or when they would cover clinics. But we really did all the work. It had one senior resident, affiliated with Stanford Medical School, which was at that time located in Pacific Medical Center in San Francisco. They'd sent us a hotshot surgeon, a fourth-year resident who would cut everything in sight, and we'd work with him. Then we would do a lot ourselves. It was really "See One, Do One, Teach One." For example, I was shown how to sew up a perforated ulcer. The next perforated ulcer, I did alone with a nurse assisting. I was shown how to nail a hip, and believe it or not, I went off and nailed hips. I even did brain surgery. We did everything, learning on the job. Which was wonderful for the feeling of now being a real doctor. And you thought, this is what surgery is really like. I never saw that as a med student, because they never let me get that close to the patient. Now I was doing it all.

I was in the emergency room every three nights and often up all night. I was so anxious and nervous that I could never sleep. I'd listen for the ambulance coming up the road, and I would wonder what it was this time, and what to do, and should I call for help or not. I was just constantly sort of tormented by feelings of insecurity and afraid I wouldn't be able to meet these crises. You do this for a couple of years and you do learn, often from your mistakes.

It became clear to me after this time that I couldn't go out in the community and do the same things I was doing in the county hospital, because young specialists were flooding the place who could all do those things better than me. The minute I made a mistake, I would have been in trouble and my conscience wouldn't let me do it. Also, I didn't want to charge patients. I didn't like the business end of medicine. I sort of said to myself after this, "Well, I don't think I want to go out and be a GP in a small town because I think what I'll end up doing is triaging an awful lot and sending it off to the specialists. I won't be able to do all the things I have been doing. I don't like the business part of it anyway."

So it was a big changing point in my life, because now I'm twenty-nine years old, and I'm still not quite sure where I'm going. I'd met my wife, Alice, who was a student nurse at Sonoma County Hospital. She'd come from a small town, Lindsay, California, and liked the idea of living in a small town and practicing in a rural setting, and that was the way we sort of staged our life plan. Then when I didn't do that, it was sort of like a dirty trick.

I ended up going back to UC San Francisco to take pathology. Now, why did I do that, to take specialty training in pathology? Well, first of all, I'd remembered as a med student that my pathology chair, Henry Moon, had said to me that if a time ever came when I got interested in pathology, to let him know. He'd try to help. That always stuck in my head. I never thought at that time that I'd do it, but now I realized, after tasting the full brunt of that GP residency, that I really didn't want to do surgery. It was boring, my nose itched and I couldn't scratch it when I was gowned up. And I didn't like anesthesia because I was bored. After you do these things, you realize that they're not all that much fun or glamorous or exciting. What I became interested

in is why things happen. I kept reading the journals. Why, and where did disease come from? And how do you explain it? And I thought that, well, pathology might open some of the doors into the science of medicine that I haven't yet experienced. And I suspect some of this was probably an influence from my dad, who was interested in pathogenesis of diseases in plants. Though I don't think I would have admitted that to myself before then, it was an influence. I went back, seeking more scientific underpinning to what I'd been doing, in a pathology residency in San Francisco. That was a four-year program, from 1959 to 1963. Two years of anatomic, two years of clinical path. Then I became a card-carrying pathologist.

JL: You took the boards?

MG: I took and passed the Anatomic and Clinical Pathology Boards in 1963. I found that going back to pathology after family practice training was a piece of cake. I could grind out those autopsies, six a day, with one hand tied behind me. I could read the charts and immediately appreciate what the clinical problem was and dive right in and make sense of it. It was easy for me to look in a microscope and see things and talk about them, because I'd had all this background of clinical relevance. I sort of sailed through the residency, again still driving trucks some, not full-time. I played a lot of tennis in Golden Gate Park while I took call, because I could run up the hill in my shorts and do a frozen section fast enough that they wouldn't catch me, but it was sort of stretching the limits of propriety. I mean, I always sort of thumbed my nose at the system. I guess to this day, I still don't want to conform.

JL: Is it true you had your wife on the tennis courts the day she delivered her first child?

MG: Yeah, well, I guess there's some truth in that. Well, there's even worse stories than that. I wasn't very kind. When she was having her first kid, I didn't want to wake my friend up who was delivering the kid, one of my basketball buddies. Alice went into labor at midnight. I kept doing rectals and pushing the baby back.

JL: And waited until morning?

MG: Waited until morning. So when it got light, we got in the convertible, with the top down, because I never put the top up, and it was sort of misty. I drove through Golden Gate Park. There we were. Every time she'd get a contraction, now coming good and fast, she'd fold up in the seat and kind of disappear out of sight. And I got her up to Moffitt Hospital. I put her on the elevator, and she went upstairs. When the nurses saw her, the baby, Suzzana, was being born. They took Alice, without even prepping her, right into the delivery room and delivered her on the spot. And I hadn't even parked the car. By the time I got there, Susie was there. I always knew Alice was healthy and strong. I guess that's male chauvinism.

One time we took a pack trip in the Sierras, in northern Yosemite. This was when Martin was being born. It was at the end of the eighth month. It was summer, and it was our only chance to get this backpack trip in. We both thought she might deliver in the back country, so each of us, unbeknownst to the other, took a sterile set of obstetrical equipment. And Marty was born about two weeks later, after we got home.

JL: You eventually had four children?

MG: We had four kids, and they're all big, strong, husky and healthy. Danny is our second son and Andy was the last. They're all around home and we see them often. They're all managing to make their way through life in reasonable shape and form. I see in my kids some of the same rebellion that I felt. It's not always helped them, but I understand it. It's interesting that Marty, the older boy, who's now in vet school, rebelled, in a sense, by identifying with cowboys and learning how to pack mules. To this day he's a mule packer and cowboy. Susie had her way of rebelling, too. That's a whole other story.

At any rate, to go back to pathology, I really didn't know if that was what I wanted either, because there really wasn't that much science to pathology in the late 50s. This was still before the molecular biology revolution. We still didn't have a handle on DNA, RNA and it was mostly looking at slides. And for clinical path, it was watching a few machines. It really wasn't very exciting. And so again, I didn't see where I was going to practice. I really did not know yet what I wanted to do. I was still looking for something more than I had gotten. And I didn't know where to go. My residency was finished. Henry Moon said they'd offer me a post-doc fellowship at UCSF. I'm thirty-four. They were going to pay me $8,000 a year and I could work with a researcher on iron metabolism at the county hospital. That wasn't very exciting to me. And just by chance, one of my friends, Mary Jane Aguillar, with whom I'd played tennis and whose husband was a surgeon down south, said, "They have some positions at USC and the best thing about working down here is that you can play tennis. The weather's good."

And who in their right mind would leave San Francisco and move to Los Angeles you might ask? Well, I thought it was a great opportunity. I flew down one night. Dean Clayton Loosli said, "Well, we need you badly. We'll take a chance on you if you'll take a chance on us. We'll give you an assistant professorship, $15,000 a year." Twice what they were offering me at UCSF. So I said [to my wife], "Let's do it. Let's get out of here." And it was the best move a guy could have made. I was quite inbred to northern Cal, but to get a chance to go off and do my own thing in a big frontier like LA was challenging.

We packed everything we owned in a U-Haul, pulled my car behind, and put Susie in a box down there on the floor. We drove off one night under a full moon to LA. I didn't know where the county hospital was. I didn't know where we were going to live. We drove around looking for rental property in the wrong places. Eventually, we ended up renting and settling in Alhambra, then Altadena. It was a new adventure, a new world. I didn't worry about the future. I didn't know what I was going to do, exactly, only that I'd be taking on a project to study the lungs of mice that were exposed to smog living near the freeways in little shelters.

JL: Was that your first research project down there?

MG: Yeah, really the first research project. I did that for five or six years. The idea was to see whether smog was causing lung tumors in mice. It was never proved, but it was a chance to become part of a scientific effort. And I read up on it. About that time an electron microscope became available at nearby White Memorial Hospital. I used it to look at the lung tumors in mice. And that was fascinating because now you could look inside the cell. And somehow biochemistry began to come alive, and it gave me a chance to do some academic things and write some papers. I still wasn't sure where I was going, but through those few papers I had written on

the effects of air pollution on mouse lung tumors, I was able to meet Robert Huebner who was such an instrumental figure in my career.

Bob Huebner had just come into a position of great influence at the National Cancer Institute, where he was sort of leading the national effort called the War on Cancer, to find out whether RNA tumor viruses (now called retroviruses) were important in causing cancer. He put me to work on that project, and that starts a whole new era. But again, it was just chance that I would meet this key figure. How did he find me? Well, as I understand it, he went to LA because he knew that was a very good environment to do research (a lot of different ethnic races, smog and environmental pollutants, chemicals) to see whether or not environmental carcinogens activate retroviruses to cause cancer in man and animal. He was hoping to some day have a vaccine for RNA tumor viruses, to prevent human cancer. That was the long-range plan. He needed a "pistol," or a lieutenant out there in LA. And I think he probably was anxious to get some person who had a reputation and credibility in the field, such as Robert McAllister, who already had his program at Children's Hospital in LA. He didn't want to take part in this thing, but suggested that I might be the kind of person that Huebner wanted. Hubner eventually sought me out at USC and presented this big picture to me. I said, "I don't know what a retrovirus is. But nevertheless it's exciting, and it looks like a great opportunity, and I'll give it my full shot." So this, then, was a chance to sort of leave the conventional course that I was on, and go on to a research enterprise and learn a whole new field of science, to affiliate with scientists, learn from them, and take part in a massive program which had national urgency.

JL: Wasn't this about the time that President Nixon had declared war on cancer?

MG: The War on Cancer, yeah. This is actually a little bit before. This was 1969, but it was the same issues that provided the basis of Nixon's War on Cancer, which was an effort to get at the etiology of cancer, primarily by looking at the role of viruses. And RNA tumor viruses were the main target. Now they're called retroviruses.

Huebner asked me to see if retroviruses existed, not just in humans, but in wild mice. Laboratory mice were being modeled, but no one had looked at wild mice. So my job was to establish a retrovirus research program to look at the natural history of these viruses in man and pet animals in Los Angeles, in animals like wild mice, domestic cats, dogs, parakeets, and zoo animals also, whether or not these viruses cause cancer in their natural hosts, and under what conditions, and whether or not such viruses might spread between animals and man, like in household parakeets to humans. I was also to share these raw materials with other researchers in the Virus Cancer Program.

So this was a large mission. To do this he wanted me to have an interdisciplinary team. You need a team of people together for a problem of this nature. He was able to funnel me support, like $600,000, as I remember it, the first year.

JL: Were these federal funds?

MG: Yeah. Through your tax dollars for the National Cancer Institute (NIH) to carry out this War on Cancer. I had a little piece of that. I became an instant general who could pull together an army of investigators. I liked to think of it sort of like a GP who had a clinic of specialists. I

was the overall guy that organized and synthesized the total information and made it go. I recruited a virologist, an immunologist, etc. I brought these people together to work. This was wonderful. And of course it led to a lot of problems for me, because I was a threat to my colleagues in the university who suddenly saw me with all this money as a threat to taking over their space.

JL: The old turf problem.

MG: The big turf problem. I had a lot of friends there, and a lot of people to help me. On the other hand, I was perceived as a threat and some were jealous, you know.

JL: Were you bringing people in from outside the university?

MG: Both from within and from without.

JL: Some of them were already on the faculty?

MG: I made jobs for people that were already there. And others were brand new. It was a threat, so the university had a meeting of the medical school faculty. Huebner came out from Washington D.C., and I stood up in front of an auditorium full of colleagues and talked about what we were doing.

Eventually, the problem was solved because Hugh Edmondson, chair of pathology there, a grand man who had been very kind to me and nourished my career like a father figure, along with his wife, Dorothy, came up with independent support money, which they gave to the school to buy a building. That building was located three or four blocks away from the medical school on Soto Street, and they used it for my program. It was a big warehouse building that had previously been used by Cal Biochem, which went to La Jolla, leaving this building on Soto Street empty, so he bought it for me and my group. And that became the Edmondson Building. That's where my research group was located for the twelve years I was there. It still is an existing building, part of the university retrovirus research is still done there, and one of my colleagues, Shuraiya Rasheed whom I recruited, still works there. The NCI gave the money, about a half a million dollars to renovate the building. So we built a beautiful laboratory building for my team. We supported this off of government contracts and grants. I had an administrative assistant there, Margo McDonald, who had gone to school in Berkeley where I had known her as a schoolgirl. She's still in LA, but now retired.

