Dr. Ralph E. Knutti

An Oral History

Director, National Heart Institute
National Institutes of Health
Bethesda, Maryland

Interview with DR. WYNDHAM D. MILES

Historian, National Institutes of Health

13 July 1972

Transcribed 1 December 1988

History of Medicine Division
National Library of Medicine
Bethesda, Maryland

Dr. Ralph E. Knutti

Dr. Ralph E. Knutti was born on 24 July 1901 in Palo Alto, California but received his formal education in West Virginia. He received an A.B. degree at West Virginia University where, in addition to other extracurricular activities, he was a consistent high scorer on the track team and a saxophone and clarinet player in a student dance band. After graduation he spent one year as a high school teacher and track coach. He then returned to West Virginia University where he was awarded a B.S. (Med.) degree in 1926. He received his M.D. degree at Yale University Medical School in 1928.

Following a year as Assistant in Pathology at Vanderbilt University, Dr. Knutti served his internship in surgery at Lakeside Hospital in Cleveland, Ohio. In 1930 he began his career in pathology and research teaching with an appointment in pathology and bacteriology at the Rockefeller Institute for Medical Research in New York City.

From 1932 to 1942 he was instructor, then assistant professor in the pathology department of the University of Rochester. Also at that time he was Director of Laboratories at the Genesee and Park Avenue Hospitals in Rochester.

He became associated with the pathology department at the University of Southern California Medical School in 1942 and served as associate professor there from 1948 to 1951. During those years he was also a member of the Board of Directors of the Los Angeles Child Guidance Clinic and Director of Laboratories at the Los Angeles Children's Hospital.

In 1951 Dr. Knutti joined the Commissioned Corps of the U. S. Public Health Service, becoming first Chief of the Extramural Programs, and later Associate Director for Extramural Programs, of the National Institute of Arthritis and Metabolic Diseases. In September of 1961 he was named Director of the National Heart Institute.

Dr. Knutti has been an active member of the Board of Directors of the American Heart Association, the Editorial Board of *Circulation*, and the International Society of Cardiology Foundation.

CURRICULUM VITA

Name: Ralph E. Knutti

Date and Place of Birth: July 24, 1901, Palo Alto, California

Education:
1923 A.B. West Virginia University
1926 B.S. West Virginia University
1928 M.D. Yale University

Professional Positions Held:

1928-29—Assistant in Pathology, Vanderbilt Univ., Nashville, Tenn.

1929-30—Intern (Surgery), Lakeside Hospital, Cleveland, Ohio

1930-32—Assistant in Pathology and Bacteriology, Rockefeller Institute for Medical Research, New York, N. Y.

1932-35—Instructor, Pathology, Univ. of Rochester

1935-42—Assistant Professor, Pathology, Univ. of Rochester

1939-40—Director of Laboratories, Park Avenue Hospital, Rochester, N. Y.

1941-42—Director of Laboratories, The Genesee Hospital, Rochester, N. Y.

1942-48—Assistant Professor, Pathology, Univ. of Southern California

1948-51—Associate Professor, Pathology, Univ. of Southern California

1942-51—Director of Laboratories, Los Angeles Children's Hospital

1949-51—Board of Directors, Los Angeles Child Guidance Clinic

1951—Medical Director, U. S. Public Health Service

1951-60—Chief, Extramural Programs, National Institute of Arthritis and Metabolic Diseases, National Institutes of Health

1960-61—Associate Director for Extramural Programs, National Institute of Arthritis and Metabolic Diseases, NIH

1961—Director, National Heart Institute

January 1965

Dr. Ralph E. Knutti
Director, National Heart institute

Date: July 13, 1972
Interviewer: Dr. Wyndham D. Miles

Q: Dr. Knutti, would you start off by telling me when and where you were born and anything else that you would like to about your childhood days?

Dr. Knutti: Yes. I was born in Palo Alto, California, on July 24, 1901. My father was completing his graduate work at Stanford at the time, and although I'm listed as a native Californian by birth certificate, actually, I only lived the first nine or 11 months of my life there, until my later career, which had no connection with the fact that I was born in California.

I might say a word about my family. My father was born in Switzerland and was brought by his family to this country when he was eight years old. They settled in the woods of West Virginia. Of four brothers, he was the only one who was able to go to college and do graduate work. He ultimately became president of Shepherd College in Shepherdstown, West Virginia, where I spent the first eight years of my life.

Sometime in 1902, then, we moved back to Shepherdstown, and in the summer of 1909, the family went to Morgantown for some summer courses in West Virginia University, and at that time my father got typhoid fever and died, which left my mother with two sons, ages eight and three, respectively.

I went to the Morgantown public school, but up until about the fifth grade, for reasons which have never been clear to me, I did not attend public schools in Shepherdstown. They had a demonstration school connected with the college, which had very few youngsters. In Morgantown, perhaps because of some obscure illness that I'm not clear about, I had a tutor until it was time for the fifth grade. Then I went through grade school, went to Morgantown High School three years. The fourth year of high school, I spent in Mannington, West Virginia, high school. This was during World War I. Because of financial reasons, as well as my mother's desire to be productive, she kept a job teaching in Mannington, West Virginia, so I had my last year of high school there. I happened to be president of the senior class that year.

Back to my early schooling, I don't remember any particular events in grade school that would lead one to suspect that I would be ultimately interested in studying medicine. In fact, I think I went through the usual childhood reactions of being everything from a railroad engineer to possibly an astronomer. The science courses in high school were quite primitive in terms of expert instruction and in terms of equipment for experiments. I must say that in the high school physics and chemistry that I took, there was not much that was inspiring about it. In fact, I found subjects like English literature and mathematics more interesting than I did the chemistry and physics which I took.

Along somewhere in the middle of my high school career, I met a Dr. Aaron Arkin, who was professor of bacteriology at West Virginia University Medical School, which at that time was simply a two-year medical school, and everybody transferred out. Living in a college town which was quite small, we had the opportunity to meet socially numbers of the faculty members. I remember Dr. Arkin telling about some of his work, and this sounded interesting to me. I asked him if I could come down and see his laboratory. He arranged this, and I was quite thrilled to have a drop of my blood taken and put under the microscope and see the red cells floating around, and later to see a stained smear which showed the red cells and white cells. I think that this probably was the event or the seed which was sown which stimulated my interest in ultimately going into medicine.

