



J. HAROLD WAYLAND
(1901 - 2000)

INTERVIEWED BY
JOHN L. GREENBERG AND
ANN PETERS

December 9 and 30, 1983
January 16, 1984
January 3, 5, and 7, 1985

J. Harold Wayland, 1979

ARCHIVES
CALIFORNIA INSTITUTE OF
TECHNOLOGY
Pasadena, California

Subject area

Engineering and applied science

Abstract

An interview in six sessions, December 1983–January 1985, with J. Harold Wayland, professor emeritus of engineering science in the Division of Engineering and Applied Science. Dr. Wayland received a BS in physics and mathematics from the University of Idaho, 1931, and became a graduate student at Caltech in 1933, earning his PhD in 1937. After graduation he taught at the University of Redlands while working at Caltech as a research fellow with H. Bateman until 1941. Joins Naval Ordnance Laboratory as head of the magnetic model section for degaussing ships; War Research Fellow at Caltech 1944-45; heads Navy's Underwater Ordnance Division. In 1949, joins Caltech's faculty as associate professor of applied mechanics, becoming professor of engineering science in 1963; emeritus in 1979.

He describes his early education and graduate work at Caltech under R. A. Millikan; courses with W. R. Smythe, F. Zwicky, M. Ward, and W. V. Houston;

teaching mathematics; research with O. Beeck. Fellowship, Niels Bohr Institute, Copenhagen; work with G. Placzek and M. Knisely; interest in rheology. On return, teaches physics at the University of Redlands meanwhile working with Bateman. Recalls his work at the Naval Ordnance Laboratory and torpedo development for the Navy.

Discusses streaming birefringence; microcirculation and its application to various fields; Japan's contribution; evolution of Caltech's engineering division and the Institute as a whole; his invention of the precision animal table and intravital microscope. Involvement with the Athenaeum. Friendship with Sidney Weinbaum; Weinbaum's trial.

In the last two sessions, conducted by his daughter Ann, he reminisces about growing up in Boise, Idaho; living conditions as a Caltech graduate student. Further comments on Copenhagen, colleagues there, meeting Niels Bohr, and his career at the University of Redlands.

Administrative information

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Preferred citation

Wayland, J. Harold. Interview by John L. Greenberg and Ann Peters. Pasadena, California, December 9, 30, 1983; January 16, 1984; January 3, 5, 7, 1985. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web:
http://resolver.caltech.edu/CaltechOH:OH_Wayland_J

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH J. HAROLD WAYLAND

PASADENA, CALIFORNIA

BY JOHN L. GREENBERG AND ANN PETERS

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Pasadena, California

Interview with J. Harold Wayland

by John L. Greenberg

Session 1	December 9, 1983
Session 2	December 30, 1983
Session 3	January 16, 1984

Supplemental interview

by Ann Peters [Wayland's daughter]

Session 4	January 3, 1985
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SESSION 1

December 9, 1983

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GREENBERG: Let's begin with your family background.

WAYLAND: Well, I was born in Boise, Idaho. My father [Charles W. Wayland] was an architect. He was there before the days when very many people were trained at the university level, so he took up his architecture by apprenticeship. He came to Boise really to work as a draftsman. The man who had the office had a contract to build a federal building. Apparently, when a nice fat payment came through, this guy skipped the country. They completely exonerated my father, so then they turned the job over to him. Well, he found himself with a business as a young architect. I don't know whether

he was married before or after that, but my mother [Daisy McConnel] was born and raised there in the Boise Valley.

My maternal grandparents came across from Iowa by train to Salt Lake, and then they had to take a covered wagon from Salt Lake to Boise. They taught in the Boise Valley for a long time. But they were living in Boise itself—not when Mother was born, but by the time Dad got there.

It was 1909 when I was born there. The family made its home there all the time. My elder brother [Charles] was born about five-and-a-half years ahead of me; I was supposed to be the girl in the family, but I fooled them. My brother decided he would go on into architecture, so he stayed and carried on the firm until he retired some years before his death. I was the black sheep of the family in that I was more intellectual. The family wanted my brother to go to Paris to study at the Beaux Arts. He took his degree at the University of Washington—had absolutely no interest in travel. I don't think he ever got out of the United States, except to Canada and Mexico.

But the early years were—what kids do in a small town. While I was still in grammar school, I got interested in radio. They had one of the first broadcast stations in the West there in Boise. Harry Redeker, who was the professor of chemistry, was a ham radioman. He first built a spark set, and then he managed to get some equipment out of the Signal Corps, which permitted him to put up a broadcast studio. I remember when I was an eighth-grade student in the spring of 1922; I played with a small group of my fellow students. We put on the first live broadcast that was given there in the Boise Valley, which was pretty early in the history of broadcasting, especially up in that part of the world. I played the piano, and one of my friends sang, another one played the cornet. I always wanted to get involved in the radio program. I was told that you cannot take the radio course until you had physics. So, I sneaked into the physics course as a sophomore—I wasn't supposed to take physics until I was a junior. But I had no trouble with it. And then Harry Redeker said, “Well, I don't think you ought to take the radio course till you have taken chemistry.”