But anyway, this was the way I got into research. I had all this energy and enthusiasm for something I hadn't yet exactly found, and when this chance came along, I grabbed hold of it. It soon became "a tiger by the tail." And for the subsequent twenty-five years, I was a retrovirus researcher. Now I can look back and say that I am one of the leaders in this field of using animal models for human disease based on retrovirus infection. Now I write mostly key review chapters. I've just been made a Fellow of the AAAS [American Association for the Advancement of Science], based on my work in the retrovirus field. I feel that I accomplished the mission that Huebner gave me twenty-five years ago.

This opportunity had opened up for me when I was about thirty-nine. I enjoyed the science, and it made my academic career.

JL: Now you have a total of 322 published papers.

MG: Well, yeah. And it's not over yet. In the beginning, I didn't know how to do it. I remember George Lundberg who was at USC with me saying, "Well, if you're going to do this research thing, then your job is to write papers. You'd better plan on writing about twelve a year." At any rate, twenty-five years later, I can say that I did that. I did that and then some. I found out that one of my strengths is that I can write. And I do a lot of editing for my colleagues, because I have a feel for it.

JL: You seem to have a sense of history, too.

MG: Yeah. I think history is a wonderful thing. I've always been interested. I'm not interested in ancient Greek history or European history. I do like the history of places I can relate to. And I was reviewing here this morning a bit of the history of retroviruses, which I took part in. I climbed on the shoulders of the people ahead of me, and am now helping people climb on my shoulders. The point is, research is a continuum, as we all know. We make a little contribution, and the world goes on. I like to play historian. I've written the history of the Virus Cancer Program and of the discovery of Simian AIDS.

My training in the animal retrovirus work made it ideal for me, who had just come to Davis in 1981, to help discover Simian AIDS. In the 1970s, I was the new kid on the block. Now, twenty-five years later, I am the old man. There's a bunch of new kids coming along. I do have a sense of history in the work I do. It's hard not to.

I'm interested in the history of the areas where I've lived, as are you, especially histories on California and the western United States. Now I'm interested in Yolo and Solano counties. All these little things that surround you in everyday life. It adds a lot more meaning if you can know the history. I've had several chances to talk to my next door neighbor, Mrs. Myrle Maxwell, whose husband's relatives came to California in 1850. I've always thought history was very meaningful. We tend to overlook the past, we're so involved with the day to day things, so impatient that we hardly ever bother to look back.

In the sciences, we're so overwhelmed with information that we don't read anything that happened before the last three or four years. We don't really appreciate and know our roots that well. So I like to play the "camp historian," and give the big picture. I'm going down to talk to the UC Task Force [on HIV] this week, about what we have learned from animal lentiviruses. What was said about the equine (horse), goat and sheep lentivirus diseases back years ago are perfectly true for HIV today. If you understood the natural history of the animal lentivirus counterparts, you'd have no surprises by the way HIV is behaving in humans.

JL: Didn't you start working with some of the veterinarians on the Davis campus while you were still down in southern California?

MG: Yes, and I worked with veterinarians down there, too. One of the things that got me "off the ground" in LA, even before Huebner came along and offered me this chance to do tumor virus work, is that Dr. Edmondson had asked me if I would like to work with some local veterinarians who did the pathology for LA county. In those days, there were no veterinary pathologists as a specialty. They saw pathology in animals and I saw pathology in humans, and

we compared them. We got together every week and looked at each other's slides. We became friends. They took me out on field trips to see buffaloes, whales and all sorts of wonderful things. I contributed to a book on all the histology of diving mammals in collaboration with a veterinarian friend, John Simpson, a graduate of UC Davis. This friendship with veterinarians made it possible, when I was asked to build this tumor virus program, to get a hold of animals. My veterinarian colleagues opened the doors.

JL: Access to animals was critical to the research you were doing, I imagine.

MG: Yeah, access to the whole veterinary community.

JL: I understand that you had a tremendous mouse collection, too.

MG: Oh, I had wild mice communities from many different trapping areas. It was a lot of fun. We formed the Laguna Niguel Community for Aging Rodents. We learned a lot from them, and published extensively on their retroviruses and diseases. We found a whole new biology of retroviruses in those animals. What we learned, in many ways, really pointed the way toward what was found to be true of HTLV in humans, the first human retrovirus that was discovered ten years later.

The similarities were remarkable. Wild mice from one trapping area—a squab farm near Lake Casitas in Ventura County—had leukemia retroviruses and could transmit them through mother's milk, and to a lesser extent, sexually. It was just the same way that humans transmitted HTLV. We were able to show that the disease causing retroviruses in wild mice were not inherited, like the cancer program thought they might be. These viruses were acquired infectiously, just like other viruses. But it did show that retroviruses can cause cancer in feral or wild mice. We also showed that retroviruses could also cause neurologic disease and immunologic suppression in wild mice. So it's no big surprise to see retroviruses cause diseases other than cancer. On the other had, I must say that never in the 70s did we ever anticipate that we would have a sexually transmitted retrovirus like HIV causing an epidemic worldwide. That just shows you the surprises that research holds in store for us.

JL: Do you find anything in the literature now that indicates that AIDS might have been happening at an earlier time?

MG: No, no. There just wasn't anything like this before. This AIDS epidemic is a unique combination of a new virus in humans and social changes and it's a whole new biology that is occurring with AIDS that we had not anticipated, based on our animal models. Should we have been doing more to the blood supply in the 70s to kill putative viruses that hadn't yet be found in humans on the supposition that they might yet be found in humans? That's a judgement call, probably so. You surely can transmit these retroviruses in blood. The equine lentivirus disease, called infectious anemia, was, by the way, the first animal disease that was proven to have a virus etiology, in 1904.

JL: That's quite early.

MG: Yeah, equine infectious anemia, was the first animal disease to be assigned a viral etiology.

Now, when HIV was first discovered in humans by the French people—Montagnier, Cherman, and Barré-Sinoussi—they found a serologic cross-reaction within this virus, EIAV. They had a human lentivirus. In the War on Cancer, we were looking for a different kind of retrovirus. We weren't looking for lentiviruses, because they didn't make cancer. Way back in the 1970s, scientists were able to transmit infectious EIAV with as little as 1 ml of blood from the donor. They could transmit it to horses every time. Now if we, in retrospect, knew HIV was in humans and HIV was related to EIAV, it would have been clear from this information that blood was a ready source of infectious virus. Should companies in the 1970s and1980s have done more to block, to inactivate, viruses of this kind when they didn't know they were there, based on the fact that they might have been there? I'm the historian. But that's fun, isn't it?

Well, that program at USC put me on the map and I built a very fine group and we did good science together. We hung in there together for about twelve years. Then the War on Cancer Program came to an end. It became too expensive. And by then, with multi-millions of dollars spent, a retrovirus cause of cancer had not been found. Huebner developed Alzheimer's disease and had to fade away.

I could have stayed at USC. But I knew that this was a good time to move on, because now, in 1981, I'm 52 and it's hard to get recruited if you get too old. I knew, of course, that Davis had a medical school. Furthermore, I knew by then that George Lundberg, my colleague at USC, had preceded me at Davis. And I thought to myself, "Well, Davis, where else in California can you have a farm and live the kind of life my family wants, and still be near a med center so that I can do my thing?" It just seemed so logical. UC Davis Med Center needed someone to run anatomic pathology at the hospital.

JL: Were you aware of problems there before you came to join the UCD faculty?

MG: Yes, but I would have come even if they had wanted me to be a home economist. I just wanted the lifestyle. And I had some friends on the Davis campus—Gordon Theilan, Tom Kamakawi, Niels Pederson and Bob Cardiff. So I knew I had my own friends at Davis. Cardiff said he would share his lab with me. I said, "Hey, I don't have to have a lab. I'll just come up there and do whatever you want." I didn't negotiate for a lab. I just said, "I'll fill that bill in anatomic pathology, while I really look around for something to happen in the research world. I'll do my own thing."

JL: You just wanted a farm. Hadn't you promised your wife one when you first married?

MG: Yes, I just wanted a farm. When they asked what I wanted, I said, "I want a barn and a pond with ducks and no fancy house. No rugs on the floor. No chandeliers. I just want to live on a farm and have space and animals." And that they found for us.

When we first came up to Davis to look around, we wanted out of the city so bad that we almost would have taken anything with some space around it. I went with realtor Lois Faulken to look at a place out on Russell Boulevard, just past where there's a Y.

JL: Near Stevensons Bridge?

MG: Yeah. There was a place for sale, with an almond orchard on it, a rambling house and

swimming pool. It had space around it. And I thought, "Great, we'll take it." I mean, I didn't want to spend any time looking. The realtor said, "Let's just make an offer." So I offered $250,000, but apparently somebody else had been looking, and when they heard they had competition, they bought it. I'm so thankful because it wasn't the best place for us. Russell is awful busy, and the road went right by it.

JL: You would have been a long way from the med center.

MG: Yeah, it wasn't as ideal. And then in about May of 1981, the realtor heard about this other place we did buy from Nancy and George Wells. They're well known people in Dixon. (Nancy Wells was just written up in the Dixon Enterprise as the Dixon Person of the Year for all her civic duties, getting her award from Peter Timm, the veterinarian who takes care of some of our animals.) So anyway, Lois Faulken had met Nancy Wells who said she and her husband were ready to move back to town, and so it was very easy. The ranch hadn't been listed, and yet it was for sale. It was the best break in the world, because it proved to be a great spot. It had a two-acre pond. It was different. It was unique. It had this wonderful 100-year old redwood barn. This was what we were looking for. We were able to sell our house down in San Marino for a fancy markup, which put us in easy range of paying the price here.

JL: This ranch is southeast of Davis, on Maxwell Road in Solano County. How many acres of the old Maxwell ranch did you buy?

MG: Ten and a half acres. We moved in and Alice took command, and since then she's made it into her image.

JL: Did she start raising the sheep right away?

MG: Actually, I knew she wanted sheep, so when we went there to look at the place for the first time, I arranged to have a flock of sheep in the front yard, waiting for her. I drove her around the block about eight times, teasing her. It worked out great.

So my coming here to Davis was because we knew we wanted to live this lifestyle, and I didn't really worry about whether I would find something to do professionally any more than I worried about that when I went to USC. I just figured something would happen, and I'd do the best I could in the meantime.

JL: Where did they put you when you started work at Davis?

MG: I had an office in the university hospital [in Sacramento], upstairs, in a little tiny cubicle. I did autopsies. I hadn't done surgical pathology for fifteen years. Research had been my primary interest and means of support. I had some wild mouse work retrovirus research going on up here in Bob Cardiff's lab. We were looking for a leukemia virus resistance gene.

I was here less than a year when George Lundberg, the department chair, left precipitously, to become editor of the JAMA. I became chair, although it was never an aspiration of mine to be chair. My main interest was in building research collaboration, which is what Dean Hibbard Williams wanted me to do first and foremost. Attending to the day to day service-related issues, people problems and money issues at the hospital were concerns that I

didn't ignore, but were not close to my heart. I tried to delegate these administrative chores as best as possible, while focusing my efforts on research, teaching and academic matters.

JL: So you succeeded Dr. Lundberg as chair of pathology in July 1982, after Dr. Cardiff had served as acting chair for some six months.

MG: Yes.

I hadn't been here [at Davis for] more than a year when the primate center director, Charles Cornelius, asked me to talk to some of the veterinarians about an interesting disease that was occurring in monkeys that seemed to have all the characteristics of human AIDS. They had identified animals having a wasting disease with immunosuppression, and I listened to the story, looked at the slides, saw they were immunosuppressed, and said, "Gee, they've got AIDS."

JL: Were you aware at that point of the human AIDS?

MG: Yeah, I had been on top of it. I knew about the first cases in 1981. I was very much aware of it. I was prepared to see it and not at all surprised. I also knew at the very beginning that it [AIDS] was going to be a massive epidemic. And I knew we were sitting on a goldmine of opportunity. I thought we could really build science here, that with our monkey model of AIDS, veterinarians and M.D.'s could come together and share their talents and resources. Niels Pedersen, DVM, shared this vision. We were ahead of our time. The planners in the medical school who divvy up the resources hadn't really planned on AIDS, nor had the primate center. We were sort of like a weed in their garden. We tried to make things happen by bringing in outside money.

The medical and veterinary school deans, Hibbard Williams and Ed Rhode, and Charlie Cornelius, director of the primate center, were very supportive, but almost all of the funding for our research, including the research team, had to be raised from NIH grants. The university was unable or unwilling to provide major infrastructure for the Simian AIDS Program, so we built it up largely from outside money from NIH.