We were quite fortunate in those days to come in contact with individuals who were on the faculty of the university, and among other family friends were Dr. Albert Reese, who was professor of zoology, as they called it then. It actually was a little broader than zoology. A Dr. Spangler, who was head of the botany department, and particularly Dr. Chauncey Wagner, who was professor of physics at the university. There were also certain members of the faculty who had been classmates of my father at West Virginia, and they took an interest in our family and a particular interest in me. Dr. Wagner organized some of the high school boys who were interested in wireless. In those days we had no radio, but we did have wireless telegraphy. Evenings, we would go to one of the physics labs and listen in to ships at sea and the NAA wireless station in Arlington, Virginia. This developed into the fact where I built my own wireless set. There were three of us in Morgantown who had wireless sets with which we could talk to each other, but which we could hear for great distances. This was just before World War I, at the age of about 15 to 16.

So with the beginning interest in the microscope, then further interests in scientific events, by the time I got to college, I had decided to take a pre-med course, although I still was somewhat undecided as to what I would do in the future. I'm sure that the family—namely, my mother—was interested in my following my father's footsteps and going into the education field. On the other hand, she never overtly influenced any decisions that I wanted to make about my own career, and was quite helpful in supporting, as well as she could, the things that I wanted to do.

I entered college in 1919 and took the usual freshmen courses, but I had decided at that time, as I have mentioned, to study pre-med and to major in zoology. I think this was partly because I liked Dr. Reese, and partly because it seemed like an interesting subject.

In college, it is interesting to note that I flunked out my second semester of my freshman year because of extramural interests and not attending to my studies. I think this was one of the best things that ever happened to me, because I was faced with a moment of either getting down to work or not being able to do what I'd projected for myself. From that time on, I did apply myself to my books, and although I did not graduate with honors, I certainly graduated with sufficient credits and high enough grades to go to medical school.

However, during my senior year in college, it became quite apparent that I wasn't going to be able to afford to go to medical school. My mother had been left a very small life insurance policy when my father died. She had managed to keep things together by taking roomers in our home, but there came a time when it was necessary for her to go to work to support the family. Fortunately, I got interested in music when I was in high school, and learned how to play the clarinet and the saxophone. Sometime during my first year in college, I got into a small dance orchestra. This is one thing that actually made it possible, not only for me to stay in college, but made it possible ultimately to help out in going to medical school. I played in a dance orchestra every summer except one from the time I was a sophomore in college until between my junior and senior years in medical school. The sums of money we made then were pretty good for those days. I remember the last two summers we played on the road, as they called it, we'd make as much as $100 a week a piece occasionally. This was very helpful for the next school year.

Q: What did you call this band?

Dr. Knutti: There were various bands. The first one was *Jimmy Hayes and His Society Syncopators*. Jimmy Hayes ultimately went to medical school, joined the Navy Medical Corps, was stationed in Guam for a considerable period of time. He retired from the Navy about seven or eight or nine years ago. He became Medical Director of this company that makes girdles, Playtex. I read in the alumni bulletin just recently that he died about six or seven months ago. He was a drummer. Another band was Al Mabey and his *Old Gold and Blue Orchestra,* and I guess this was the best and the biggest band that we played in. We had 12 pieces. I remember my mother's forbearance. At times when we had no place to practice, we'd go to my home and start out about 8:00 o'clock in the evening and go on until midnight. I don't know why the neighbors didn't care, but she was able to take it, and the neighbors were, I'm sure, sympathetic.

At one time, as a matter of fact, I considered very seriously the possibility of going into music as a career, rather than the medicine, which I had wanted to do simply because of financial reasons.

I might say that I guess I was good enough in college to go on in biology, because Dr. Reese suggested that I go to Woods Hole one summer to work on a little research project. This was in about 1921 or 1922. I just couldn't do it because I couldn't afford it. I had to play in the dance orchestra that summer, so I didn't get what would have been a very wonderful background and experience in that type of research activity.

At any rate, it became apparent that I wasn't going to have enough money to go to medical school. Having been on the track team in college and having had quite a successful career as a hurdler, I was invited to coach track and teach some subjects in a Grafton, West Virginia, high school. So between 1923 and 1924, at the age of 22, I joined the faculty of the Grafton, West Virginia High School. I coached track, assisted coaching football and basketball, and I taught physics. I also taught general science and second-year high school mathematics, which I had not even taken myself at that point. I had to keep a day or two ahead of the students, and I found it so fascinating that one summer when the orchestra didn't have any jobs for about six weeks, I went to summer school at the university and took trig, and enjoyed that very much. At any rate, I think that this year of teaching school did two things: it gave me a little money, and it settled for all time in my own mind that I did not want to teach at least that kind of school, and that I did want to study medicine, with the ultimate aim, of course, of going into practice.

The cheapest place for me to go to medical school, at least for the first two years, was West Virginia, because this was my home town. At that time, there was no tuition for students who lived in the state, and there was a minimal laboratory fee of about $12 a semester, and that's about all it cost. So I was able to take my first two years of medicine there, play in the dance orchestra during that period, and save a little money, looking forward to going somewhere for my last two years of medicine.

I did quite well in medical school. I think I was one of the top two or three people in the class. This was easy to figure because they gave you a numbered grade and they posted on a bulletin board averages every semester. I was not at all impressed, as I look back on it, and I don't think I was even then, with the system of teaching they used there, because obviously they were trying to get all their boys transferred into reasonable or reasonably good medical schools, and many of the courses really pitched toward teaching one how to pass examinations, rather than teaching one how to think. As I look back on this and compare it with my ultimate experience at Yale, I learned a lot of facts at West Virginia, but I really think that if I had continued in that type of environment, I would have been a very poor practitioner of medicine, and that would have been the only opportunity that I would have had in that type of environment. In other words, the medical school was not very good, but it was the only thing that was available. Fortunately, it has improved by leaps and bounds since, and is now, as you probably know, a very good four-year medical school, improving as time goes on.

The time came to make a decision to go somewhere else, and I was advised to apply at Yale. At that time, Yale Medical School was just undergoing reorganization, and Dr. Winternitz and a group from Hopkins had been brought there to reorganize Yale with the help of the Rockefeller Foundation and other funds that were available to them. So I arrived in New Haven in the fall of 1926. To me it's interesting to note that this was the farthest I'd ever been away from home, if one discounts the trip from California to West Virginia when I was an infant. I'd never been to the East Coast before. I'd never been to New York City, and I got quite a thrill in changing trains in New York, from the Pennsylvania station to Grand Central. This was also my first ride in a subway.