Well, then, I took chemistry. Working with my sidekick, Dick Hollingshead, we had gotten so far ahead of the class that Redeker had to give us something to do to keep us busy. So, he set us to work. He was a teacher who was working for his doctorate at

Stanford; he needed some thorium and some zirconium, the pure metals, because he was studying the ability of an arc to oscillate: How could you get an arc to oscillate to get high frequencies out for communication? It eventually turned out that if you add a slight bit of radioactivity this would do it, but they just knew empirically that some of these substances would and some wouldn't. So, Dick and I got busy and produced a small amount of both thorium and zirconium, which we purified, which was better than Redeker could buy and gave us a lot of *Fingerspitzengefühl* as we worked in the laboratory. Then, of course, we were behind the class and we had to work like hell to catch up with it. But we finished easily, well ahead of the class.

Then I was hooked on chemistry, so I was going to be a chemist. I had been torn, however, because when I went to high school, I had decided I was going into law, so I went in for debate and public speaking. Then I got interested in writing and worked on our school newspaper and decided maybe I'd go into journalism. But finally, science prevailed. The family wanted to send me to the University of Washington, where my brother had gone. But I felt that if they would just promise to help me in graduate school on the money they saved by letting me go to Idaho instead of Washington, where they would have had to pay tuition, then I would go to Idaho and study chemistry. At Idaho, the best course was in chemical engineering, not in chemistry proper. So, I proceeded to go into the chemical engineering curriculum, and on the side, I took advanced courses in physics. I found the chemistry terribly dull, because what I'd done in high school was far more advanced than what they were giving us in freshman chemistry. I found the physics quite exciting. But as I got along in my chemical engineering curriculum into my junior year, they forced us to take a lot of trivia—a special thermodynamics course for chemical engineers, which wasn't even as scientific as the one they gave to the mechanical engineers, which was by no means as scientific as the course in heat I had been taking in the physics department. And the same thing was true of electricity and magnetism. I was supposed to take a course in electrical machinery. The professor admitted that I was wasting my time, but the dean wouldn't let me out of it. I'd already had more electricity and magnetism in the physics courses than they were giving in the specialized course for the chemical engineers. So, in disgust, I had a big battle with the dean, and in the middle of my junior year I left the engineering division. But in the meantime, I had been elected

to Sigma Tau, which was the engineering honorary society, because I was one of the top students. Eventually, while I was a senior, I made Phi Beta Kappa at the beginning of the year, before the regular election, although I'd only been in letters and sciences for six months. So, I became one of the unique creatures who was both in the engineering honorary and in Phi Beta Kappa. When I graduated, they made me an associate member of Sigma Xi.

By that time, I had moved into physics, though—not into chemistry—because the physics professor got very interested in me. He had taken his PhD here at Caltech. He wanted me to go to Caltech and do theoretical work, because I had taken essentially a triple major—in physics, mathematics, and chemistry. I applied at several schools. My first acceptance was the University of Washington, and I had to let them know before I would hear from anyplace else. It took a lot of soul-searching to turn them down. And then I began to get rejections. Finally, I got first alternate at the University of California, Berkeley, and then the offer from Caltech came through, with a teaching assistantship, because I had to have it.

At that time in the Depression, 1931, Dad wasn't even taking in office rent, so the promise of helping me with my education was out the window. There was just no possibility that he could do it. So, I came down—I had relatives here in Southern California anyway—and dropped in to see Earnest Watson [professor of physics and assistant to Caltech head Robert A. Millikan] to see what my teaching assignment would be. Earnest looked over my credentials and said, "I think you ought to teach mathematics." So, for the three years I was on a teaching assistantship, I taught mathematics. I started with sophomore math, and then they decided I was good enough to teach freshmen. The next two years I taught frosh.

Also, at that time, you paid tuition even if you had a teaching assistantship. But your tuition, instead of being \$250 a year, was \$180 a year. Once you were admitted to candidacy for the doctorate, the tuition was cut in half. That extra \$10 a month for the nine months looked awfully big, so the group of us that had TAs just broke our necks trying to pass all of our candidacy courses in the first year.

There were five courses. Electricity & Magnetism was taught by [William R.] Smythe. Analytical Mechanics was taught by Fritz Zwicky; we used Painlevé & Platrier, a French text.

Smythe gave problems of great mathematical difficulty. And he was not a good teacher. He would lay stuff on the line, and he would give you very difficult problems; you had to work them out. If you finally pinned Smythe down, he would, at least, make a real effort to explain it. But with Zwicky, it was very difficult even to understand him half the time. And it was only two-thirds of the way through the year that he discovered that very few people read French well enough to understand. He was just quite unconscious of the students' problems, so it was quite a different level of difficulty. I also took another course of Zwicky's later, in astrophysics. That was very clear, very beautifully presented, but it was entirely lectures based on things he was learning at the time.