Perhaps we didn't do a good enough job of building a broad base. It was seen by some too much as Murray Gardner and Niels Pederson doing their own thing and not really a university-based priority. Some of the old guard thought we got money easily because AIDS was a big money maker, and all we had to do was snap our fingers. And therefore we were going to taint this holy school with less than the best by bringing in soft money scientists. I went up to this UCD "Think Tank" at Lake Tahoe in the early 80s and gave my two bit's worth about animal models for AIDS. I told them what a wonderful resource we had, that we were sitting on this goldmine. I explained what we could do with it now, how we should exploit it, build science and put ourselves on the map, especially since we were way behind scientifically in some of these new areas (e.g. molecular virology). But UCD was the slowest moving, most conservative place, largely because of it's history. I'm not trying to be too critical. It's just the way it evolved. We're the last to build company contacts and hustle for patents. We've been really slow to take advantage of an opportunity like this. When I presented these sentiments at the UCD "Think Tank," one of the old guard got up and shook his finger at me. "Young man," he said, "Young man, you're a prostitute for science. You're going to ruin this university because you're going to

bring in untenured people, do lousy science and fill up our building." For him, science had to be soil cycles and nitrogen fixation. Some of our distinguished faculty used to badmouth AIDS research because it's expensive and it has this image. They'd say, "People don't like AIDS, and they don't like the people that have AIDS, and they don't want to see this money spent on it. We ought to be studying prostate cancer, or something noble and worthwhile."

JL: How did the primate center figure into this equation?

MG: Well, this is where the monkeys with AIDS were, a unique resource present nowhere else in California. This was a Taj Mahal, waiting to be energized. They would have been in big financial straits if they had not had AIDS as a shot in the arm. Of course it has been a double-edged sword for them, because we have to have more facilities and this and that. But Simian AIDS now brings in about one-third of their budget. The Simian AIDS research project made jobs, recruited the key people that are now directing projects there. We really got things moving, but we never really did anywhere near what we might have done. But so be it. We got our initial Center for AIDS Research grant for animal viral research in 1987, and that was a coup.

Most importantly, the productive teamwork between MDs, DVMs and PHDs working on Simian AIDS, led ultimately to the building of the Center for Comparative Medicine (CCM), to be opened in late 1998.

By the way, long ago, when the vet school came to the Davis campus in 1948, I drove the truck that brought the horses to a farm on Old Davis Road, and I delivered supplies to the back of Haring Hall when it was brand new. I remember I had trouble backing the truck into the loading zone there at Haring.

The vet school has been here since 1948, and the med school came in 1968. Of course, there was a lot said through the 70s about how it would be great if the vet school and med school could work together. The first time that this could happen on a major scale was when AIDS energized and funded the scientists. We did put together a productive, interdisciplinary team. We made a center out of it (CCM).

That's what happened. That's what the CCM is all about. AIDS is only a piece of the action. The main mission is to get scientists to work together on animal models of human disease. Davis' uniqueness that I saw so clearly when I packed my bag [to come here] is that animals are useful models. Maybe plants would be too if we think about it more. Plants are being used for making human viral antigens now. Crown gall is a good model for oncogenes, so there are examples of how plants could be better used to learn about pathology. But we stay in our own world. My dad, a plant pathologist, used to say, "You guys could learn from plants. Read our literature and get acquainted. We can do a lot for you."

Even to this day, what is Davis? Davis has got a lot. This whole agricultural world. Veterinary world. Medicine world. Environment, Genetics. It has just been difficult for anyone to congeal into a meaningful focus what Davis is all about. Maybe that's the way a university should be. It used to be clear-cut. It was an ag school. It was all agriculture and experimental farms, things like that. But now it's all over the place. Animal models for AIDS is just one little piece of this whole scene, and not the major focus, except in the eyes of us that are making it

happen. So it has been fun here, and exciting to make science happen, and I believe that I have made a solid contribution. I wouldn't have retired, except with corporate downsizing they almost made it impossible not to. I wasn't even thinking about it.

JL: When did you retire?

MG: Oh, just last year, on June 30,1994. I turned 65 shortly thereafter, in October, so it was not that early. But they just made it almost irresistible, because they gave me seven years of service credit. Anyway, I'm working just as hard as ever, only doing it without any hospital stuff. I'm doing just about what I did before for about one-third of the pay. But I feel good about it. I feel I'm still contributing. I still have a grant. I'm still teaching. I give a new course to freshmen undergraduates on animal models for AIDS research. I don't travel as much because I'm not invited as many places, but I still get around.

JL: I was going to ask you about your travelling experiences. For a while, in the beginning of that AIDS research, you were all over the place.

MG: I was all over the world. I was in France, Africa, India. You name it.

JL: You went to the Pasteur Institute in France?

MG: Many times. And I knew the people there.

JL: When did you first go there?

MG: The first time was about 1984. I went there with our monkey virus right off the bat, comparing them with the new AIDS viruses. They were historic times. I was there when Bob Gallo was there and we saw what the French had. He came home and said he found the same virus within a few months. Did he take it from there? Did he know he had the virus? I was part of all that.

JL: Did Dr. Gallo work with NIH?

MG: Yeah. So I was there at the time all this was happening.

JL: You should document that.

MG: You know, I could, but it's so darned tied up in lawsuits and money.

JL: With whom did you associate when you went to France?

MG: Well, Gallo from this country, and of course, Luc Montagnier from France. But the people under Montagnier who made the discovery of the AIDS retrovirus were Jean Claude Cherman and Francois Sinoussi. Montagnier got the credit, but these young people did the work and made the discovery. And I was there when all this was happening. I knew every piece of it— the background, the individuals, what's happened to them since, and how they have been treated by science. I could write that history just like I could document the previous era of the Virus Cancer Program. Those are two exciting eras in science. My piece of the AIDS era has

been with the monkey model. That was followed here at UCD by Niels Pederson and FIV in cats. So I've lived through two exciting chapters in medical research, cancer virology and AIDS, and my role in each has been similar in that I have tried to pull people together.

JL: A catalyst, you might say.

MG: A catalyst. Yeah, I'd like to think so. More than that, I've actually helped to give the field credibility. I've helped to put it on the map by writing about it, documenting it. I haven't made a lot of great discoveries. I'm not in the lab digging very much, but I am able to crystallize or synthesize results from different disciplines.

JL: Have you worked with the human virus?

MG: Yeah, in comparison with SIV and FIV.

JL: Didn't you bring the AIDS virus back from France?

MG: Yeah, I brought the French virus back. I brought the first bona fide AIDS virus back on the airplane under my seat on Air France. I got off the airplane and forgot it, had to talk my way back on the airplane to retrieve it. We promptly showed the human AIDS (HIV) virus was related to the monkey AIDS lentivirus (SIV), but not related to another immunosupressive monkey retrovirus (SRV).

I was all over the world, Africa, India, giving talks here and there. I was trying to tell Indians in Bombay in 1989 that they were going to have AIDS there just like Africa, and their government, of course, ignored it. And, of course, it happened. It teaches you a lot about human nature, and a big part of what I do is education. I go out and talk about how not to get AIDS. Just this week I gave an hour's talk to ninth graders from Emerson Junior High School, all of whose mothers had to sign permission for kids to go hear about AIDS. It didn't bother the kids one bit. They asked lots of intelligent questions. I've organized the med students as a teaching group to go out to high schools and teach AIDS prevention.

I've even taken a talk show on KGO radio's "Open Line" on AIDS, with five million people listening.

JL: Do you feel like you're changing people?

MG: Yes, I think we are changing people's behavior. We are making some impact with the people that want to hear, but not as fast as you would hope, and it's a drop in the bucket when it comes to the third world where AIDS is spreading relentlessly. I don't think I had any impact in India or Africa, where education has had little effect. But I think we are having a positive effect in our own communities and in high-risk groups. On the other hand, it's like teaching people not to smoke. You've got to tell them for years, because it doesn't happen right away. You've got to be persistent. That's the preacher side of me, and it's very easy for me to become evangelistic.

JL: You have those genes. Wasn't your maternal grandfather a preacher?

MG: A Methodist minister. Since he had prostate cancer, I figure I got a set of genes from him which led to my prostate cancer and the end of preaching for him. On the other hand, my mother would have been happy had I been a minister. My dad was scientifically oriented, so he could care less about religion. I was lucky that I had a chance to do either. But I do like to preach. I get enthusiastic about what I'm doing. It's fun to convey that. That's the secret, I think, to teaching, and has made teaching easy for me. And it's fun because I have lots of stories to make it interesting, too. On the other hand, when you teach med students, you can't get off the beaten track or they get upset.

JL: You have been very involved in the teaching.

MG: I teach everywhere. I answer the bell whenever I'm invited, as you can see from my CV list of talks. I'd go off and talk all the time about different subjects. I give cancer lecture, and now I mostly talk about AIDS. But biology is fun. Life is a great adventure. It's fun to get kids excited about it. It's about as simple as that.

JL: Weren't you here at a time in the 1980s when the pathology department, in general, was expanding?

MG: During my tenure as departmental chair, from 82-90, we recruited six new faculty members: Ray Teplitz, Liisa A.Russell, Jerry Kost, Ed Larkin, Philip J. Vogt and Stephen Hinrich. We recruited mostly for positions that had become available when people retired. I didn't build a lot of new service positions that had not been on board before. We certainly maintained our services. We had about seventeen people on the faculty when I took the chair. I was not overwhelmed with the economic problems of managed care, because those times had not gotten as tough as they are now. I could see what was happening, and the pressure was beginning to mount, but I was able to avoid some of the business negotiation unpleasantries by asking the dean to defend what we had and represent me to the hospital director. When the hospital director wanted to make cuts, I told the dean I wasn't going to play those games. I said, "If you want me to do what you asked me to do, then get this guy off my back," and he did. He defended the department, and helped me, i.e. build research, justify budget and keep anybody from being fired or their salaries cut.

JL: There was also the problem of some of the labs being dislocated.

MG: Well, splitter labs are always a problem, and Lundberg had done a good job of routing them to pathology labs again. That had been his main mission. I just maintained that. I was doing a good job of making science happen, making research go in the department, primarily on the campus side. I wasn't being a very effective leader in Sacramento. It was clear that I was not the person to lead the charge into the future over there. It wasn't my main interest anyway.

JL: You did get the J-Lab in Sacramento.

MG: Oh yeah. This was a joint research lab between medicine and pathology, located in FOLBII. We got that started, and it was primitive in the beginning. It ended up being a fine lab, but initially we just broke ground, knocked down walls, put in the equipment for our research, and we got it started.

JL: You physically went in and knocked out the walls yourself?

MG: That's right. I removed some and built other walls with hammers and nails myself. We got people working there that were paid off soft money. We were on the right track scientifically. This lab has now become part of the new Cancer Center at UCDMC.

JL: So the J-Lab was really one of the first example of some interdisciplinary research that happened within the medical school.

MG: It sure was, and it proved to be successful. We even did some of the first AIDS work there. We compared the viral HIV-I isolate from France and the USA with our monkey AIDS isolate and found that the human and monkey viruses were distinct. However, we also noted that the USA (Gallo) virus was identical to the French isolate. We were the first to show that.

JL: You did that work in Sacramento?

MG: We (Martin Bryant) did it in the J-Lab. Jerry Lewis (in medicine) provided the original J-Lab space.

JL: Considering what was happening between the veterinarians and the primate center and the pathologists on this campus, that J-Lab was the complement to it over at the med center.

MG: Yes it was. It was a solid example of interdisciplinary cancer research right on target on oncogenes and tumor suppressor genes.

JL: Is it still standing?

MG: Not physically, but it has evolved into a Cancer Center lab with some of the same people still working there.

It became clear to me in 1988, and it was also pointed out to me by others, that the path department needed someone who would identify with the hospital, be a strong on-site leader, and better handle the business end of it. I could see that Cardiff was interested in being chair if he had enough resources to do this job the way he felt it should be done. We had a national search and chose Cardiff.

JL: Dr Cardiff had been in the department since 1977.

MG: Yes. I came to UCD to work with him and he shared his lab with me. He knew every Indian trail, and he believed in management as a discipline.

JL: People were pretty well established in their respective fields of research by the time you came.

MG: Yes. I didn't like to have to write job descriptions. I let everybody do what they could do and encouraged and supported them. I would rather have them do what they thought they

could do and do it creatively. So it was hard for me to live in a bureaucracy where people had to fill defined boxes.

JL: Well, one thing you did while you were chair was to organize the pathologists in the immediate area into the Northern California Pathology Society.

MG: I tried to bring them together. There was little interaction between the university pathologists and those who were out in private practice. I thought, "Gee, we ought to have some kind of professional organization and have some fun together, like a town and gown club."

JL: Were you the charter president?