I arrived at New Haven and I had the usual interview with Dr. Winternitz. I remember the impression that this man made on me at that time. Winternitz was one of those individuals that the student either loved or hated. His technique was to try to needle the boys into thinking for themselves, and do this in somewhat less than a polite manner. I waited in line and heard his technique from some of the others before it came by turn. When I got there, he, first of all, didn't know how to pronounce my name. Then he said, "Oh, you're from

West Virginia." I said, "Yes, sir." He said, "Well, you're going to have to do some work around here. The last guy that came here from West Virginia flunked out. We wouldn't want to have you do that. Did you ever see an autopsy?" At that point, I had passed the course in pathology that they gave at West Virginia, and this was a rather unique course in that it was completely theoretical except for the microscopic part of it, and I'd never seen an autopsy, although I had on the record, by the fact that I had passed pathology. I suppose this is probably one reason, too, why I ultimately went into pathology, because I realized that I knew so little about it. Winternitz had said, "You'd better make contact with Gene Woodruff and Doug Sprunt, and stand in on as many autopsies as you can, because this is a terrific lack. If I'd realized this, I don't think we would even have admitted you to Yale Medical School."

So I did make contact with Woodruff, who at that time was the resident in pathology at New Haven Hospital, and with Doug Sprunt. Both of these individuals became old and long-time friends. Sprunt is now professor of pathology at the University of Tennessee, and Woodruff is chief pathologist at a TB sanatorium in Michigan. I can't recall the name of it at this point. I found the atmosphere at New Haven quite different from that at Morgantown. One of the things, for example, that medical students were required to do was to submit a thesis before graduation on a piece of original work. Having come there at the end of my second year in medicine, I hadn't had the opportunity to either get interested in a problem or to get started on a thesis. Many of the boys had started these theses when they were in their second year, and wound them up before graduation.

It took me a little while to get on the beam, so to speak, to try to find out with which department and what subject I would like to do a little work. I finally settled on a subject, studying the leukocyte picture in erysipelas. Erysipelas, at that time, before the advent of the sulfa drugs and antibiotics, was ripe in the northeast, and the infectious disease wards in the New Haven Hospital always had patients with erysipelas in them. The isolation ward in itself was interesting, because the rigid techniques that they used then were quite different from some of the techniques that are used today, simply because of the fact that we don't see streptococcal conditions and other infections so frequently anymore because of the fact that we do have the antibiotics and sulfa drugs.

The approach to this little simple study, the experience in going through the literature before I started work and educating myself on everything I could find out about the subject were the best training I had had to date and stimulating my interest in another approach to medicine, other than the practice, and I think at this point the first seeds were sown that ultimately took me away from my original goal, namely, to practice medicine.

At that time in our medical school, quite differently from the situation that exists today, the activities of the department usually revolved around the interests of the head of the department. Dr. Francis Blake was Chairman of the Department of Medicine at Yale Medical School. Dr. Blake was famous for his work on pneumococcus pneumonia. The whole department really revolved around pneumonia and infectious diseases, although at that time there was a young instructor by the name of Jack Peters, who was getting interested in the chemical and metabolic approach to medicine. Dr. Peters, ultimately, of course, became a professor of medicine at Yale. He collaborated with Dr. Donald Van Slyke of the Rockefeller Institute, and wrote the monumental textbook, *Quantitative Clinical Chemistry*. I'm not sure that that's the correct name of it. I think it's called *Quantitative Clinical Chemistry* by Van Slyke and Peters. Dr. Peters, of course, ultimately, with Dr. Van Slyke, I think deserves a great deal of credit for being a pioneer who brought chemistry into medicine and paved the way for many of our modern approaches to clinical medicine and to clinical chemistry.

It was interesting, years later, when I came to NIH, to find Dr. Peters a member of the Metabolism and Nutrition Study Section. Then as we all know, he ultimately went through the traumatic experience of clashing with the McCarthy committee, being fired from the study section because of some cloud over his name, ultimately going to the Supreme Court and having his name cleared. This was one of the most unfortunate events that took place in my career at NIH, to see a man like Peters and others being fired or put on black lists with no possible ability to confront their accusers, and with no real knowledge of anyone, without trial, to the fact that they might have had associations with organizations that ultimately turned out to be subversive, or they considered subversive in nature.

If I can digress for just a moment, I can say that there were a number of us who felt that as grant administrators at NIH, we were aiding and abetting the cloak and dagger movement, and there were several of us who thought very seriously of resigning from the NIH at that point in time. I think it was only the wisdom of Cassius Van Slyke, who persuaded some of us to stay here, saying that if we resigned, what kind of people would they get to replace us who would go along with this type of activity; rather, we should stay and do what we could do to keep the best scientific atmosphere and the best possible consultants at NIH.

To go back to Yale, then, I was talking about the set-up of departments. I think this was fairly general in all the medical schools in the United States at that time, that the head of a department of medicine, for example, would play his own hobby in his department, and although they would give more or less balanced instruction in clinical medicine, the greater research activity was based, because of lack of funds and specific interests, on those things that the head of department knew most about. This has changed considerably today, and I think most of the change is a result of the categorical approach to disease which has been brought about by NIH. In other words, with emphasis on heart disease, arthritis, diabetes, cancer, and so forth, it's made it possible for departments of medicine in medical schools to have broader programs and to have a more balanced department which addresses itself to all aspects of human disease, not only from the standpoint of teaching, but from the standpoint of having actual research going on in these various areas.

This was brought about, of course, first by imbalance. I remember back in 1951 or 1952. When I first came to NIH, I went around and visited almost every medical school in the country the first year I was here, and the thing that I heard most of all was; We're so glad that the new Institute is starting, which is going to emphasize arthritis and rheumatism and diabetes and other metabolic diseases. Because we have been terribly overbalanced up to this point by heart disease and cancer. At that time, there were institutes and there were a number of medical schools who thought that there was more available in the heart and cancer fields than there were in some other fields, which needed emphasis.

Through the general evolution and formation of other institutes, of course, this situation was alleviated, and alleviated to the extent that now we hear noises about the fact that NIH has overbalanced the medical schools in another aspect, namely, there's more emphasis on research and less emphasis on teaching than there should be. I dare say that as time goes on, this will rectify itself, too. But I certainly must confess that I am a believer in the philosophy that to ultimately get balance, you have to create a little imbalance, and then you progress and things catch up.