GREENBERG: We get the impression that in the early thirties, Zwicky was really out there at the forefront of astrophysics research. And it wasn't until many years later that his work became important.

WAYLAND: The stuff he was most interested in in the early thirties was in crystal structure, and his ideas of cooperative phenomena. A lot of his ideas have taken hold. I'm sure other people also had some of the same ideas. I know there's a lot of question of how much credit he really deserved in some of these. But he certainly was a real pioneer on a lot of things.

To be a doctoral candidate, you had to have advanced calculus. I didn't need to take advanced calculus; I'd already been through it. But rather than take it off by exam, I just took mathematical analysis out of Whittaker & Watson, from Morgan Ward. That was a splendid course. We had an awfully good group in there. And then there was a mélange of spectroscopy and atomic physics that took up a year. Then there was a course in mathematical physics, which [William V.] Houston taught. I figured if I had passed the other four by going to class, I could pass the math physics by exam because it covered

much the same material. So that's what I did and managed to get my candidacy in the first year.

There were ten of us given teaching assistantships; they only really had assistantships for eight, but everybody had accepted. And with the tough financial times, they weren't very anxious to keep everybody around. Five of us came without master's degrees; five already had their master's degrees. At the end of the first year, there were six of us left. I was the only one who didn't have a master's degree who survived. The other four couldn't make it. They all went on and did advanced work elsewhere and have done very well. But they just didn't have the analytic background. I came with so much more mathematics than most of them.

GREENBERG: So, you were really prepared for the rigors of the Institute when you came.

WAYLAND: Much better than most of the students from small colleges, yes. It was mostly because I had had a good, sound background in classical physics; but I had an especially sound background in mathematics. That's what saved my neck, actually. And, of course, that's one reason that I kept up my interest in mathematics. During the second year, I started doing research with Otto Beeck, who was a German who had been working on the ionization of gases by ion bombardment, and then he got interested in ionization by accelerated atom bombardment. You accelerate the ions, pass them through a cloud of the same gas. A certain fraction of the collisions will result in a transfer of an electron, from the neutral atoms in the gas to some of the accelerated atoms. Those accelerated atoms which continue in essentially a straight line and which have lost virtually no forward momentum, have essentially the initial velocity. So, if you can sort those out, you've got an accelerated beam at energies far above anything you can achieve thermally. Otto built a tiny little thermocouple to measure the intensity of the beam. We worked on the efficiency of ionization of accelerated argon beams.

He decided he wanted to go back to Germany. Just before he left, he said, "Well, you know, this is my field. You can't work in it." I wasn't mature enough to tell him to go to Hell, which I would do today. So, I just felt, "I guess I have to drop it." He suggested I move into studying the excitation of various substances with ions. I fooled

around for about a year, building up the little mass spectrograph to get the selected ion beams, but I never could get the intensities I needed. After all, ion sources available at that time, in the early thirties, were quite different from what you can get today. We were dependent on hot cathodes with a coating on them. I never could get the thing to work worth a damn. In the meantime, I had accepted a position to teach mathematics at the University of Idaho. I thought I'd be finished, but I wasn't. But I was fool enough, again, immature enough, to feel that I had to keep my commitment instead of telling them to go jump in the ditch, which I would do today.

In the meantime, I was married in the spring of '33 to the daughter of an old friend of my Father's from Idaho. Virginia [Kartzke] and I were born in Boise four weeks apart. Our parents had known each other before we were born and knew each other all through our growing-up period. Somehow, we never met until she was teaching in Glendale, having finished at UCLA, and I was here as a graduate student.

She gave up her job, which, of course, was a mistake, because married women couldn't get jobs—they could hang on to them, but they couldn't get them. During this year at Idaho, I was given the poorest section in one of the elementary math courses. They selected the students, put the best ones in one section, middle ones in another, poor ones in another. The students did very badly on the final exam, but if you took the total curve, they fell right on it. The professor was out of town. I turned in an undue number of F's. I started back to Caltech when I got a telephone call to come back to find that I was being fired because I had flunked too many students. And the professor wasn't even there to mediate.

So, we packed up our goods and came back. Well, of course, I had not applied for a teaching assistantship, so there was no teaching assistantship available. But Earnest Watson was a wonderful, wonderful person—you've probably heard this from others. I just wish we could say as much for any of the provosts that we've had since. None of them has held a candle to him in terms of a combination of guts and real understanding of the needs of a faculty, and a willingness to tell either the faculty or the Board of Trustees to go to hell. He did it in a gentle way; he was a preacher's son. But he did it very effectively. And he was a real proponent of the faculty throughout his entire period, and of course, he was just Millikan's assistant.

But he dug around, looking for a job for me. They'd had a man with a PhD running the stockroom, because jobs were too scarce. Of course, he got a job, so they offered me the stockroom. Well, I was working, and doing my thesis officially under Millikan, so I asked the Chief about it. That looked like an awfully heavy job, to continue and finish up my thesis. So, he said, "That's only eight hours a day. That leaves you eight hours a day for research." So, I took it. I moved back to the atom bombardment.