MG: Yeah, I think so.

JL: I see you were president from '85 to 1990.

MG: Yeah. But now Cardiff has taken the presidency and the organization is growing more.

JL: It now takes in all of northern California.

MG: Yeah, the more the merrier.

JL: It's good to have that feedback between the community and academic pathologists.

MG: Particularly now that there's pressure to become competitive with each other; it is nice if we're friends and stay friends. Also, I brought this Edmondson Fellowship program up from USC and that's booming now.

JL: That's to encourage young people to become interested in science.

MG: That's right.

JL: Do you take them as high school students?

MG: High school or freshmen in college. We give them summer experiences in clinical or basic research labs. It's tailored for minority students. Frank Loge, the hospital director, has pumped a lot of support into it. It's now very successful.

JL: I understand that the residency program in pathology was enlarged during this era that you were chair.

MG: Well, we added eight or nine residents at the hospital that had been affiliated with the VA, the reason being that the VA hospital needed a university affiliation to strengthen their training and we needed them as a patient resource.

JL: That occurred after the 1987 Loma Prieta Earthquake badly damaged the VA Hospital in Martinez.

MG: Yes. We did a good job training residents there. One of our big struggles has always been to try to train the next generation of academic pathologists. We don't do a very good job of that. Our residents go into clinical practice.

JL: Do you think that might have been different if the hospital had been built on the campus.

MG: Sure.

JL: Perhaps it will happen more with this new comparative research program.

MG: That will expose young people on this campus to experimental pathology. But it's still a big challenge to attract young MDs into a research career. Any rate, that opportunity is here.

JL: Your CV lists 322 publications to date. Which ones do you think made major contributions to scientific knowledge?

MG: I think my major contributions, both at USC and UCD, have been to build and nourish teamwork among my scientific colleagues. At USC (1963-81) my research was directed to the natural history of oncogenic retroviruses in wild mice, cats and humans. At UCD (1981-date) I have studied the immunosuppressive retroviruses that cause AIDS in monkeys and humans. So I have had a 35-year love affair with retroviruses that has opened a fascinating world of science that I never learned about in medical school or as a pathologist. And this allowed me to meet many interesting and famous people and to travel all over the world. Looking back now over the past 35 years, I can summarize my research accomplishments, apart from building teamwork, helping my colleagues and sharing resources, as follows:

At USC (1963-1981):

1. Discovery and characterization of the natural history, biology and pathogenesis of oncogenic Type-C and Type-B retroviruses in wild mice (*Mus musculus*), the progenitor of laboratory mice.

2. Discovery of two new oncogenes (*feg & fgr*) in naturally occurring sarcomas of domestic cats.

3. Discovering new strains of feline leukemis virus, understanding its natural history, and showing that it is spread among cats by saliva, but is not infectious for humans or other domestic house pets.

4. Helping my colleague Robert McAllister isolate and characterize a new endogenous retrovirus (RD114) in domestic cats.

5. Helping my colleague, Dr. Suraiya Rasheed, isolate and characterize rat sarcoma viruses that led to the naming of the ras oncogene, an important "player" in human cancer.

6. Showing that LA smog , although an irritant, was not oncogenic for wild or lab mice or humans or other domestic animals.

7. Showing that inherited retrovirogenes in wild mice (and humans) had no demonstrable virologic function.

At UCD (1981-date):

1. Discovery and characterization of the natural history, biology and pathogenecis of immunosupressive retroviruses (Type D retroviruses) in Asian (Macaques) and African monkeys.

2. Efforts, mostly unsuccessful, to develop vaccines to prevent Simian AIDS.

3. Building of an NIH-supported Center for AIDS Research (1988-1992) at UCD, based on our animal lentivirus models of AIDS.

Rather than list papers in my CV that encompass these accomplishments, I will attach several of my summary type papers which cover this material. I've had a lot of fun, feel good about my accomplishments, never felt at all insecure, and like to think I am still going strong. My dad once told me, "You can make a difference at your school (then USC)." In retrospect, I think I did help USC (which now has an NIH approved Cancer Center) and I did help UCD (which now has a Center for Comparative Medicine and will soon have an NIH-approved Cancer Center). Most importantly, I think I have not hampered any of my colleagues and have unselfishly helped them in advancing their careers, and in so doing, I have had a great adventure.

JL: Your CV shows that you've worked with lots of different people over the years collaborating on these research projects. And you have received a few honors, too. You were named Alumnus of the Year at SC, and you were chosen as commencement speaker by students in the School of Medicine at UCD in '89. That is an honor!

MG: That was nice!

JL: Is it true your students gave you a tranquilizer prescription one time?

MG: Oh, they gave me a big prescription. They wanted to slow me down. I talk so fast.

JL: It's probably hard to take notes in your classes.

MG: I try to make clear outlines, to speak more slowly and to stay on course, but I don't always succeed too well.

JL: You've had a very interesting career so far and I appreciate all your recollections.

ROBERT D. CARDIFF, MD, PH.D.

CHAIRMAN OF THE
DEPARTMENT OF PATHOLOGY

UNIVERSITY OF CALIFORNIA, DAVIS

JULY 1990-AUGUST 1996

Joann Larkey Interview with Dr. Robert Cardiff

April 7, 1995

JL: Dr. Cardiff, you were chiefly responsible for organizing the 25th anniversary celebration of the Department of Pathology, held at the Pathology Forum on March 3rd and 4th, 1995. That forum featured all of the past and present chairs of the department as speakers.

RC: I think that probably the thing I appreciated the most was Dr. [Wilfred] Toreson coming up to me when he was leaving and saying that he had left [in 1977] with some mixed feelings about the university, but that this forum was sort of a very healing experience and he hadn't really understood how much appreciated he was.

JL: He did warm to the occasion. The first time I talked to him he was rather negative about doing the proposed interview. Then, as we talked about it more and more, he wanted to tell his side of the story, so to speak. I think he also really valued other people's comments about him.

RC: I think for me the emotional highlight was at the end of the afternoon when we presented them [the past chairs] with plaques. They all stood up there [on the stage] and the audience stood with prolonged applause. I thought that was really, really very nice.

JL: It was rather unique to get all of the past and present chairs together in one place and to have them think and speak about the things that were important at the different times while they were in charge of the department.

Now that you've heard the comments of others who preceded you as chair of the Department of Pathology, I wanted to get your input about your early career, about how you got into pathology, and the things you think were significant over the years.

RC: How far back do you want me to go?

JL: Well, why don't you start with your family and early childhood.

RC: I was born on December 5, 1935. I was delivered by a third year medical student at the University of California at San Francisco [hospital].

JL: Was San Francisco home to your family?

RC: My parents, George Darrell Cardiff and Helen Kohfield Cardiff, were living there at the time. My mother, as the story goes, went into labor and she didn't have enough money to pay the trolley fare, so she walked twenty-six blocks in labor to deliver me, and so I'm a native of California.

JL: Did you go to school in San Francisco as a young boy?

RC: No, that was during the Depression. My father had family in Santa Cruz and so shortly after I was born, my mother, brother and myself went to Santa Cruz to live with the grandparents while my father continued working in the Pest Control Business in San Francisco and on the Peninsula. Eventually, he developed enough of a business in Santa Cruz. He moved to Santa Cruz and he established a pest control business. He maintained the pest control business, I think, for something like thirty-six years in Santa Cruz.

Originally, the plan was that I would inherit and take over the business. So when I graduated from high school, I went to Cal Poly at San Dimas and then San Luis Obispo, which was viewed as a technical college that would prepare me for entomology and for the pest control business. I spent a year on the Pomona campus for a variety of reasons. I spent the second year at the San Luis Obispo campus. Probably the most influential thing for moving was that I was very interested in playing baseball at that time, and the San Luis campus had a much better caliber baseball team.

JL: So that influenced your career choice.

RC: That was very influential. And in part, during that period, it was sort of like the early days at University of California at Davis, the San Dimas campus was a satellite campus and the rules required that a student spend three-quarters there during the four years on the San Luis campus. A friend of mine, a roommate, was going to spend the summer on the San Luis campus, so I went ahead to the San Luis campus.

It was clear that if I was going to go to school during the summer and not work, I was going to have to get a job. I am not quite sure how, but I eventually heard that there were jobs available as psychiatric trainees at the Atascadero State Prison, and so I applied and I became a psychiatric trainee. That actually was my first exposure to medicine, and it was an absolutely fascinating event and experience. Those were the early days of the use of the Thorazine type drugs to control psychosis, and they were still doing electric shock. And so there were a number of people wandering around terribly overdosed on these Thorazine drugs. You could tell them because first of all they were Parkinsonian and second, they drooled so much their bed shirts were just sopping wet. Some carried around towels to drool into. During that summer, I worked nights.

JL: You were going to school during the day and working at night?

RC: Yes, I slept during most of my classes. It was embarrassing because at Cal Poly the classes were sort of small. Usually we had about twelve people in our class, and it was painful because I had to struggle to stay awake.

So that was my first exposure to medicine, and I think it was a very pivotal event in my thinking. I also played baseball and it turned out it was not a very good team, but we played against very good teams. The coach was a very poor coach and not very inspiring, so it was sort of the end of my baseball career. At that time I knew, one, that I didn't have the physical skills to play professional baseball, and number two, that there had to be something else.

The interesting thing that happened at that particular time in my life was that I was befriended by an Afghanistani boy, Rafi Youngosai, who for some reason took a shine to me. He was older and wiser and really sort of an interesting fellow. He had gone to Cal Poly at San Luis Obispo because he had flunked out of UC Berkeley, and he had to make up grades in order to get back into Berkeley. We had several labs together and we sort of befriended each other. The next summer, instead of staying in San Luis Obispo, I went back to Santa Cruz to work for my father in the pest control business, and I brought Rafi with me. He worked during the summer and stayed with my family. He had made up his grades, and when he decided to go to Berkeley, I went to Berkeley.

JL: You were a junior at that point, so it was not difficult to transfer?

RC: Well, there were a number of courses that were sort of questionable about whether I could transfer, and what I could transfer. As it turned out, I transferred plenty of my units and it worked out fine.

And so I started out in Entomology and Parasitology, continuing along the line. There are two things that happened to me at that particular era. The first thing is I took a course from Edward Steinhaus, an insect pathologist who had a style and an interest, absolutely riveting to me as a young person. One of the things that was interesting was that he put everything in a historical context, and I really loved thinking about science in a historical context. The other thing was that it encouraged me to think about being an experimentalist. So I think he was very influential. And there was another entomology professor who taught insect morphology named Dr. Ed Bonhag who also was very stimulating to my mind and sort of encouraged me along the line. I can recall a discouraging day. I think there were twelve students in his course, and it was a huge course, about six units. The first mid-term came back, and it was actually the first mid-term that I had ever taken at Berkeley. He handed back the mid-term, and there was my paper right on top. It was a 92 and I thought, "terrific, fantastic." So I pulled it off and the next one down was a 93, and the next one down was 95. I had the lowest grade in the course, and it was really a sobering experience. I found out that I had done OK, but these other people were going to be professional entomologists. I think those two professors really influenced the thinking I did, and particularly the study of disease. Steinhaus pointed out that our understanding of the germ theory and so on was really based on entomology, that Pasteur's original ideas and original studies came about because he was trying to save the silk worm industry in France, and what he did in medicine was a spin-off in the subsequent development. It was very fascinating.

The second thing that happened was that I took a course in embryology from Richard Aiken. I can remember that I went into the lab, and I had heard this was a pivotal course for pre-meds. If they got an A in this, their future was assured. If they got a B, it was shaky, and if they got a C, forget it, they weren't going to get into medical school. And so the atmosphere was very tense. I got to the assigned lab late. There was only one seat left. There was this tall young man. He looked at me and said. "Hi', I'm Ron Bean and I've got a 4.8. Who are you?" And that was sort of my introduction to pre-meds.

At that point, in fact, I became involved in a study group that in fact Ron Bean organized. What he did was he tried to get the smartest kids in the class and formed a study group. We kept

detailed notes, annotated textbooks, all these kinds of things, and really were superbly organized. But working with these people, coupled with my experience at Atascadero, sort of riveted my attention, and I thought, you know....Ron was sort of typical pre-med, and to this day I supposed we continue to question his character, but I really met wonderful people who are idealists about medicine. They had quite a different viewpoint about why they wanted to get into medicine, and I think they became very influential.

And as I went on in Berkeley, I was doing fine in grades. Even though my 92 was the lowest in the class, everybody still got an A. And sort of under the influences of these people in embryology class, I subsequently applied for medicine at [UCSF] medical school.

JL: When were you admitted?