To go back to Yale, I suppose the greatest impact there on my ultimate career was this little research project, but I went through a whole series of questions in my own mind of what I was going to do in medicine. I went through a gamut of things, and many of these were connected with individuals who were strong and interesting teachers. At one point, I considered going into the infectious diseases part of medicine; at another, I even considered obstetrics and gynecology. Finally, however, I decided upon surgery, and when the time came to apply for internship, I applied at Cleveland City Hospital at Lakeside Hospital in Cleveland, at New Haven Hospital, and at the Presbyterian Hospital in New York. The latter three were for straight surgical internships. The one at Cleveland City was an ace up one sleeve; in case I didn't get any surgical internships, I would enter into this rotating two-year general internship. Then I was faced with a real decision, because I was awarded all of these internships, except the one at Presbyterian Hospital in New York, and I had to make up my mind which one to take. I finally decided on the one at Lakeside, because at that time, Dr. Elliot Cutler, who ultimately became professor of surgery at Harvard, head of the Department of Surgery there, he was in the height of his career. Lakeside Hospital and Western Reserve Medical School were considered, in the part of the country I came from, top medical institutions. They were near my home in West Virginia. I felt that perhaps I shouldn't stay at Yale, but go somewhere where they had different attitudes, my medical school attitudes, go somewhere where I would meet different people, rather than stay in the same groove. So I decided upon the appointment at Western Reserve. However, this didn't start until April, following graduation in June, because they took on two interns at a time every three months. I got a letter from Dr. Cutler advising me to spend the intervening time in pathology. Fortunately, Gene Woodruff, who was the friend who had helped teach me pathology after I got to Yale, had gone to Vanderbilt University Medical School as resident in pathology, and I wrote to him, asking if there might be some possibility for me to come to Vanderbilt for nine months in pathology. I almost immediately got a letter back from Dr. Ernest Goodpasture, who was professor of pathology at Vanderbilt at that time, saying that they did have an opening and that they would be very happy to have me come on as one of two assistant residents in pathology at Vanderbilt for the period of nine months. So this was settled in a hurry, and Goodpasture and Cutler got together and ultimately arranged for me to stay there for ten months, rather than nine months, and start my surgical internship one month later than anticipated.

This experience at Vanderbilt was the experience that decided me not to go into the practice of medicine, but to go into pathology. Dr. Goodpasture was one of the leading pathologists and researchers of this period of time. He is the man who developed the techniques of inoculating the chorioallantoic membrane of chick embryos with viruses, a technique which is used now to produce vaccines. At the time he was studying fowlpox, which is a disease of chickens and other fowl, which is associated with very large intracellular inclusion bodies. During the year I was there, I was embarked on a little problem relative to local immunity in the skin of chickens as a result of viral infection, and my job was to try to transplant skin from baby chickens to other baby chickens and get takes, so that we could solve once and for all the problem of whether viral immunity of this type was humeral or local. In other words, whether skin developed a local immunity rather than there being a general immunity. At that time we were not sophisticated enough to be able to produce viral antibodies. As a matter of fact, at that time it was not known just where the virus was in viral diseases that produced inclusion bodies.

Gene Woodruff and his wife, Alice, working with Goodpasture during that period, were able to digest the cells away from these large viral inclusion bodies, in other words, digest all of the cells by use of a trypsin solution which did not affect the inclusion body itself, to wash these inclusion bodies and pick them out separately through micro-pipette techniques, and introduce single inclusion bodies into the feather follicles of the chicken. It was through this technique that they proved that a virus inhabited an inclusion body or possibly even a virus formed the inclusion body.

During this same period, another event took place which I suppose paved the way for subsequent jobs that I got. This was when a student nurse died of a mysterious disease. I was called up at 4:00 o'clock in the morning to perform a post-mortem examination. This disease had not been diagnosed clinically, except in terms of an acute fulminating ascending myelitis, which ended with respiratory paralysis. The incision in the body was limited from the umbilicus to the xiphoid process, but in this limited incision, I was able to knock the bodies off of a couple of vertebra and get some fresh spinal cord, which was put into rabbits for testing to see whether or not some viral agent were present.

An interesting sidelight to that one was that I cut my hand during this autopsy, and ultimately, three weeks later, when we proved that this weird disease was a variant of rabies, we all became quite concerned about this cut on my hand that I had forgotten. I started taking the Pasteur treatment the day we proved that this was rabies, and this was sort of a traumatic experience, because it entailed three subcutaneous injections the first day through the second, and one every day after that for 19 days. Also about that time, I started reading the literature about the effects of vaccinations and found that all kinds of paralyses would develop as side effects of the vaccination. So we went through a very interesting guessing-waiting period there after we'd proved that this very bizarre acute ascending paralysis was indeed due to the virus of rabies. I wrote this up and published it in the JAMA in 1929, and this is referred to as the first well worked-out proven case of an acute ascending paralysis being caused by the virus of rabies without the hydrophobic symptoms that are usually connected with rabies.

Two or three years later, there was an outbreak of rabies in Trinidad. At that time, I was working at the Rockefeller Institute, and Dr. Flexner had received some specimens of spinal cord from some of the human cases in Trinidad, and asked me if I would like to see what was causing this. I was able to show that the cases in Trinidad were, indeed, caused by rabies and were similar to this one case that had occurred in Nashville in 1929-29.

Simultaneously, a group from the Wistar Institute worked on this and got the same result. Dr. Hirst and Dr. Pawan, Hurst being from the Wistar in London, were able to prove the vector in the Trinidad cases. This was the vampire bat, which bit animals that were infected with rabies, or humans, and then bit other humans and gave them this acute ascending paralysis which was a variant of rabies, as we had known at that point in time.

Before I left Vanderbilt, I decided to go into pathology, but I felt that a surgical internship would be good background for this, so I went ahead and took my surgical internship under Dr. Cutler at the University of Missouri.

I'd like to say a word about Vanderbilt. I think that this was an example of a small, close-knit, real center of excellence in which the young men had every opportunity to associate with the scientists who were there at the time, where even a youngster, his first year out of medical school, got to know the heads of all of the departments, where everybody bent over backwards to try to help and push the young men. It was quite small, it had just been reorganized as a result of grants by the Rockefeller Foundation. Dr. Leathers were dean. He had succeeded Dr. Canby Robinson, who at that point in time was playing the role of the organizer of certain key medical schools in the United States as a result of the Flexner Report. Dr. Robinson had first reorganized Washington University in St. Louis. He'd then gone on to Vanderbilt, and then he later went to Cornell, and was responsible for the early reorganization of the Cornell University Medical School in New York Hospital.