By that time, Beeck had come back to the States. He couldn't stand Hitler, and the girl he wanted to marry refused to go to Germany. She was a Russian refugee who had come out through China, and she was damned if she was going to be under another dictator. So, he came back to this country, married Kisa, and went to work for Shell Development on catalysis. He realized that he'd never get back to the atom bombardment, so he turned that field back over to me. So, then I got back into something I knew. And after rebuilding the apparatus two or three times, I finally was able to finish up my thesis. I had hoped to get it finished in order to take my degree in June. In July, we rebuilt the whole apparatus from one end to the other, and finally I had assistance. A student assistant had been helping take data, and we were just getting nowhere, because he was so meticulous. Finally, I threw him out and put my wife to work. We'd take a set of data in ten minutes, whereas it would take us an hour and a half to two hours with this student. But pressures were changing; we were dealing with pretty crude vacuum pumps in those days—mercury-vapor pumps. We didn't have good oil pumps like we have now. So, anyway, I did get it finished up.¹

In the meantime, I had been given an American-Scandinavian Foundation Fellowship to go to Copenhagen to work in the Niels Bohr Institute for Theoretical Physics. But instead of getting there in the beginning of the academic year, we didn't get there until after the first of the calendar year [1937]. So, I didn't have as long there as I had hoped, but I had several months. It was a fascinating time, because it was just at the time when there were a lot of refugees coming from Germany and eastern Europe—Jewish refugees—who were using that as a way station. And many truly outstanding

¹ "The Ionization of Gases by Atom Bombardment," 1937.

individuals, like Otto Frisch, who's been on the faculty at Cambridge for many, many years, and Victor Weisskopf, who's been at MIT for a good many years. Hilde Levi—I don't know where she went, but I think she went to Sweden. George Placzek was the man I worked with most closely; he came to this country and died quite young. And the work we did was on essentially the self-absorption of a neutron beam. Apparently, George used it a lot in his work on some of the A-bomb development. But it got clamped down and couldn't be published. When the war was over, we thought we ought to get it published, but we were so far apart we just never did get the stuff together. As I understand it, the General Electric people in particular used it a great deal, but it never did get published. That time didn't end up with one of the things required in academia—a piece of paper.

When I came back from Copenhagen, jobs were terribly scarce still. I had no job. I came back here and picked up a little bit of tutoring. My wife worked in the office in the bookstore, for forty or forty-five cents an hour, something like that. I eventually picked up Hobart Bosworth's son, to try to get him back into Caltech—he'd flunked out. I finally had to say, "Look, I will take him on if, and only if, you bring him over, leave him, and he sits here all day." Mama was a big nuisance. She didn't like it too much, but they paid me enough for us to live on. Most of the time, I just had him study on his own; that's what he needed, to learn how to study. He got back into Tech with flying colors, and he lasted another year before he flunked out again.

But by February, there was big squabble at the University of Redlands, and they fired their president, so the head of physics was made acting president. That left them with a hole in the physics department, so they asked for somebody to step in on a temporary basis. So, I took that job.

GREENBERG: When you came back, you continued with the work on neutron absorption here?

WAYLAND: Yes, I continued some of that work.

GREENBERG: Did you have anything to do with the Kellogg [Radiation] Lab people—[T. W.] Bonner, [W. M.] Brubaker, etc.?

WAYLAND: None whatsoever. I knew them personally very well, of course, but no direct contact, no. I did a little bit of work when I had free time, finishing up that paper, which I then sent off to Placzek, who was supposed to polish it up and get it ready for publication. Well, he procrastinated so much that it never got published before the war, and then it got buried. I don't know if I even have a copy of it.

But anyway, I took this job to teach physics there at Redlands. I found that they were terribly lacking in anyone to teach advanced mathematics; the professor of mathematics there just didn't know analysis. In the meantime, I'd gotten tangled up a bit with Harry Bateman. I'd taken his partial differential equations course while I was a graduate student here—the year he wrote his book on partial differential equations. So, I came back the first summer there. In the meantime, Redlands had decided to make Professor [Herbert E.] Marsh the acting president; thus, they had an opening in physics, so they offered me a position as assistant professor. Since I was able to teach some of the advanced courses in physics, and particularly some of the advanced mathematics that they didn't have anybody else to teach, it was a good deal for Redlands.

The first summer I worked as a research assistant with Bateman. Then I was promoted to research fellow from then on out, until I left for the war. But out of that, he was interesting, but I didn't quite grasp what he was really excited about.

GREENBERG: Bateman seems to have been a rather unusual person.