RC: I finished my B.S. degree in entomology in June 1958.

And in fact, immediately before I entered medical school, I spent the summer in New Mexico in a required field trip for entomology. We collected insects in the Chiricahua Mountains during the summer, which was quite an adventure in of itself.

JL: You did graduate then in '58, Summa Cum Laude, I see by your curriculum vitae; so you kept up your grades.

RC: That was a little surprise. That was sort of interesting because I hadn't expected to do that well. In fact, in my last year, in order to graduate, I had to take twenty units a semester.

JL: That's a heavy load with lab courses.

RC: Yes, lab courses and everything. I can remember the fall semester, I had twenty units, with heavy lab courses. I went into the finals with straight A's and I came out with straight B's, and I missed every one of those by one of two percentage points. It was just the way the chips fell, and it is sort of a philosophical defining point because the difference between an A and a B was one heck of a lot of work. And with twenty units, I just couldn't devote that kind of work to every course. Plus, I was working at that point, so I lived in a sorority. I was a house boy in a sorority. I served three meals a day and did maintenance on weekends, so, I stayed busy.

JL: Did you ever think about applying for other medical schools?

RC: I applied for two medical schools; UCLA and San Francisco. I got the flu and missed the interview at UCLA, and so I was accepted at San Francisco, and never bothered about anything else. No ambitions here, so I just sort of went on to San Francisco.

JL: So you spent four years there. Did you become interested in pathology during your medical school days, or did that come later?

RC: Well, let's see, there's two parts to that. The nicest thing about San Francisco was it had a very active semi-pro baseball circuit, so I got to play baseball all year round.

JL: You probably didn't play at Berkeley; not with twenty units of course work a semester.

RC: I had thought about it. I knew one of the coaches, Pete Van Houten, but the players had to be there at 1:00 every day, and I just couldn't do it. It was just too much. So, anyway, I played all year round for four years at San Francisco. I pitched on the weekends and played basketball during the week. One of the things that had happened out of the all of the B's in UC Berkeley during that one full semester, I thought, "You know, I'm coming out of this with the same experience, the same intellectual content, it just isn't worth it to bury myself in grades in this thing. And I'm just not going to compete. All I'm going to do is make sure I get through school, and I'm going to try to maintain a balance." So one of the ways of maintaining a balance was playing ball.

I think that Steinhaus had really convinced me that being an experimentalist was a delightful thing. In fact, when I was at Berkeley I started doing some experimental work. Interestingly enough, one of the things I was doing was experiments on cockroaches, and I had maintained a colony of cockroaches in the basement of the sorority.

JL: I'll bet they were happy about that.

RC: Well, the housemother found them one day and that was the end of the experiment. She wasn't very tolerant of my experiments. I had three large fifty liter crocks filled with cockroaches that I was doing experiments on. She was not very tolerant of that.

When I went to San Francisco, I immediately began doing some kind of research. I worked first with Malcolm Miller who was in the anatomy department, and then in subsequent years I worked with Carolyn Piel who was in pediatrics. None of that was really productive in the sense of publishing papers or developing life long skills, but what it really did was it allowed me to explore a number of different kinds of technologies, and to explore my own mental agility. It was something I really liked to do. Probably the critical period in my life at that point was that I decided that I wanted to do a specific project, which was to do electron microscopy in the lateral line system of congo eels. I won't tell you why, but it was interesting to me, and I was interested through Malcolm Miller and peripheral innervation, [to know] how the body receives signals from the outside. So the lateral line system in fish and eels was very interesting and the Congo eel had some particular characteristics that I found interesting.

JL: Had someone there been doing research in that area?

RC: No, it was something entirely of my own invention. So I wandered around trying to find somebody that had an electron microscope and would let me do the project. In the process of wandering around, I was told that in pathology there was Sefton Wellings. I hadn't heard of Sefton Wellings, but I went up and met Sefton Wellings. I told him about my project and what I wanted to do, and he said, "Well, unfortunately, I'm leaving this fall to go to the University of Oregon where I'll be in the Pathology Department, so I can't help you. But I'll send you to Dr. Carolyn Piel. If anybody has this kind of imagination, he should really go into pathology. And so if you are ever interested in going into pathology, you write us at the University of Oregon,

and we'll bring you in and train you in pathology." And that is exactly what happened, eventually.

There's another interesting story built into this that always amuses me and I think about it frequently when I'm dealing with students. Carolyn Piel didn't have any money to support me during the summer, so I had to sort of scramble around trying to figure out some way of getting money. I heard that Salvador Lucia had a fund that he supported medical students with. Salvador Lucia was, of course, a very famous internist and wine connoisseur, and really a well known character, and so I applied to him for a fellowship to do research during that summer. He brought me in and he sat me down. He told me that in the position he was in now, one of the real joys that he had was watching the development of promising young people. He said he had watched my career develop, he'd watched me go through classes, and he was convinced that I was going to become one of the great leaders in medicine. And so it gave him great, great pleasure to give me this fellowship for the summer....

JL: That was certainly an encouraging statement.

RC: It really was. And so we walked outside. He put his arms around my shoulder, looked at his secretary, and he said. "Make sure that Mr. Johnson here gets a fellowship."

JL: (Laughter)

RC: So, he didn't have the foggiest notion who he was talking to. I guess I must have looked crestfallen, because the secretary looked up at me and winked. I took his money, anyway, under false pretenses. I wonder....

JL: Do you know who Mr. Johnson was?

RC: No, I have no idea. Probably a great leader in medicine somewhere.

JL: So you were working that summer...

RC: Then when I finished medical school, I went to Brooklyn Downstate, [New York University, Downstate] for an internship. There were two reasons for doing that. The intellectual context of pathology really appealed to me, so I thought I wanted to go into pathology, and I wanted to have a rotating internship in a place that was very, very active. Downstate, which was the Kings County Hospital, seemed like an ideal choice. And also, my wife was flying for Pan American at that point, and she was flying out of New York.

JL: You were married in 1962?

RC: Yes, I married Sally Joan Bounds. We got married in Piedmont, California and from our honeymoon, we went directly to internship. Internship was internship. Sally was a stewardess for Pan Am and she flew out of New York and I was never home. But, we were able to travel some. Actually, it was a beautiful year, because she was traveling and when she was home I could sort of rearrange my schedule so we could try to get our schedules put together. It was a

wonderful year. Probably the capstone to it was at the end of the year, before I went up to the University of Oregon for my pathology training, we flew around the world on Pan Am, which was wonderful, and we did it for $117.00 apiece.

JL: That's even more wonderful.

RC: Yes, that was terrific. We basically paid the taxes on the tickets and that was it. Anyway, that was a real adventure.

Then, I really did follow Sefton Wellings up to Oregon, and spent my residency time in Oregon, where I immediately became interested in doing research. They gave me a fellowship one year to devote entirely to research.

JL: In what field?

RC: I was interested in electron microscopy and enzyme histochemistry at that point. The chair, Jackson Crane, was a very, very impressive person. Do you know the poem Richard Quarry by Edwin Arlington Robinson?

JL: Yes.

RC: OK, Jackson Crane was a Richard Quarry. He was a prince of a man. He had a photographic memory. He was good looking, wealthy, had a way with women, was a role model, chair of the department, but he had personal problems. He ended up being booted out while we were there in Oregon, and so at one point I looked around for another residency. But, Wellings took the chair and convinced me to stay on, so I did.

Then he encouraged me to do a fellowship and, in fact, go to UC Berkeley. Since he had gotten his Ph.D. from Berkeley, I had a connection there, so I went down to Berkeley and the type of training I got there really made sense to me.

JL: You spent three years in Oregon?

RC: Yes, three years in Oregon, and two years at Berkeley.

JL: With whom did you work while getting your Ph.D.?

RC: I did it with Ken [Kenneth B.] DeOme, in Ken's lab, which was the Cancer Research Genetics Laboratory. He is a wonderfully permissive individual who really allowed me full rein. I did almost all of my work with Phyllis Blair who is in the Department of Microbiology and Immunology. And probably some of the pivotal work that I did was with Peter Duesberg in Dr. Ed Robinson's lab up at the Donner labs. I ranged all over the place. Wherever it seemed there was something interesting for me to do and people were interested, I just simply worked with them. I did quite a bit of my work with Adeline Hackett in the Naval Biological labs. So, I hopped all over the place. And because I was a member of the Barry Plan, I had only two years to finish my degree.

JL: What was the subject of your thesis?

RC: My thesis turned out to be basically the culturing of the mouse mammary tumor virus. My intent at that point was to develop the basis for the molecular biology of the virus. We were going to have to learn how to culture the virus and to do tissue cultures. At that time, there weren't really any other tissue culture systems, so basically that was my thesis—developing adequate tissue culture systems and beginning the molecular biology of the mouse mammary tumor virus. DeOme recognized the value of the project, and sent me to the right people to accomplish the project, and that reflected the character that was basically DeOme. Those were very tough years because I knew I only had two years to finish this.

JL: So you were obligated for some military service at the end of your training.

RC: Absolutely. Plus, we had our second child. The first one was Darrell, born up in Oregon. This one was Todd, born at Alta Bates, and it was really touch and go. Everything turned out fine, though. And I finished the Ph.D.

Actually, all my Ph.D. experimental work was done in a two-month period, everything else was preparation. But the reason I was able to complete it in two years was, number one, when I was working with Wellings, I started defining what the project was, and so I had most of the technical tools that I needed. Plus, I had the basic concepts. Medicine is really a very good degree in general biology. So when I went back to graduate school, I knew the vocabulary and most of the concepts. I didn't have the experimental detail that was required in graduate school, but while most graduate students were learning vocabulary and concepts, I was working on experimental detail. So it was quite possible to do my degree work quickly that way.

JL: You had the building blocks to make it all happen.

RC: Yes, all of the pieces were there and the foundation was there, so it wasn't difficult to push on through. As with a lot of experimental things, there are a lot of false starts. But in retrospect, they are all more building blocks, and when I hit the magic experimental breakthrough, I could spend two months of really concentrated work and do it all.

JL: Did you feel you made a breakthrough at that point?

RC: Oh, yeah, we'd gone from not producing any virus in culture to all of a sudden realizing how to do it, what the tricks were and going through all of the experimental procedures.

JL: That was a major step for medicine, in general.

RC: Well, at that point, for that particular field. When I started graduate school Harry Rubin had said, "Mouse mammary tumor virus is not really a virus and here's the reasons why." So my work was very helpful. Our tissue culture system allowed us to metabolically "tag" the virus and prove, with Peter Duesberg's help, that MTV is an RNA virus. We went on to prove, using immunoassays, that the virus that we were producing in culture was the same virus that was

being detected in the mouse milk. Subsequently, we fulfilled Koch's postulates by proving that the virus produced in culture caused nodules and tumors in mice.

And from there I went into the Army. That was another one of those kinds of things, just sort of scrambling around trying to find some kind of post. I ended up in a neuro-psychiatry unit because I knew electron microscopy and they wanted to set up an electron microscope. As it turned out, I never did any work for them, but spent three years setting the microscope up there. But there was a group of virologists on the floor below us who at that point really needed somebody who knew advanced immunology and molecular biology. They had the virology part, but they didn't have the molecular training that I had had. So I was very fortunate, and I just sort of stepped into another one of those situations where they were just waiting for somebody like myself.

JL: Where were you stationed?

RC: That was at Walter Reed Army Institute of Research, Washington, D.C. I did three years of research there. Basically, that was my focus, really an ideal circumstance for me. Once again, the people that were involved insisted that I do neuro-psychiatric research, but they recognized that I was doing what I was brought in to do in terms of setting up in the electron microscope lab, while I worked mostly downstairs in virology.

JL: They did not have an electron microscope when you arrived?

RC: They had a pathology unit that had an electron microscope lab, and, in fact, a number of people that I know now in pathology—Dick Estensen, Rob Rock, and some other people—were all out there at the time I was there. They were in pathology. I had hoped I was going to go in and work in pathology, but they already had their slots filled.

All your life you meet people who take the time to listen and try to help, and that's become part of my rule as an educator. How many times have I met somebody thoughtful, who listened and encouraged my work, or something like that, somebody who has really said the right thing, done the right thing for me? In this particular instance, my contact was Dr. Helmet Sprintz, who was at Walter Reed and the Chief of Experimental Pathology. When I went out to interview with him, he told me that he didn't have any jobs, but he knew people in Neuro-Psychiatry were looking for somebody who would install an electron microscope for them. If I was interested, he would recommend me. Well, that was a heck of a lot better than going to Viet Nam.

JL: Yes. That was your choice at that point?