For one just out of medical school, to associate with what were considered at that time some of the big names in American medicine was a very unique experience, and certainly it was helpful in more ways than one, because if one was fortunate enough to do some good work, then one could lean on some of these individuals he'd known in the past for some support in what he might want to do in the future. I could talk for hours about Vanderbilt, but I'll go on to Cleveland. The internship at Cleveland was one of these routine affairs. One worked hard and long, took care of pre-operative and post-operative treatment of many patients, was lucky to get four to six hours of sleep a night, was lucky to get any night off. In those days, we didn't have every other night off in internships as they have today. There was one period of six weeks when the intern was on call for 36 hours in the accident ward, and then was off call for 12 hours one week. The next week, he was on call for 12 hours and off for 36, but during that 36, he worked in the surgical outpatient department.

Cutler had set up the surgical service, however, for the benefit of the house staff. There weren't very many surgical services in those days, and I doubt if there are any today in which the intern got the amount of operating experience that we got there. This was obviously under supervision up to a point. But I remember that we got to perform many simple operations, such as hernias, and even some appendectomies, along with such procedures as cystoscopy and particularly fracture setting. So it was very practical service. I was asked to stay on as an assistant resident, but at that point I had decided to go into pathology.

Somewhere in the middle of the internship, I got a letter from Dr. Simon Flexner, who was then Director of the Rockefeller Institute in New York, asking me to come there for an interview. This was a result of my having written to Dr. Goodpasture, telling him I'd decided to go into pathology, and was interested in any opportunities that might exist in that field in the future. Presumably he heard from Flexner, saying they were looking for somebody at the Institute.

The net result of this was in the summer of 1930, I went to the Rockefeller Institute in New York as an assistant in pathology and bacteriology immediately under Dr. Peter K. Olitsky, who was immediately under Dr. Flexner. There were one or two problems that I engaged in, which brought me in direct contact with Dr. Flexner himself. I was there for two years, a highly rewarding experience. I got to meet and know such people as Landsteiner, Sabin, Amoss, Leslie Webster, Alexis Carrel, Osterhout, and the other members of the Institute.

As a sideline, I also got to know Charles Lindbergh, because at that time he, having flown the ocean, was helping Alexis Carrel develop a profusion pump. Carrel had the idea that organs could be taken out of the body and kept alive if the proper fluids could be produced through them properly, and Lindbergh actually invented the spiral profusion pump, which was one of the first ones that worked well. They had had other profusion pumps at this point. Later on, we had whole batteries of these pumps diffusing such organs as thyroid, secretion of thyroid hormones. This was used contemporarily by a number of investigators at that point in time. I've lost track of that field now, and I doubt very much, however, if the Lindbergh pump is used anymore.

The particular problems that I participated in at the Rockefeller had to do chiefly with some undone work that Noguchi had left behind, Noguchi being the Japanese bacteriologist who Flexner picked up in one of his trips to Japan, who received the credit at that time for doing very, very remarkable bacteriology. To the best of my knowledge, in spite of the many, many publications Noguchi had to his credit, there was only one piece of work he did and that was his discovery of the etiologic agent of *Oroya* fever, or *verruga peruana*, which is a disease that exists in the Andes in South America. He showed that this was due to a spirochete. He also did a very fine job in his classification of spirochetes but nobody has been able to repeat anything else that I know of that Noguchi did and got credit for. He published a long series of papers on the etiology of yellow fever, and he proved that this was due to spirochetes. He was misled in this because he was getting blood from yellow fever from South American physicians, and these patients didn't have yellow fever at all, but they had Weil’s disease, which is caused by a spirochete. Noguchi, being a bacteriologist, didn't know the difference. So what he actually did was to confirm that Wilde's disease was caused by spirochete. He later went to Africa to study an outbreak of yellow fever there. He got yellow fever himself and died. The stories that I have heard—and I never met Dr. Noguchi—led me to believe that he was convinced that he was correct in that spirochetes did indeed cause yellow fever, and that it's conceivable that his infection of yellow fever in Africa was not an inadvertent one. It's pure speculation, but there's enough of that type of speculation going on at that time among people who knew Noguchi, that it is conceivable that this is what happened.

So this is a long way of saying that the work that I engaged in at Rockefeller was not very productive in terms of proving that another organism, namely—I've forgotten the name of it now. I spent a year and a half working with it. I'll think of it before we get through. It caused trachoma. This was a very small organism which was very difficult to grow, which was isolated from the eyes of children with trachoma in the Home for Mentally Retarded in New York City on Randall's Island. It also was isolated by Noguchi himself in some cases of trachoma in American Indians in the far southwest, and also from some material that was sent to Noguchi from Egypt. The organism was very difficult to grow. It was grown on some special media that Noguchi devised, both in what he called *Leptospira* media and on special blood auger plates. At any rate, the organism that Noguchi isolated found in cases of trachoma, which he called bacteria [unintelligible] did not turn out to be the etiologic agent of trachoma. We did do a number of experiments with the organism, however.

I think that the experience I got out of this had more to do with learning an imaginative approach to a problem and learning to develop techniques that were necessary to solve the problem. For example, this organism was very small. It was very difficult to isolate. In some other work that we'd been doing, using Berkefeld filters, it occurred to me that there was a possibility that this organism might pass one of the coarse Berkefeld filters because it was so small. Indeed, this turned out to be the case. We were able to filter suspensions of this organism through the bigger Berkefeld filters and get out all the other bacteria that were present, as well as cellular material, and get it out in pure culture. We published this in *Science* in 1931. This was Knutti, Olitsky, and Tyler.

We also devised techniques working in chimpanzees, where we could reduce a granular conjunctivitis with this organism, but in order to do this, we had to traumatize pretty severely their conjunctivae. The organism did take. It did not, however, produce the pannus formation, or clouding of the cornea as a result of the conjunctivitis. We were, however, able to inoculate the organism directly into the cornea and produce an inflammatory lesion, which ended up in a scarring of the cornea. These were all indirect evidence that maybe this thing did cause trachoma, but we never were able to actually produce trachoma that simulated, in all of its clinical manifestations, human trachoma. We were certainly off on the wrong track. Although I still think that it probably hasn't been proven that this organism might be a secondary invader in trachoma, which is now known to be caused by a virus, it might, because of the injury of the iris, produce a double assault which might be responsible for some of these manifestations that trachoma causes.