WAYLAND: Oh, he was. He was, well, almost a sweet person, if you want to use that term. He was very gentle. Most students were scared to death of him. I was when I was his student. Eventually, I got to know him and realized that, for instance, when he would lecture, he would print on the blackboard in letters about this big [gestures]. If you weren't in the first row, you could hardly read it. But when we took the differential equations course, because he had published the book, everybody thought, "Well, now here's something he likes." So, thirty-five people signed up. When the year was over, there were seven of us left. But he would write along, and then he would say, "Now gentlemen, it is obvious ...," then he would think a bit, and he would do a little doodling, and he'd write the result. He would be working it out with these little doodles. We used

to call that “Bateman’s theorem”: “It is obvious that...” Later I found that if we’d had the guts to ask him to explain, he would have been delighted! But we were all so intimidated by him that we just didn’t have the courage to ask him.

A beautiful example of his approach to a lot of things was during that period when Einstein was here in residence [1931-1933]. And the Abbé Lemaître came here from Belgium, I believe it was, who was another great relativist. And Richard Tolman, of course, was an outstanding relativist. There was this special seminar for a discussion among these three, with Tolman in the chair. And Tolman was flapping back and forth, in French to Lemaître, in German to Einstein, and English to most of the people in the room. The gist of the seminar was to discuss a particular model of the universe. I don’t know whether it was Lemaître’s model or whether it was one of Einstein’s models. It was a relativistic model with no mass in it. And the thing I remember more than anything else is that there was a great big capital lambda on the blackboard, and that was the integration constant. The point was that if lambda was zero, the universe was stationary. If lambda was positive, the universe was expanding; if lambda was negative, the universe would be contracting. And they went around and around, much of which I didn’t understand because of the languages. I could read French and German, but understanding was another matter. As we got up, Bateman was sitting next to me. He turned to me and said, “You know, I wonder if the gentlemen ever stopped to examine whether their basic premises are correct?” [Laughter] He really put his finger on it.

GREENBERG: Did he influence you?

WAYLAND: Oh, in many ways, yes. Bateman and [Morgan] Ward gave me a feeling for mathematical analysis, which is the only field of mathematics in which I have any competence. And [Theodore von] Kármán, from whom I never took a course, but I sat in on some of his lectures, gave me a feeling of the need for a physicist or an engineer or a person who was using mathematics as a tool, to try to understand the physical meaning of the terms that go into the differential equation, or whatever equation you’re writing. It was primarily differential equations that I was concerned with.

GREENBERG: Did you get that from Kármán? He influenced you?

WAYLAND: That aspect of it I got from Kármán. Even Bateman was very much a believer in understanding the hypotheses on which you base things. But he looked at it in a more global way, whereas Kármán would go through a differential equation to explain what each term meant, and what it meant physically. When you came out with the result, you had a feeling for the physical sense.

Ward gave me a beautiful feeling for the tools of mathematical analysis. I'd had a fair background, but it was Morgan who gave me the real feeling for it in that course in math analysis. And then, with Ward, we went through Courant's and Hilbert's *Methoden der mathematischen physik* [1924-1927], which was available only in German at that time. We took two terms of that. And the third term we went to John von Neumann's *Mathematische Grundlagen der Quantenmechanik* [1932], which was an attempt to bring together different philosophies of the quantum mechanics through an appeal to Hilbert space, which is really what it amounted to. But it was those two men who gave me my mathematical philosophy. And it was that on which I based my course when I came back here to teach applied mathematics, which was very much behind the thinking in the two books I wrote on mathematics, the differential equations book and the complex variables book.

GREENBERG: Did you have anything at all to do with [E. T.] Bell or [Aristotle] Michal? Did you know them?

WAYLAND: Oh, yes, I knew them. I knew them quite well. I sat in on some of Bell's courses. I had an interesting experience with Michal. This research I was doing with Bateman ended up with my moving into a study of the expansion of determinantal equations into polynomial form, largely because Sidney Weinbaum, who was working with [Linus] Pauling, felt the great need for better techniques. They were just beginning with the computer to solve a determinantal equation, which they were running into all the time, by any of the matrix powering methods. It was just too cumbersome to use the computer for this, because the computers weren't fast enough. And so, if we could work out algorithms for getting them into polynomial form, then the tools for solving the polynomial equations were very strong. So, I was working on that. And in the course of

that, I ran into several interesting manipulations on matrix equations. I wrote one of them up for possible publication. I handed it to Michal, who was supposed to be the great expert on matrices and tensors. And he just tore me limb from limb, “Nothing original, not worth a damn. You just shouldn’t have the temerity to do something like this.” I forgot about it as best I could.

During the war—this was just before the war—Michal got involved in consulting with the aircraft industry, and he was being forced to work out procedures for solving some of these problems. When I came back to work here on the war projects, the minute he spotted me he cornered me and made me come up and talk to one of his classes on essentially these techniques that he thought were beneath dignity before the war.

GREENBERG: And what do you make out of all this?

WAYLAND: Well, admittedly this wasn’t pure mathematics I was doing—it was applied mathematics. And Michal was, in his own mind, a very pure person, until he found there was money in the applications.

GREENBERG: Was he a good mathematician?