RC: Yes. I either had my family and three years, or Viet Nam and two years, and there was no contest. It was funny, because in order to get a preferred duty station you had to sign up for three years. Colonel Ed Buescher who was the commandant at that point was very insistent that I sign before I came on duty, before he would approve my papers. Subsequently, I found out from Dick Estensen that the reason that happened was I was taking Dick's place. They thought Dick had agreed to stay three years, but at the end of two years he was mustered out. Then

Buescher came running up to him and said, "I thought you said you were going to stay three years," and Dick looked at him and said, "You asked me if I would think about it. I thought about it, and I'm not going to stay." And so, Buescher was very insistent that I sign the contract before he would approve my papers. Anyway, those were three very good years, very, very productive in terms of research.

JL: So, from 1968 to 1971, you served in the U.S. Army Medical Corps, in the Research and Development Branch Division of Communicable Disease and Immunology, Department of Virus Diseases, at Walter Reed Army Institute of Research. What was your next step?

RC: Actually, when I was in graduate school at UC Berkeley, I started looking around for jobs, but Wellings, by that time had come here [to UC Davis]. When he became chair, he asked me to come out. I looked at UC San Diego, I looked at Columbia, and I basically had a choice between San Diego and Davis. Davis was wild, wild west, and there were a lot of leadership vacuums there. There was a tradition of cooperation, really built around Dr. Stowell's HSAA [Health Science Advancement Award]. So I could come here and there was a tremendous springboard. I didn't have to buy equipment; I didn't have to go out for grants that required all that heavy equipment. Everything I needed was basically here and available to me. If I went anywhere else, basically, it was a different kind of support system and I would be required to pay my dues. They'd give you a microscope and a closet, and maybe after four years they would let me out of the closet. Here [at UC Davis School of Medicine], there was space, a different kind of welcome. Stowell had made sure that the basic equipment was in place and the infrastructure required for basic science was provided.

JL: Had you known Dr. Stowell previously?

RC: I had not known Dr. Stowell. My only contact was through Wellings. And because I had not done a fellowship in traditional pathology, I was simply out of contact in terms of pathology medical politics.

JL: You hadn't been attending meetings regularly?

RC: Yes, but I had never gone to a pathology meeting. I had no real contacts with academic pathology, and the contacts that I had and the jobs that I looked at were really basically through some kind of experimental group. Anyway, I came here to Davis in August 1971.

JL: What was your physical set up as far as office and laboratory space when you first came?

RC: We had the old temporary [Surge] buildings, and we moved in there. HSAA was across the street. We had to clean out Wellings' fish tanks in order to move into the lab. I brought in three recruits. Two people, Manny Fuentes and Yosh Teramoto came with me from the Army, and I recruited Larry Young who had been Ken DeOme's technician at Berkeley. Larry moved on up here with me, and we've really worked together ever since.

JL: So you more or less brought in your own technical staff to help you with your research?

RC: Yes, all of these people were more than a technical staff. Yosh actually was a graduate student, so he was my first graduate student. Manny was a trained chemist out of the Army. He was sort of looking for a job, wandering around, so I convinced him that maybe a chemist might play a role in the kind of molecular biology that we wanted to give him. He stayed with us for five years and did a really terrific job. He is originally from San Diego and wanted to go back to San Diego, so off he went. Academia is a transient community. Very few people stay around.

JL: You're one of the long-timers.

RC: Both Sally and I reflect back on the different kinds of experiences. Because for the first ten years of marriage, we moved seventeen times. In the last twenty five, we haven't moved an inch.

JL: When you did come to UC Davis, what areas of research were you to be responsible for, and has that changed over the years?

RC: Well, basically, Wellings had put together a breast cancer group and when I arrived, I was really initiated into the work that he and Hanne [M.] Jensen would become famous for. Conceptually, I was going to provide the basic science part of a breast cancer group that also included Glen McArn. That was the basic group.

One thing was that Wellings became chair.

JL: Yes, that happened rather quickly after he came to Davis. When Dr. Stowell was asked to go out and head the Primate Center, then Dr. Wellings took over as chair.

RC: That really took a tremendous toll as far as his [Wellings'] research was concerned. And that was coupled with his domestic situation. He basically didn't have the emotional energy to support a research group and fulfill all of his other responsibilities. Another component part of it was, in retrospect, that in order to put together a group like that, you really have to have strong clinical leadership. We never really had strong clinical leadership in breast cancer, so you couldn't really develop a comprehensive group. As time went on, it became obvious that the kinds of things that I would have liked to have done for him, and that we had talked about originally, were not going to happen.

The Medical Center [in Sacramento] did not have the clinical resources. I took one look at the Medical Center and it became very clear to me that I was not going to make my career as a trained surgical pathologist. There were not the resources available. The Medical Center was not in the mood to provide those resources....

JL: Was the lack of clinical leadership at the Medical Center in Sacramento because of the hospital director or related to the medical school's agenda?

RC: I think that the leadership wasn't there that was needed to do it, and it was not something that was malicious or a set agenda, more ignorance.

JL: The Department of Pathology was under the Division of Surgical Sciences, was it not?

RC: Yes, for a number of years.

JL: When did that change?

RC: Oh, boy, that didn't change until Lundberg came as chair in 1977.

JL: When they did the whole curriculum revision of the School of Medicine?

RC: Yes, I think that at that point when Tupper stepped down [as dean in 1980], the division structure started breaking down. But we were originally under the Division of Surgery, and that was headed by Dr.[Earl F.] Wolfman. Resources simply weren't there. I suppose that I could have spent the time and energy trying to organize the resources. My own personal style is much more of an experimentalist than that of an encyclopedist. I don't collect things. I don't have a shell collection. I tend not to collect and categorize. When confronted with a scientific problem, I am more likely to think of a clever experiment which will prove the point rather than collecting a lot of data and trying to figure out what the data means. So my instincts were not right for providing that kind of leadership. It really was sort of a very pragmatic thing. I looked at what was at the hospital, what the demands were at the hospital, what the resources were, and I looked at the kind of money that I was able to pull in terms of research contracts and grants. There was no contest. I could be perfectly happy doing experimental work, so that is the direction that I took.

JL: No doubt there was a great need for experimental research.

RC: Well, there was an interest in research, although you can dispute the need. It was interesting. I think I told you this story before about my promotion to professor. Several of my colleagues in the Department of Pathology were asked to evaluate my research because the ad hoc promotions committee was unable to do that. Basically, they [my departmental colleagues] wrote back to the committee and said, "Sorry, we can't do it. We don't understand the research, therefore we don't consider the research relevant." At that particular point, I was working with Harold Varmus and Mike Bishop who won the Nobel Prize for oncogenes in 1990.

JL: Someone should have been able to figure that out.

RC: So much for everybody's vision for the future. In any case, early in the '70s, I was having fun. I was able to develop a research laboratory with Larry Young and others, established the Diagnostic Electron Microscopy Laboratory with Bob Munn, started the Diagnostic Immunopathology Laboratory with Judy Lund, did general surgical pathology, ran the Ob-Gyn conferences and cut my teeth as an educator.

JL: You were appointed an associate professor when you joined the UCD faculty in 1971, and were promoted to full professor in 1977?

RC: Oh, yes, oh, yes. There wasn't any hold up. That was during Fred Toreson's reign as chair.

JL: He has said that the main reason for him wanting to come to Davis was because both you and Dr. Wellings were there already. He said he knew that you were going to be a good research combination and he was interested in being a part of it.

RC: I am sure Wellings was a part of it. I'm not sure that I was. I was too young of a whippersnapper that anybody could be sure what I was going to produce. I think that my real reintegration into the department, so to speak, came when George Lundberg got here as chair in 1977. I served as vice chair under Wellings, and had tried to balance things, but it was all a learning experience.

JL: You served as vice chair for a number of years, from 1973 to 1979 and from 1982 to1988, according to your CV. You also served as acting chair for periods in 1980.

RC: Well, when Wellings resigned in 1975, then I thought, "OK, I should resign too, and it would be a clean slate for whomever came in. At that point, Toreson was acting chair, and we went through **a** period during which the internists tried to take over the pathology lab at the Medical Center, and there was a terrible dispute. I did have an appointment as vice chair then, but I agreed basically to hold down the fort on this side [on the Davis campus] and I was Toreson's confidant in terms of the running of the department.

JL: And developing the teaching curriculum as well.

RC: That really came under Lundberg. Again, when Lundberg came in as chair, I resigned as vice chair so he could chose his own people. He waited, I think, about a year and he asked me if I would serve as vice chair and I agreed.

The other night George repeated the story about the teaching where he, by an administrative fiat, declared a vote an overwhelming mandate to revise the curriculum. And part of the revision was that we were to revise the medical pathology curriculum. I remember him calling me into the office and asking me what I wanted to teach. I said, "Gee, I really want that first course in General Pathology, because that's my real love." And he said, What you're going to do is....

JL: He had other plans for you?

RC: Well, there were to be basically three pathology courses in the new curriculum: Laboratory Medicine, Systemic Pathology and General Pathology. Because of my love of basic science (General Pathology is really where I lived), I could conceive of doing a wonderful course in the mechanisms of disease. But for whatever his reasons were, George wasn't going to do that, and so he gave the General Pathology course to Sam French to organize. My job was to be Systemic Pathology. I think that part of it was that nobody else would do it [Systemic Pathology] because it was a big, big task.

But at that point, George had hired Joel Lanphear who had **a** Ph.D. in Medical Education. He told me that I could have Joel and we'd work it out together. So I met with Joel and we decided that we would go look at some other curriculums. One of the first places we went was to the University of Nevada, Reno. Merle Haber was up there and had been doing some innovative things. So as we drove over the Sierras, and we started talking. By the time we got to Reno, we knew what we wanted to do. Joel was perfect. Our educational philosophies were very, very similar. I knew what I wanted to accomplish, and Joel knew how to do it.

JL: A good combination.

RC: It was perfect. You know, to try to develop a course in problem solving, to teach students diagnostic skills, is pretty ambitious. To try to figure out how to do it and not overwhelm the students was a tremendous challenge. I had done a couple of things in the past to try to sort of introduce the students to these kinds of skills, with varying degrees of success. But in the designing of this course and working very closely with Joel, he could grade this series of exercises for complexity and sophistication, and when the students had the vocabulary and the content to go to this level, etc., etc.

Then there were two other people that were involved in this course that were really very pivotal. One was Stowell. He understood what we wanted to do, had a lot of experience in curriculum design, and could tell us a lot of things that wouldn't work.

JL: He was here when the medical school was first developed, so he knew what worked and what didn't work.

RC: He'd been chair at Kansas before that and had done innovative things at Kansas. You know, there was a lot of things we'd enthusiastically embrace, and say, "Let's go do that, and let's do this." And he'd sit there and say, "Well, we tried that in Kansas." He was very skilled at telling us why something wouldn't work. A lot of people just say, "Well, that won't work, you know, I've tried that before." Stowell is very thoughtful about it. He could frame the issues, and we'd say, "OK, we have to address that." And we'd work on addressing that.

The other person who was really pivotal was Wellings, with his art skills, really his visual skills that come out of being an artist. He really loved teaching, and so he was sort of the artist that pieced these things together. He would set up a lab. He would go find all the resources and piece it together. He made a pretty picture out of all the different pieces of mosaic. So, the four of us pieced the course together. I think my appreciation for Stowell really came out of that more than any other single thing. That is the closest I have worked with Stowell. Initially, to a young person coming in, he seemed sort of past tense. He had sort of gone through a series of accomplishments, was, in one sense, preparing to retire and was off doing international politics. He wasn't doing hard-core research. But then to work with him...It really took us a year and a half to piece everything together...but to work with him, to gain from his experience and for him to share his experience with us, was really invaluable. The opportunity to work with Wellings once again was great, because there had really been a hiatus in our work together due to all the things happening in his life and the troubles in the administration. There'd been a split, and for him to be able to come back in the last parts of his career, in retrospect, and really put something

together that was so powerful like this, almost magical. It was a very nice feeling. We collaborated on this course and we came out with a wonderful sense of accomplishment as a team.

JL: When they were developing the three courses in pathology, did different people work on different aspects, or did some people cross over and work on all of them?

RC: I think that primarily myself worked on all of them to different degrees, and certainly the first time we got through the big summer course, the Systemic Pathology course, people like Clancy Miller, etc., got the message: This is a fantastic way to teach; this is exactly what our students need. So what developed in Laboratory Medicine, became very much a continuation of what went on in Systemic Pathology. However, we struggled with the General Pathology for a couple of years. I guess I never taught the General Pathology course that I would like to teach, but....

JL: You're still up for another revision?