At any rate, it was a very fine experience, and I think I must get on the record another event that took place at the Rockefeller, which is not in the literature, which people who knew me at the time know about. That was during the time when we were working on trachoma. I had developed a little technique for measuring the numbers of organisms in a suspension, bacteria in a suspension, and had gotten interested in substances which possibly might inhibit growth of bacteria. This probably was actually suggested to me by Dr. Olitsky. So we tried a lot of compounds added to culture media to determine whether or not they would inhibit the growth of what we called a standard strain of staphylococcus which we had in the laboratory. I remember we used brilliant green was one of these compounds.

This was before the sulfa drugs came out, and it was very difficult to convince bacteriologists at that time that there was even the possibility of having any chemical agent that might destroy bacteria without destroying cells, living human cells, because everyone thought in terms of phenol units, and a certain concentration of carbolic acid will destroy bacteria, but in order to get it strong enough to destroy bacteria, it will destroy the cells of the body. Therefore, you can't get a chemotherapeutic agent strong enough to kill bacteria without killing body cells. This was a built-in tenet that these individuals believed, and there were very few people who had the imagination to set off on another approach to this, thinking possibly that there might be something to do with enzyme systems that the bacteria had which could be interrupted, rather than a gross assault of just poisoning the whole organism.

So we started measuring organisms and trying out various compounds to see if they would inhibit their growth in vitro. I was tossing out some plates out of our walk-in incubator one day, and noticed a green mold which was growing on many of these plates. Around the green mold there was no growth of organisms. My contemporary and peer, Phil Forsbeck, who was working in the lab at that time, and I started talking about this. We had not been bacteriologists, we had not seen this before, and so we called it to Dr. Olitsky's attention and said, "Look, here's green mold and there aren't any bugs growing around it. What's that?"

He said, "Oh, we've all seen that. This is differential nutrition. This mold grows so fast, it takes the nutrient substance out of the medium so rapidly, that the organism can't get to it."

I said, "What is this mold?"

He said, "I don't know, but go over to Avery at the Rockefeller Hospital and he'll be able to identify it for you. He's an expert on mold."

So I took some of it over to Dr. Avery. This was in 1931. Dr. Avery looked at it and said, "Oh, that's *penicillium notanam*."

I said to him, "Did you ever see this phenomenon?"

He said, "Oh, yes, we've all seen this. This is differential [mutation?]." Well, because of the fact that we'd been adding substances to media, to see whether or not they inhibited growth of organisms, Forsbeck and I thought it would be a good idea to grind some of this stuff up. We'd been working with filters, so we had all of the equipment necessary to make mold-free filtrates of extracts of the organism. So we grew ourselves a batch of *penicillium*. In fact, we grew many batches of *penicillium*. and ground it up with various solvents, everything from acetone to ordinary saline solution, added concentrated these extracts by a very crude lyophilizing mechanism which Dr. Olitsky had been using in some of his virus work, and added it to media. Lo and behold, the extract of this stuff, added to media, inhibited the growth of organisms significantly.

I got real steamed up about that, and I told Olitsky about it, and he began to get interested. He suggested that I go tell Dr. Flexner my story. So I did. Flexner was interested, and he said, "Have you read the literature?" I don't know whether he had or not.

I said, "No. I've been so busy working on this, we just thought we'd follow this thing through as far as we could and then get around to the literature."

"Well," he said, "I'd see if anything else has been done about this. We've all seen this phenomenon."

So I went down to the Rockefeller Library at the Institute and found in the *British Journal of Experimental Pathology* in 1929, an article had been written by a gentleman by the name of Fleming, in which he described this phenomenon, and he just described the phenomenon, really. He hadn't carried it, at that point, as far as we'd carried it in the laboratory there. So I took this article back up to Dr. Flexner. It took me a couple of days to find it. He looked over it, and he said, "Well, very interesting. It's all been done, hasn't it?"

I said, "But . . . but ... Dr. Flexner, don't you think this should be pursued?"

He said, "Well, this is Dr. Dubos' field, really." At that time, Dr. Rene Dubos had discovered the cranberry bog enzyme that dissolves the capsule off the type three pneumococcus. He said, "You've got other work to do. This is interesting. You've done a good job. I'd advise you to get back on your regular work." So this was dropped. Nothing ever came of it. Nothing much more would have come of it if I'd continued to work on it, but it taught me a lesson, that sometimes the administrators and the bosses in science are too prone to inhibit work that might turn out to produce something. I'm sure that if the Rockefeller Institute had gotten seriously interested in this substance, with the resources they had in 1931, that penicillin would have been on the market long before it was. But it took World War II and the formation of a team of chemists headed by [Sir Howard Walter] Florey, and that's what it would have had to have done. The pathologists and the bacteriologists couldn't get this stuff out. But it took Florey and his group to show how to make penicillin. I wasn't at all surprised when I read the headlines in the papers one day, when I was later in California, that a wonderful new wonder drug called penicillin developed. I didn't even have to read what penicillin was, because I knew immediately that this was something from *penicillium notanum*. which indeed it was.

That was an experience which was a lot of fun, as one looks back on it. At least one has the satisfaction of knowing that one was working on the right track on something once upon a time, and having the satisfaction of seeing something very crude work.

Dr. George White Whipple, Nobel Prize winner, professor of pathology at the University of Rochester, came to the Rockefeller Institute about the middle of my stay there, and gave a Friday afternoon staff meeting, in which he described his research. The way he described it, his general demeanor and knowing something about some of his work in the past led me to want to go to work in his department. I felt that the work at the Rockefeller was such that it took me away from direct connection with medicine. It was strictly laboratory work, with no connection with patients, and I wanted to get into an institution to pursue my career in pathology.

So I ultimately received an appointment at the University of Rochester, which was then a very young school, as an instructor in pathology under Dr. Whipple. There I got embarked on a whole series of interesting experiments and became much more interested in the experimental approach than to the aspects of pathology that a pathologist is supposed to address himself to or even to teaching. As years went on, I spent more and more of my time in research and less and less of my time in teaching and doing routine pathology.

The results of the research I did while I was there are on the record. It was a very fruitful and rewarding experience. It was rewarding in terms of productivity. It was not rewarding in terms of income. This was in the midst of the Depression, and when I went to Rochester, I went for the sum of $2,000 per year as an instructor, and room, board, and laundry in the staff house that they had as an adjunct to the medical center there, where house staff and unmarried members of the faculty could live.