WAYLAND: As far as I know, yes. I really had so little to do with him that I had no real way to judge him. His widow is still a good friend of mine. But I really have no way to judge him. He had a good reputation. But the men I respected the most were, of course, the men I was closest to—Bateman and Ward. I think Ward in some ways was a more original mathematician than Bateman, but Bateman had a sense of the physical. He was really an applied mathematician, in the days before computers. He certainly made me realize the need to try to see your way through and to look at the answer and see if it makes sense. Of course, Kármán was superb in that also. He didn’t influence me as directly, but I think there was a strong influence from Kármán.

GREENBERG: So, you feel the mathematics department of the thirties was pretty good?

WAYLAND: Within the field of analysis, and in number theory. You see, Bell and Ward were really number theorists. And I would say that in those areas, they were good, very good. And Bateman was very good in applied analysis. I just can't judge where Michal fell into the picture. Of course, most of the rest of the staff, like [Luther Ewing] Wear and [Harry Clark] Van Buskirk, were just crank turners. They taught the classes and kept the students trained to a point where they could solve the necessary equations for their physics courses.

GREENBERG: Did Bateman have anything to do with the origins of your interests in hydrodynamics?

WAYLAND: Not really.

GREENBERG: Kármán?

WAYLAND: No. My interest in hydrodynamics came quite accidentally during the war.

GREENBERG: You know, Bateman is said to have described applied mathematicians as mathematicians without conscience, according to Maurice Biot.

WAYLAND: [Laughter] That's interesting.

GREENBERG: Do you make anything out of that? Does that surprise you?

WAYLAND: Well, yes and no. Bateman really had a very good sense of humor. It was hard to find, he being British; and we're not used to the British sense of humor. He really had a very good sense of humor. Just to give you an anecdote that might throw a light on it, though in a very left-handed way, he got interested in finding cheaper methods of reproducing mathematical manuscripts. This, of course, was in the days before Xerox. So, he got interested in the possibility of automatic typewriters. I remember his giving a lecture on the typewriter one day. The typewriters were all over the lecture table, and he was talking about some of these old models, when he came to a word he just couldn't

find. He looked utterly blank, and finally you could see the light come up on his face, “Typewriter.” At that time, I thought surely, he had just had a lapse of memory. But after I got to know him better, I wouldn’t be a bit surprised if this were a deliberate gag. He did have a sense of humor, and I can see that, in a sort of a facetious way. Of course, at that time, you couldn’t do as you do now—set the problem up as accurately as you can understand it in mathematical terms, and then depend upon the computer to get you out of the soup.

Of course, in linear problems, that’s quite straightforward. In nonlinear problems, it was much harder to know whether the result you get makes sense or not. The whole tendency was to make approximations. And it is here where a question of conscience or no conscience had to come in. I remember, we asked Morgan Ward to solve certain problems during the war. And sometimes he’d say, “I can’t tackle this; I don’t know enough of the physics to know which terms I can drop. Without a real understanding of the physics, I don’t know what to do with the mathematics.” And the mathematics was too complicated to be solved literally as it stood.

So, I would interpret that in this sense, with Bateman. But I don’t remember ever hearing him say it.

GREENBERG: Were you at all familiar with Bateman’s work on theories of radiation in the twenties, trying to account for the Compton effect?

WAYLAND: No.

GREENBERG: No? Because I guess theoretical physicists found him pretty strange. Paul Ehrenfest, for example, who visited the campus in the early twenties, wrote a paper jointly with him, and said he had a hard time understanding Bateman.

WAYLAND: His students always had a hard time understanding him. But, again, Ehrenfest should have had the background to be able to talk to him man to man. But I found as I got better acquainted with him, there was no one in the world I’ve ever met who was so anxious to help if you were in trouble and had the courage to stick by. It’s true that he would often give you references; you’d have to go dig them up and read

them, and then try to understand them. And then, if you were still missing, you could come back to him. But he was always generous with his time, his willingness to help. But the point was, he seemed formidable to most people; I think it was partly shyness and partly his reputation. One of my great regrets was the fact that he was dead before I came back on the faculty. We used to see him socially as well as in science. He had a lovely family; he had an adopted daughter.

GREENBERG: Does that pretty much take care of the prewar years?

WAYLAND: Yes, that pretty well takes care of it. Because all of the research I was doing was here on campus on this paper that eventually came out in the *Quarterly of Applied Mathematics*.

GREENBERG: Had you abandoned atomic and nuclear physics?

WAYLAND: There was no opportunity to work at it. The paper I had worked out with Placzek was still hanging fire, and I couldn't get him off the dime. I didn't feel competent enough to fire it in under my own name, with his name on it. He had to approve, and he just never got things done; he was just that much of a procrastinator. So, there was just no opportunity to get back to some of these things.

GREENBERG: Was there anybody else around who was doing the variety of work that you were into then?

WAYLAND: You mean the kind of research?

GREENBERG: Yes. By this point, you'd already done some atomic and nuclear physics. Now you were doing applied mathematics with Bateman—all in the span of just a couple of years.

WAYLAND: Of course, even during the time I was doing my doctorate, after Beeck left, there was no one interested here.