RC: Well, at this point, everything's in continuity. There are things we've sacrificed, really, because we suffered from our success. They [the administrators of the medical school] have taken more and more time away from us...the number of hours we give in the medical school curriculum has diminished, so we've had to telescope the courses and we've had to sacrifice certain aspects of the course to try to give our students a comprehensive experience.

JL: Dr. Lundberg mentioned the fact that the key to your success was the willingness of the pathology faculty to teach in the hot summers months so that you could have more time with students.

RC: Yes, that's certainly one aspect of it. I think that the students and the faculty get completely involved in the course. This is one place where the faculty acts as a faculty with a common goal, no question. It seems like every year we hear that everybody's really burned out, that this is going to be a very difficult year. Then they get into the summer and it's just exciting. The only reason we're here, and the reason they call us the University of California Medical School is because we teach. Our faculty can do research and they can do service. They can do everything else someplace else. But the unique quality of this institution is we teach medical students. So, if you don't like teaching medical students, you have no business being here.

The way that the course is set up and basically what Joel insisted upon, my major role is not teaching medical students. My major role is preparing faculty. So most of my energy and focus goes into making the faculty the most effective teachers they can possibly be.

JL: You're speaking of your position as chair at this time?

RC: No, as course director. What we basically do all year round in planning the course, modifying the course, getting the resources, updating, and during the course, really, is prepare the faculty. Then the faculty can go into the course very well prepared. They know exactly what they're doing. They have their study plans, they're briefed, and they can go in and concentrate

on being superb faculty. When the faculty can go in prepared, the students love it, they respond. It's a cycle and they feed on each other. So if you want to be in a medical school because you want to teach, then you want to be an effective teacher. We've got to have a curriculum that a.) makes you an effective teacher, and b.) makes students love it. You get all kinds of feed back and it's very intense. The students go through a transformation during the course from being a medical student to being a physician. You can predict that during the first two weeks of the summer that students are really mad, because in addition to their multiple choice skills we ask them to start problem solving.

JL: And these are first or second year students?

RC: They've finished their freshman year and now they're starting their sophomore. So now we've taken them right out of what has made them successful medical students and put them into a training program to become diagnosticians. They've got to learn how to use information, to make a diagnosis, and there is no answer. It is not a, b, c, d or e. But they've got to pull something out of the hat. They have to make a different type of commitment, and a lot of medical students become really disturbed about this. By the end of the course, they understand, and they learn that they can do medicine.

JL: So you maintain this role as director of this course and each year you prepare the faculty.

RC: Yes.

JL: Do they do that for General Pathology and other courses, too?

RC: We have a committee that meets all year round. It analyzes performances and determines what we did right, what we did wrong, and puts in the new resources.

JL: Is this pathology curriculum a model that has been followed in other medical schools?

RC: The answer is yes. Almost every year we have at least one educator from another medical school come in and spend some time with us to see how we're doing. A lot of these people have become part of the network of friends. They bring in their own suggestions and ways of handling education. So the course has really created a very active exchange with other schools. I can sort of go down the line naming various contributors.

Dr. Molly Valdez-dePena at the University of Miami came out. She's developed a wonderful curriculum that has a testing center that we would love to be able to emulate. She came out one time when we were busy trying to develop a film on how to examine microscopic slides. We were struggling over the concept of how to distill all of this information down to some kind of symbolic thing, basically for a thirty-minute presentation. She was here while we did some of the filming and she said, "You know, I've always used something I call my magic circle as a way to explain this." And I said. "Geez, tell me about it." What she told us about was just absolute perfect. It was a distillation of a whole conceptual framework, and so we've created this film that deals with Molly's Magic Circle. That's a film that is shown to our students every year and is marketed nationally.

Suzanne Stetsass who was at Utah and is now at Cornell developed The Slice of Life [a laser disk collection of pathology images] that we use extensively. She came out early in her career, and we've always been involved in exchanges with Suzanne, creating new ideas, etc. Anyway this has all been very enriching for us and hopefully for other people.

It's all been an adventure. Probably one of the more satisfying parts of my career, was to see that the course can grow and develop.

JL: How does the current pathology curriculum differ from how pathology was taught during the early years of the medical school? I understand that there wasn't a specific pathology course then.

RC: No, the curriculum was originally patterned after the Case Western organ system curriculum, so pathologists were invited to give lectures here and there. In fact, that was a major problem. I can remember during one of the first lectures I gave, I saw that I was drawing complete blanks from the students. So I stopped and said, "What's wrong here?" One student asked me a question and I said, "Well, wait a minute. You have had no exposure to the general principles of inflammation? "No," was the reply. "Then why am I giving this lecture," I asked. Well, the basic principles were not being taught. You'd get into an organ system and teach advanced pathology that went along with the organ system, but the students had no vocabulary, they didn't know the basic concepts.

JL: Dr. Lundberg indicated that in order to revise the pathology curriculum, it was necessary to revise the entire medical school's curriculum, and it was the Department of Pathology that got the ball rolling. In 1977, there was no way to revise pathology courses in-house, so to speak; it had to be done throughout the entire faculty.

RC: Well, I think that we were long overdue for another curriculum revision. I think that every ten years you ought to revise the curriculum top to bottom.

JL: Is there an interest in doing that?

RC: No, I don't see any current interest in doing it. But the reasons for doing it is that things start becoming routine, you slide into bad habits, and you lose the enthusiasm and the challenge of doing something new. If you don't make revisions, the creative part of teaching sort of gets buried, and I think that what sort of invigorates people is simply turning it upside down. Sort of like an hour glass, ten years time's up, turn it over and let's start over again.

JL: There are lots of new approaches to medicine that were not even available in earlier years.

RC: Absolutely. For example, right now we're trying to figure out how to integrate computer resources into our curriculum. In so much as the faculty has no experience with this, we don't know how to do it. And yet I'm sure if you really turned the curriculum upside down and were forced to do something novel, you would invent a way of using this stuff.

JL: During your career here at Davis, you did take a sabbatical?

RC: I've taken two sabbaticals. On one sabbatical I went down to USC and I worked with Bob Lukes for a couple of months in 1980. This was a time shortly after we'd gone through this period where pathology department was being assaulted. One of the assault points was that the pathologists were not introducing the new technologies in leukemias and lymphomas. Fred Toreson had asked me to take over the lymph nodes, as it were, because we were making a lot of mistakes in lymph nodes. Part of the problem was that we needed a pathologist that knew something about immunology, and I was the only one available who knew anything about immunology. One of our primary critics was Malcolm McKenzie. Do you know Malcolm?

JL: In internal medicine?

RC: Yes, right. You know he is a real character. He can be vociferous in his criticisms, and he is not only vociferous but he is correct. So for whatever reason, I guess I never took Malcolm too seriously. I thought he was very funny, and I didn't take umbrage at his statements. You know, his comments were always sufficiently theatrical and hyperbolic, but they had an element of truth in them. And so Malcolm and I started working together on lymph nodes. Actually, Clancy Miller was then in his lab working on immunophenotype marker systems, and so that's how she got started and eventually ended up in our department. Fundamentally, somebody in pathology had to be enlisted to work with these people. I felt that Malcolm had a lot to offer, so I accepted and we started working together. As a part of that, I thought I should go down and spend some time with Bob Lukes who was the grand guru of lymph node pathology at that point, so I did that. I split my time between the gurus [at USC], and then went to the Jackson Lab in Bar Harbor, Maine to satisfy the experimental part of myself by working with mice for a couple of months.

That was a wonderful sabbatical for the family. We had three young children then. Our daughter Shelley Lynn Cardiff was born in 1970 and we were in Pasadena during the first year of the big busing experiment. The kids went from an all white environment in Davis to Pasadena where they were the fourth minority. I can remember interesting differences in the culture. At Davis, my eldest son was playing clarinet, but he couldn't make the junior high 9th grade band because he wouldn't practice enough. We went to Pasadena and he came home and he said, "Guess what?' I'm in the honors band and we're going to play a big concert." And they did. A little different criteria than there was in the Davis schools. Then we went to Mount Desert Island, which is at the end of the highway in Maine. I don't know if you have ever been there.

JL: Not to the island, no.

RC: It is the most enduringly beautiful place I've ever been in. Every turn in the road has a different beauty. All of a sudden then we went from a place where kids in junior high school were slam dunking to this place at the end of the road, where they said, "Wow, they're from California." I can remember my middle one, Todd who was an all right athlete but rather small for his age, ended up on the all-star Little League Team, the sort of thing that he never could do

in Davis. You know, all of those kinds of things that come with a small community. So it was a terrific family sabbatical.

While in Maine, I worked in the laboratory of Dr. Richmond Prehn, an eminent tumor immunologist and then Director of the Jackson Memorial Laboratory. I spent a great deal of my time in the library reading and preparing myself for the next set of grant applications. I made friends with a wonderful Pathologist-naturalist, Dale Coman who had retired to the Island to fish, study and watch nature but he spent some time at the lab. It was a refreshing experience for me and I did come out of it with a new set of research grants but no research papers.

My other sabbatical was right before I became chair in 1990. I was a Visiting Professor of Genetics, in the Department of Genetics, at Harvard Medical School. That was a wonderful sabbatical, but that was cut short because I came back to be department chair. One of the reasons for doing the sabbatical was I had time coming to do the sabbatical, but the other thing was they were jerking me around sufficiently and not making decisions [at Davis]. I finally said, "Well, if you want me to become chair, I'll come back. But you have to sign this piece of paper. Otherwise, I won't come back."

JL: As we've mentioned, you had served as acting chair of the department for a couple of brief times before.

RC: When Lundberg went on sabbatical and, again in 1982 when the AMA offered him a job, I was acting chair there. That first time wasn't very much of anything. But when George left and we were in the process of choosing a chair, then I was acting chair. I was clearly not interested in being chair at that point.

JL: But you succeeded Murray Gardner as chair in 1990 when he stepped down. What was your thinking then, that you were ready to take on this responsibility after you had already had a taste of it on a few occasions?

RC: The only reason that I became a candidate was that I felt that somebody had to articulate the faculty needs, and I was concerned that if all of the candidates came from the outside that they would come in with their own agenda, and the needs of the department would never be addressed. Now how I ended up being the only candidate is a bit of a mystery to me, because I took some really strong positions....

JL: What did you see as the critical needs?

RC: Well, unfortunately, the same things we need now—space, money, informatics and people. I can recall a very interesting phenomenon, because the first interview that I was supposed to have with Frank Loge, hospital director at the Sacramento Medical Center, he stood me up. What was interesting to me about that was after I had been told that he was not available, that he was out of town, I happened to look out of the office window and watch Frank Loge walk by. I thought, "OK." Later, he did interview me in what was my office down here in Davis. That was a very interesting interview. He told me that he opposed my candidacy, that I was known to be an incompetent administrator, and that he didn't see very much usefulness in

continuing the process. I said. "OK, that's fine, but this interview is scheduled for thirty minutes and I want to tell you what the faculty needs, what this department needs, so you're going to sit there." I made him sit there, and I told him what the agenda was.

Realistically, I think John Keltner from Ophthalmology probably did a lot of lobbying with Frank to make him accept me, because Frank made it very clear that he wanted nothing to do with me, and there was no question of significant opposition to my candidacy internally, both at the technical staff level and faculty level. So I'm sure that he was getting a lot of that information. But again, it was sort of flattering to think that he felt threatened by me, and the important point to me was that what I was asking for were the things that the department needs.

JL: What specifically did you outline for him?

RC: First of all the chair could not be lateralized. Under the way that he [Frank] had wanted, the departmental structure kept the chair lateral to Dick Lowe (the laboratory manager), so the reporting relationships and control elements came through hospital employees. As far as I was concerned, that was a non-position. We also needed the electronic services, the computers, upgraded, and, at that point it was quite clear to me that if the department was ever going to survive, we were going to have to have a reference lab. (Now Murray had been working on a reference lab, but we had to throw major capital at the lab.) So that was an important agenda item. Also, we needed adequate space. We did get the new clinical lab building, Research II, but at this point we still don't have a home at the Medical Center. So we never really got what we asked for, or what was promised. We only got a temporary building, and it creates a divided facility, a divided department which is not very cost effective. Going into an era, as we are, where labs are going to be judged not so much on quality but in terms of cost savings, to have something that is a half a mile away from the people that you serve is not at all cost effective. The original design for the building was wonderful. We went in and designed it the way a clinical lab should be. Then they (the architects) said, "No, you can't do it that way because it is a faculty research building," and they redesigned it. So all of the efficiencies that we potentially built in no longer exist.

JL: What was the reason?