In 1935 I was promoted to an assistant professorship, but at the time, Dr. Whipple told me that he was very sorry, that the work I had done certainly rated an assistant professorship, and he was promoting me to this position, but that because of the Depression and funds shrinking up, it wouldn't be possible to give me any increase in salary. So I became an assistant professor at the rate of $2,000 a year, plus room, board, and laundry.

In the meantime, I had had the opportunity to go some other places and make more money than this. But I knew, I think everybody would agree with me, that I was in one of the top experimental departments of pathology in the world, and that if I would sit still for a while and not become too avid, the time would come when I would get the break for not only a good job, but adequate compensation. So the roughly eight years I was in the department there, as I said, were very productive ones from the standpoint of research and research opportunity.

It would be interesting to speculate what would have happened if I hadn't got tuberculosis in 1940 and had to take a year off, at which time I had the opportunity to evaluate my whole situation. But it didn't happen while I stayed in the department at Rochester.

Dr. Whipple ran his whole department in this manner. In 1939, there were three senior members in the Department of Pathology there. One was Dr. Whipple himself, another one was Bill Hawkins, who was associate professor, and then I was the third. I was assistant professor. Hawkins was promoted to associate professor and he received $3,600 a year. This was not even in tune with some other departments in the medical school, but with all due respect to Dr. Whipple, whom we called "the dean," and he was also the dean of the medical school, he was a pretty hard penny-pitching gentleman from New Hampshire, and ran his whole department on a shoestring. I will say this, however, that I never needed any equipment or any technical help that I didn't get. Although salaries were not very good, we didn't skimp on equipment, supplies, and laboratory space. So it was an excellent environment in which to do research if one adhered to the old Osler tenet that you lived, drank, slept, and ate medicine and thought of nothing else. Indeed, this concept, I suppose, had more to do with the dedication of some of my contemporaries at that point, including myself, than should have. I must confess that I don't believe in it quite as strongly now as I did then, because it did slow down many individuals in being able to have at least financial security at a period where they needed to start to build up some estate.

Dr. Whipple is still living. I was in his office one morning, and I don't recall the year—that's certainly on the record—when his telephone rang, he picked up the phone, and he said, "Oh, is that right? Well, thank you very much," and hung up. And he turned around to me and looked over his half-glasses and said, "They tell me I just won the Nobel Prize." With no emotion whatsoever.

Q: I'll be darnn.

Dr. Knutti: Of course, he shared it with Minot and Murphy from Harvard, as a result of the liver therapy of pernicious anemia. So this was a cause of great do at the University of Rochester, to have the dean and professor of pathology share a Nobel Prize. There was speculation as to whether he'd go to Sweden to get it, because he didn't like to travel. He never went to medical meetings. In fact, he didn't read much medical literature. But he did go to Stockholm in the winter, took Mrs. Whipple with him, and brought back his share of the Nobel Prize. Whipple was responsible for the way the University of Rochester was set up and developed. When it was started in the mid-twenties, the oldest department head was, I believe, Dr. Bloor, who was 44. He was professor of biochemistry. Whipple was next oldest. I think he was 42. The rest of the professors were under 40, and this included such people as Bayne-Jones, Dr. William S. McCann, Dr. John Morton, who was professor of surgery, and the others.

This created a very stimulating atmosphere. Although it was a bigger school than Vanderbilt, it was still small enough so that the members of the staff could communicate with each other freely, where one had a general idea of what was going on in other departments, where one could go to get help from another discipline if they got into trouble on their own research problem. It's very interesting to note that at one point when I was interested in nitrogen determinations, I was trying to devise a micro-technique, I went to Dr. Henry Scherp, who gave me some good advice as to how to work this out. Dr. Scherp is now at NIH in the intramural research program of the Dental Institute.

The whole period at Rochester, as I've indicated, was devoted to research in the academic phase of medicine. It was my ambition to become head of a pathology department. In fact, I had two offers while I was in Rochester, and was advised by Dr. Whipple not to take them. One of these was at St. Louis University in St. Louis, another was at the University of Pittsburgh. For various reasons, Whipple felt that the head of the department at St. Louis University didn't have the voice in the administration of the medical school that he should have, and thought that I would be in a weak and indefensible position for departmental activity if I accepted that situation that existed then. In fact, he volunteered a letter to Father Schwitalla, who was then dean of the St. Louis University Medical School, indicating that he felt that every department head should sit on the Executive Faculty Committee, which at that time did not occur in that institution. I don't know whether Dr. Felix has changed that or not, but I dare say if it hasn't been changed, he will change it.

In July 1940, the University of Rochester decided that all of its staff should have routine chest X-rays. At that time, Phil Hawkins, who was the associate professor of pathology, and I rather scoffed at the idea as it applied to us, because we had been doing autopsies on patients with open tuberculosis for seven or eight years, at least, and if we were going to get TB, we'd certainly have it by this time. So it was somewhat of a shock the next week when the X-ray man came down to my laboratory and held up a film. I looked at it and said, "My God, is that somebody in the department?"

And he said, "That's you." This was something that took a little while to adjust oneself to.

An interesting little light on Dr. Whipple's character was that I went down and told him about this. I was perfectly healthy, apparently. I had no symptoms whatsoever. I had not lost any weight. I'd been working hard and playing hard. I played tennis and squash and prided myself on not getting more than six hours' sleep a night. I'd had no night sweats, no symptoms at all, no cough. But there it was in the X-ray. So Whipple didn't believe it. He said, "There's something wrong here. You're too healthy to have TB. Why don't you go over to the Iola Sanatorium (which was the county sanatorium, which was right near the medical center) and have Ezra Bridge go over your chest."

So I made an appointment with Dr. Bridge, and I went over there. So he looked at the X-ray and said, "We will take better films than that here. Let us take one of you." They did, and I went up to his office, and the X-ray technician called him on the phone. He said, "Yes, he knows about it. Bring it on up." It was so evident, the X-ray technician himself had made the diagnosis. So Dr. Bridge didn't go over my chest. He said, "Ralph, you'd better plan to take some time off. You're going to have to take some rest."

So I went back to Dr. Whipple and said, "Well, Dr. Bridge says he thinks I have TB."

Whipple said, "Did he go over your chest?"

I said, "No, he just took another X-ray", and Whipple said, "I don't know what's the matter with people these days. Why don't you go up to Saranac Lake for the weekend and get Heise to go over your chest. I'll call up Heise." Dr. Heise then was medical director at Trudeau Sanatorium.