Begin Tape 1, Side 2

WAYLAND: When Otto Beeck left, and left me more or less to work on my own, Millikan was interested in the problem we were working on, but he was not deeply interested. At that time, no one understood the cathode fall in a gas discharge. The role of the photoelectrons had not yet been recognized. And somehow, gas discharges took place much more readily than they ought to. And the potential distribution along the length of the tube was essentially inexplicable. One thought was that a neutral atom—this came out of some of [Fritz] Zwicky's predictions—ought to be more efficient in ionizing gas than one that has a negative charge, because there's that much less charge holding electrons in place. So this was the reason why Beeck started studying the ionization efficiency for accelerated neutrals. And we found the efficiency was distinctly higher than for ions of the same mass. And then I studied onset potential, or velocity, as it were, at which one can begin to ionize a gas. What is the minimum energy required to knock an electron off of an argon atom or a neon atom dealing with an accelerated argon atom? All of this was to try to understand more clearly the theory of the gas discharge.

Millikan had been interested in gas discharges for a long time. But at the time I finally came back to finish my degree, I was the only one on campus working in the field. When I needed help that I couldn't get out of Bill Houston or Ralph Smythe, I usually went to Berkeley and talked to Leonard Loeb or Robert Varney. Varney did his thesis on the same topic, but using quite a different method, as a matter of fact. Interestingly enough, our data agreed quite well. Loeb, particularly, was helpful. But there was nobody here to talk to, and nobody really interested in the work. There had been some work on mass spectrometry with Smythe—Lynn Rumbaugh, for example. And there was some cross-interest there. But the real problem I was working on was of no great interest.

To me, another amusing Millikan anecdote—when I finished up, he brought in the president of Technicolor. He was taking him around the campus. I was to discuss with Millikan that day what was to be done with my equipment. So, Millikan came in with this Technicolor man. I explained what my work had been and what the apparatus was all about, and what the different things did. I said, "Now, of course, the question is, should this work be continued?" Millikan asked my opinion, which was that if there weren't a new approach, there was no use continuing in the same direction. So, as he left

the room, he gave a very dramatic gesture and said, “Scrap it!” And I could hear this visitor mumbling, “What a wonderful grasp this man has of so many details.” Well, this all sounded very great. That afternoon, I dropped down to see Ralph Smythe. He said, “By the way, the Chief was just in asking me what we ought to do with your equipment?” [Laughter] He was putting on a show in typical Millikan fashion.

GREENBERG: So, when the war began, it seems that this part of the research you were doing had come to a dead end.

WAYLAND: It had come to a dead end. That’s right. I could have moved on, had I been at an institution where it was possible. It was not possible at Redlands. It’s the kind of work that you cannot do by just dropping in for a summer. If there’s somebody doing it, then you can drop in and work with them; but there was nobody doing it here. And so, I’d gotten started on this theoretical work with Bateman. In fact, when I went to Copenhagen, my thought was, here’s my chance to see if I am indeed a theoretical physicist or had better stick to experiments. I came to the conclusion that theory was not my forte, at that time in Copenhagen. Although the kind of work I was doing with Bateman, which was more applied mathematics than theoretical physics, was something I seemed to be able to grasp. And then, when the war came along, I was under pressure from friends; they needed people in military research. I went before we got into the war and ended up in the Naval Ordnance Laboratory. There I got moved into protection of ships against magnetic mines. I had had a good background, from surviving Smythe and [Paul] Epstein. Smythe gave us the tools, Epstein gave us the understanding. And the most efficient way was to take them simultaneously. But I took Epstein after Smythe, because I had to pass Smythe to get my candidacy to get my \$10 a month extra money. And the next year I took Epstein. And Smythe just fell into a pattern; I began to see what the hell it was all about. Epstein was a magnificent lecturer, beautiful teacher. He was one of the great teachers. So was Houston. Smythe was a terrible teacher, but if you stuck to it, you could learn a lot working under his tutelage.

GREENBERG: Was leaving Caltech to go to the NOL connected with the research coming to a dead end?

WAYLAND: No. You see, I was still teaching at Redlands, and I really went from Redlands. My tie with Caltech was through Bateman. That work could have continued. But there was no way I could carry on an experimental program at Redlands, and there was no stimulation to carry on any theoretical work. So, it was just a feeling of patriotism more than anything else. The pressures were significant from people who were looking for help. I took a leave from Redlands and got involved in the magnetic mine work—building magnetic models of ships, and before long found myself head of the magnetic model section. We had some damn good people.

GREENBERG: For example?

WAYLAND: Oh, John Bardeen, who had two Nobel Prizes in physics! He wasn't directly in my section, but he was head of the theoretical section and I was head of the experimental section, and we worked very close together. John Kraus—I don't know what the devil ever came of him. John Sweer—again, I've lost track of him. But Bardeen I have kept in touch with, not just because he made a Nobel Prize, but because he was a great guy, and I happened to have some reason to cross paths with him at various times.