RC: The reason given was that it is a temporary building as far as the lab is concerned. Like a lot of these things, they become permanent because they don't have a place to move a clinical lab. I'm sure that the day I retire we will still be there, and there will be no solution for pathology.

JL: Did you have a conversation with the Dean of the medical school when you took over as chair about what your views were, and what his views were?

RC: Oh, yes. I went in to see Dean Hibbard Williams with a detailed agenda. When we agreed originally, I put together about a twelve page memorandum of understand detailing the agreements. Frank [Loge] refused to sign it, and so I said, "Fine, I'm going to Boston on sabbatical, I'll see you later." Early in December, he agreed to sign a short form of it, and so before I left I met with the associate directors at the Medical Center and what I found out was

that they thought they were negotiating with me, and so all these things that had been in our original agreement, they were not privy to, and they were telling me that this couldn't happen and that couldn't happen. So I simply said to Frank and the Dean, "Look, either sign the twelve page agreement, or I'm not coming back, period." It took another four months for Frank to sign it, sort of reluctantly. But what I did, at that point, I simply sent the agreement to all the associate directors and said this is what has been agreed upon in detail, and so don't give me any heat, this has already been agreed upon, these are not negotiable.

JL: Was this agreement between you and the dean of the medical school?

RC: It was a three-way agreement between Loge, Williams and myself. I had to have it in detail because exactly what I saw in late December came true. Frank would agree on one thing, but he would never tell anybody else what the agreements were. So if I had something that was signed, sealed and delivered, I didn't have to go negotiate with Harry. I said, "Look, it's right here, and so you pay up." So to the extent we have been successful, it was based on having in place a document where you know I wasn't questioned going around and reinventing the wheel, but this is agreed, now we'll go forward. So it really took a long time to agree upon the details. I think if I went back and looked at all the different versions of that thing, it probably took a full year to negotiate and get them to sign that agreement. The irony of it is that, in fact, the major agenda of the pathology department today is on that agreement, in terms of space, finances, computers and people.

JL: I wanted to ask you if there have been major changes in the department, and if you see changes in the future. You're indicated that some of those agreements have not come to fruition yet. Is there a possibility that they will, or is the current budget situation going to be dictating the future.

RC: Well, there are two major things that I think have happened, and the first is certainly the dramatic changes in medical economics. Fortunately, the things that were in the agreement and the direction that Murray had initiated, were the correct directions. So, without a doubt, the future of the department is tied up in the reference lab. David Harry and Murray Gardner initiated that. At this point, you'd always like to be further ahead. I think everybody's bought into the concept and we've got the appropriate people in place, and we're moving. Whether we're moving fast enough, I don't know. But going from a situation that I entered in 1971, with Sefton Wellings as chair, where ninety percent of our revenue came from the state, to the current situation in which the Department of Pathology gets less than five percent of its revenue from the state, is a drastic change in economics. And so, an inordinate amount of my time has been devoted in trying to position my department into modern economics and into a position where it can take care of itself financially, and I think we have been successful in doing that. A large part of that is because of a dowry that I got both from the Dean and from Frank. So in terms of finances, there is no question that in fact they did come through. We are almost there. Hopefully, the money that they invested in the department will come out the other end in a financially sound department. At the present time we are able to pay our salaries and we've got a reserve fund of over a million dollars, so I think we're OK. It sort of depends on how the medical center develops itself economically.

The other issue is the people issue. This is an elderly department and the future generation, future leadership, is not here. That is probably the real down side and the most difficult thing to deal with. We do not have, in our department, a future chair of the Department of Pathology.

JL: That means that they'll have to have another nationwide search.

RC: Well, as I leave there will have to be an open search, but more importantly, we need to replace the Murray Gardners, Boris Ruebners, the Ray Teplitzs, the George Lundbergs of the department with another generation. At this point we have some very fine service pathologists, but we do not have the kind of intellectual leadership that a Murray Gardner or George Lundberg provided us. So whether it becomes my responsibility to recruit that generation of the next chair to bring that generation in, I don't know, but probably the change that has occurred in the five years is that we've lost the people who have really made this a well known department. Of course, people like Murray and Boris are still around. We haven't lost them in that sense, but still they are not providing the same kind of leadership that they have provided in the past.

JL: Yes, and that's part of the economic situation, with people retiring at earlier ages now.

RC: And so we need to really be very careful about developing our resources so that we can bring in the future. I would have to consider that as a major agenda for the next couple of years. Bring those people in and develop the next generation of leaders.

JL: Everyone seems to be hoping you'll stay on for a long time because you have done a good job of balancing the research aspects as well as administering the department. That is not an easy job to do, I'm sure.

RC: That's flattering. I'm in a decision mode at this point. It's going to take another five years to develop a junior faculty. While we do have a good reserve, if we take major economic blows, we can very rapidly lose that reserve. So it's a little difficult for me to see what is going to happen in the next five years. It may be more appropriate at this point to bring somebody else in who simply because they come in as a new chair will gain advantage for a new department. The real advantage of a new chair is that the dean and the director have to negotiate.

JL: That seems to have been a key.

RC: Yes, that's right. And so, one of the reasons for changing chairs is simply that, in order to bring in a person into the chair. If they have any value, the university has invested, and so that is a gain for the department. It's probably time for the university to reinvest in the department, and that they don't do it for people that are in place because they are cheap and they're here. So it's probably wiser in the near future for me to step down just so that we can gain those kinds of advantages.

JL: Considering all the grants and contracts you have personally received over the years, I was wondering if you could note some of the projects you thought were the most significant as far as the research you've accomplished.

RC: You know it's funny. I'm sorry. I love history, but I'm a futurist, and so in order to be a futurist, one is never content with the present. I find it difficult to look back and say that was really something good, or that was a major contribution, or something like that, because there's always something that is much more fascinating right around the corner. The projects that are coming or the next challenge is much more exciting than anything that we've done before, so that is a difficult question.

JL: You're always looking ahead. Do you have research projects now that are stimulating?

RC: Oh, yes, absolutely. No question. Murray Gardner and I are kindred souls. He is much more of a wordsmith and an enthusiast than I am, but we're futurists, you know. There's always something really interesting right around the corner, so yes, that's happening.

You know in retrospect, there are probably some milestones that are interesting. Probably initially we were the first ones that developed a radioimmunoassay for the mouse mammary tumor virus, which really was effective for the scientific study of the virus, but it also was the first tool that you could use to disprove the hypotheses that there was a related human breast cancer virus. And so the assays that we developed were really pretty effective. They debunked a lot of the poor immunology of the era and sort of laid to rest the idea that there was a human cancer virus. I think that we made major contributions to laying the foundations of the structure and function of the virus. Those were really basically done in the '70s.

In the '80s, there were two things that happened that were basically worked out. First, the molecular biology and the emergence of clones in tumor biology. This is an important concept and establishes that the precancers are really clonal proliferations, which means that they are already genetically altered. They are already neoplastic. They are already tumors. They are just not quite far along in their evolution to cancer. We did that with some really excellent molecular biology. At the same time, we got off on a tangent that turned out to be very interesting. This research was in the area of keratins. It initiated our major contact with industry and biotechnology. We ended up with a patent that still brings in royalties. It was interesting because it was really a very practical application of science brought to fruition.

JL: This involved genetic research?

RC: Well, it really wasn't genetics, it was immunology. We developed an assay while we were searching for an immunological approach to breast cancer. It turned out that we made a very novel discovery that nobody else had recognized. So the good news is that we found out what the protein we were finding in the circulation in the urine of patients with cancer was. The bad news is that it was keratin. The reason that that's bad news is because everybody knew that keratin was an insoluble substance that's inside cells and here it was floating around in the circulation. It took an awful long time to get this finding published because all of the people that knew anything about keratin knew that it couldn't possibly be soluble.

JL: There was a mind set there.

RC: There is a mind set there, yes. It was a very interesting experience in scientific politics. We proved that we were right, and now we have a patent. Ciba-Corning markets our antibody, and it is such a good idea that people are now challenging the patent, claiming that they found it first. So you know, all of that was a very interesting experience which is entirely outside the usual federal grant mechanism.

Recently, really out of the work I started during my sabbatical with Phil Leder (in the Department of Genetics, Harvard Medical School), evolved into my current work on transgenics. That is very fascinating. Because of my unique role as probably the only pathologist in the field of transgenic mammary biology. I've found a unique niche and everybody is sending me all these fascinating transgenic materials for pathological analysis....

JL: It sounds like you have your work cut out for you.

RC: Well, I've got Larry Young and Peter Barry and Tony Cheung and Dave Morris all doing projects with me and really doing the hard core science. The one thing that I'm doing that I do myself, personally, nights and weekends, is the pathology on the transgenic animals. That's turned out to be a very fascinating thing. There are all kinds of spin-offs from it that probably I'll enjoy over the next five years.

JL: Have you been involved at all in the AIDS research that's taken place on the Davis campus?

RC: Yes and no. That's another interesting experience because when AIDS first emerged, Willie Brown, Speaker of the State Assembly, with Mike Gottlieb's urging, set up a state fund for AIDS research. The University of California was asked to administer the research funds. So the University developed a universitywide AIDS Task Force. The money went to the Task Force for distribution. So I, as a retrovirologist and a pathologist, agreed to be the Davis campus representative on that Task Force. Part of what we agreed to if we were going to be on that Task Force, was that we would not personally do AIDS research or apply for money from the Task Force.

JL: To remove any conflict of interest?

RC: That's right. So that left the door open, and on one level it was fascinating to go through that era...

JL: When was this Task Force active?

RC: What was it, '83 to '88? And so just by going through the whole development of that, being involved because I was close to the legislature, I frequently was the AIDS Task Force representative at hearings, and watched science being politicized. Merle Sande was the chair of the Task Force. There was a real debate early on about how to use the money, but we decided to do it programmatically, which in retrospect was the right thing to do. We made a lot of the right decisions. Consequently, when the federal government decided to start funding AIDS research, California was in an excellent position to compete for the funds, and did so very, very

successfully. It was primarily because of my position on the Task Force that I was able to get information to Murray Gardner, and Jim Carlson, had some funds. I know quite a few of our faculty were funded by the Task Force and did very well. And part of it was that I had early information. I could come back from the meetings and tell them what was going to be next and they could respond to it. In fact one thing we did at the very beginning was that we had in our faculty of the future, when Murray first came into the chair in 1982, we sent them all [faculty]up to Lake Tahoe for a week to write grants. They all went up, they all wrote grants, came back, we reviewed and critiqued the grants, and they all were sent in. I think all but one were successful, and that then became the foundation for our junior faculty to establish academic careers. And of that group, Jim Carlson has been the most successful. Steve Hindrichs started with a nice grant. I think he ended up taking that to Nebraska with him, on down the line. So it is just a prime opportunity, and being on the Task Force and knowing what the information was, really placed the department in a good position. I held the door open and they walked through, so to speak. Anyway, as a result, by the time I was finished with the Task Force, I was so far out of AIDS research that there really wasn't very much left....

JL: It sounds like you found plenty of other things to occupy your time.

There was a lot of discussion at the Pathology Forum about cooperative research between the veterinary school, pathology and some of the other departments within the medical school? Do you see that as a trend for the future, or is that going to be difficult to accomplish?

RC: The future School of Medicine will have academic departments but the research will be done in organized research units. To an increasing degree, things like the Neurosciences Institute across the freeway will become the paradigm. We'll put up the Center for Comparative Medicine, which is really a monument to Murray Gardner, and it will be an interdisciplinary group, part of our pathology faculty will go to that. On the Sacramento campus, Research III will become a cancer research lab. Part of our faculty will go to Sacramento. We must now consider how the pathology faculty will be distributed. I think dispersion is going to become the paradigm. I think that ten years from now the most recognizable thing in pathology will be it's service, unless there's really strong leadership that draws a number of these organized research units under pathology. Otherwise, pathology becomes a division, and pathologists in the group that are currently working in this building [MS1A] under the Department of Pathology will be under the Division of Basic Sciences.

JL: So there will be some changes during the next twenty-five years.

RC: I think we will see changes in the next three years. And as the traditional FTE [Full Time Equivalent], a title indicating that the UC Regents are responsible for your entire salary, goes away, the FTE will become an endowed chair, and young people will come in, not guaranteed FTE's, but in some other kind of capacity. It is a good chance that in the future an FTE will be just like an endowed chair.

JL: It'll be interesting.

Well, in closing, I think you are to be commended for pulling all these former department

chairs together for the 25th anniversary and development of a history of the Department of Pathology. You're making a contribution to the future.

RC: Let's see how this comes out. No, I mean it has to be done, and it has to be done soon. Thank you very much.

JL: Thank you.