So that week, Henry Keutmann and I drove up to Saranac Lake. They again took an X-ray and looked at it, said, "I guess you'd better plan to take some time off."

So I said, "Well, Dr. Whipple wanted somebody to go over my chest." He sort of laughed and said, "Well, we don't think we need to do that now."

So I went back to Rochester. In the meantime, apparently Heise had called Whipple, and Whipple was convinced then that I had tuberculosis, without having my chest gone over. An interesting coincidence is that when I went up to Trudeau as a patient a few weeks later, the first person who really did go over my chest was Don Whedon, who is now Director of the Arthritis and Metabolic Diseases Institute, who was up there recovering, himself, from tuberculosis.

This was an interesting year. I wouldn't want to go through it again, but for the first time in one's life, one got the opportunity to do all the reading he wanted to do, to listen to all the music he wanted to listen to, to learn some new things, such as painting, and to have really a sabbatical of sorts, also to take stock of one's life and one's career, and to make some decisions. While I was there, I made the decision that I was going to take a position, hopefully an academic position, but one which paid more than the Rochester job, and one which would give me the opportunity to run my own shop now that I had been working as second assistant under a distinguished individual for so long.

So ultimately, after doing some part-time pathology and being pathologist for the Genesee Hospital in Rochester after I got back from Saranac Lake, I was offered a position at University of Southern California as assistant professor of pathology there and director of the laboratories and pathologists of the Children's Hospital in Los Angeles. I was there for nine years until I came to NIH. I'm afraid that I'm spending a lot of time on before NIH, but some of this experience in California are a result of my coming to NIH.

I didn't know whether I wanted this position, but the Children's Hospital were kind enough to obtain my airplane fare from Rochester to Los Angeles, to look it over and discuss it with them. I did this in the days when the DC-3s were flying across the country. This was early 1942, during the war. I was quite thrilled to be able to fly across the United States in 21 hours, where it had taken me several years previously six days to drive across.

It looked like an interesting possibility at Children's. They had well-equipped laboratories, they had a very well-equipped animal house, a new suite of research laboratories on the top floor. Nobody had done much research there, but they had an endowment for orthopedic research. They also had an endowment of one research fellowship in hematology. Dr. Burrell Raulston, who was then dean at the University of Southern California, spent a great deal of time with me, telling me about the possibilities in the medical school and in the Department of Pathology itself. I've forgotten for the moment the professor's name; I always called him Ernie, and that's all I can think of right now. I'll provide that name later. He was professor of pathology, and he told me about the opportunities there in teaching and working in his department, although my base would be at the Children's Hospital some five to six miles away from the Department of Pathology, which was at that time housed in the Los Angeles County General Hospital.

So in the fall of 1942, I packed my belongings in my car and I drove to Los Angeles to take on this new assignment. Financially, this was a little better. It paid $7,200 a year, and this was a little in contrast to my salary at the University of Rochester. I saw possibilities in building up not only a good research department, but also in building up a little more income as time went on.

In a word, the experience at Southern California was rewarding and, at the same time, somewhat frustrating. At the time I agreed to go, which was very shortly after the war started, I had slots for adequate numbers of technicians and a pathology resident. By the time I got there, the draft had occurred, and the young man who had signed up to be the resident for the following year had been drafted, and it was too late to get anybody at that time for the ensuing year. Children's Hospital had 200 beds. The autopsy percentage had always been very high; it varied between 95% and 99%. While I was there, we led the nation in autopsy percentage rates for at least one year, I think maybe two. So there was a lot of gross pathological material, and there was a tremendous amount of clinical laboratory work which had to be supervised. I was very fortunate in inheriting a Miss Isabel Holbrook [sp?], who ran the clinical laboratories, a very highly qualified clinical technologist. She also ran a school for medical technologists, which was the only way we were able to survive the war, because we used our student technicians and paid them for night calls and helping out with determinations which we just didn't have enough manpower or womanpower to cover.

Insofar as research was concerned, it wasn't possible to get anything started with the load of routine that I had to take on because of the lack of personnel, and also because of the fact that I couldn't get any research technicians, even if I had had time to use them. So research was out of the picture until the war was over. At that time, I learned a lot. I learned how to get along with the surgeons and the practicing pediatricians, I learned a whole new approach to pathology, because pediatric pathology is quite different from adult pathology in terms of the types of diseases, and also the different ways in which some diseases that affect both adults and children manifest themselves in children. This was a very educational experience for me.

After the war was over, I decided to pick up on some problems that I had left hanging fire when I left Rochester. Shortly I found out that it was possible to get research grants through an organization that I'd never heard of before, which was called the National Institute of Health. I don't remember where I first heard about the NIH, but shortly after I first heard about the NIH, I was called on the telephone by an old friend who, with his wife, was in Los Angeles, and so we got together. He was in uniform, with a strange insignia that I had never seen before, namely the Public Health Service insignia. He was Dr. Floyd Daft, who later became Director of the Arthritis Metabolic Diseases Institute, who had worked in Whipple's department in Rochester when I was there for several years as the biochemist in the pathology department.

Floyd was one of us in Rochester who had been very close to each other. This group included Dr. Oliver McCoy, who is now head of the Rockefeller Foundation's China Board; Dr. George Packer Berry, who just retired as dean of the Harvard Medical School, who was at the Rockefeller the same time I was there, who went to Rochester at the same time I went to Rochester, then who went to Harvard from Rochester; and Dr. Henry Keutmann, who is now emeritus professor of medicine at the University of Rochester. As a matter of fact, this group owned a sailboat and sails on Lake Ontario for about eight years. We used to take a cruise every summer and go across the lake and fish and indulge in some of the activities on the Canadian side that were not possible legitimately on the American side because of the Volstead Act.

To go back to Southern California, Dr. Daft told me that he was at the NIH. The last real contact I had had with him was when he was at the Hygienic Laboratory working with Dr. Sebrell downtown, before the National Institute of Health existed as a name. So I found out a lot about the research activities in the Public Health Service during that visit, and I'm not quite sure what year it was, but it was probably about 1947. It had to be 1947 or perhaps even '46.

I learned through Dr. Daft that funds were available for research grants, so I wrote to the proper authorities in Bethesda and got some applications, filled them out. Lo and behold, I ultimately received two research grants from NIH for two separate projects. They were small in terms of NIH research grants even then, but they were very big in terms of the amount of money that was available at Southern California to engage in research activity.

Maybe we'd better stop now.

End of first interview