And so, we went to Washington, DC, with a six-month-old and a not-quite-three-year-old. It was pretty traumatic. Of course, the war broke out. We went on what I used to call a five-and-a-half day week—nine hours a day, five days a week, and eight hours a day on the sixth. Lynn Rumbaugh was more or less my boss. But I just felt that my family was going to fall apart if we stayed in Washington. So, I screamed bloody murder. When [associate director of research] Ellis Johnson finally came to the conclusion I was definitely going to leave, he pulled strings and got me a good position with the 11th Naval District. They were just building a new degaussing station here in San Pedro. So then, I got into real engineering. I did enough engineering design, electrical design, at that time, that I could easily have qualified when the war was over as a licensed electrical engineer. But I felt it wasn't fair, because I just didn't have the breadth of background. But I don't know many people in current times who had as much

experience as a director of design. We had to build breakers capable of handling very high currents, and a lot of very elaborate equipment.

GREENBERG: I thought one of the problems with engineers in those days, and maybe a lot of engineers now, is that they don't have much breadth.

WAYLAND: Well, that's true, and of course, this is where my background in physics was so helpful. In fact, what's kept me alive through the whole business is that solid background I had in optics from Ike [Ira S.] Bowen and in E&M from Smythe. Boy, I've lived on that the last few years. I had never had a course in fluid mechanics in my life. But that good solid course in E&M gives you all the tools you need for classical non-viscous fluid mechanics. As soon as you get into viscosity, you've got another story. But as long as you're dealing with ideal fluids, it's all there in the mathematics of Electricity & Magnetism.

GREENBERG: Did Bateman teach you anything about ideal fluids?

WAYLAND: No.

GREENBERG: He was supposed to have been a real expert on perfect fluids.

WAYLAND: Oh, yes. He even had a book on ideal fluids; I've used his book a little bit. But the differential equations course was more existence theorems and things like this, and strange and peculiar functions, which I'd gotten a good start on in Whittaker & Watson. No, I must say that the two fields that I have worked in, other than the mathematics which I taught for a good many years here, the fluid mechanics and the medical research, I had no formal background in.

GREENBERG: And you came back to Caltech from San Pedro?

WAYLAND: I came back here, yes. The research phases of that work were gone. We had built the station; the station was operating. I got involved in detection of lost objects

underwater using electromagnetic detectors. And I used to come up with the mine disposal unit to look for lost torpedoes in Morris Dam. When the time came, it just looked as if it made no sense to stay with the 11th Naval District. So then, the question was where to go. I was under some pressure to go to Los Alamos, and had I done so, I would have been back in nuclear physics. But Virginia was pregnant at the time—we lost the child later—and I didn't know for sure what they were doing there. The idea of going up to the boondocks in New Mexico just didn't sound like the thing I wanted to do with a pregnant wife. We had two small children. So, I talked to Earnest Watson about the possibilities of coming up here. And he was prepared, if any one of the teams wanted me, to take me on. And Fred Lindvall decided that I'd fit into his team. So, I moved into the torpedo development group and essentially took over the hydrodynamics of water entry. So that's where I got started, really, on hydrodynamics. And of course, this is quite a different hydrodynamics than the simple fluids, the ideal fluids that you can do with complex variables and simple linear differential equations.

The first theoretical problem I solved was to work out the actual pressure distribution on the nose of a torpedo as it hit the water. And then we measured it with little indentation pressure gauges, and it fitted the theory quite nicely. So it gave us some data to work on in calculating what the force—essentially the moment, that would be given to the torpedo body when it hit the water and was given a kick on the nose that would tend to make it swing. We had to work out basic ideas—much of it was qualitative—to give us some idea of how the thing would behave as it made this big cavity and went into the cavity and felt this force on the side of the cavity, and where would it go. How you kept it from going out and hitting the airplane that launched it was one of the big problems. Bob Knapp [director of Caltech's Hydrodynamics Laboratory (aka the Pump Lab)] really solved that problem empirically in his water tunnel, putting the ringtail on it.

GREENBERG: Did Lindvall have an influence on you?

WAYLAND: Oh, yes, a very great one. Partly by indirection. He was the kind of person who believed in giving people rope; if they used it to climb with, great. If they hung

themselves, that was their hard luck. And this was the way he operated his division. He was a very easy man to work with in many ways, though he had very high standards. As long as you were producing in an area that made sense, you were free to do it, and you weren't being pushed.

A shift really began about the time I moved from NOTS to Caltech. You see, I stayed with the Naval Ordnance Test Station and was chief physicist for underwater ordnance for some time and then became the head of the Navy's Underwater Ordnance Division. We built the big variable angle launcher at Morris Dam, and I had a lot to do with the instrumentation side of that.

We did a lot of work on water entry of torpedoes, and I also had to worry about getting money when I became head of the division. Particularly, I had to worry about getting money for instrumentation, for control, and so on. Gardner Wilson still lives just across from the Athenaeum, and he was head of my instrumentation section. Don [Donald E.] Hudson was very much involved in the mechanical part, but he left very quickly to go back to work here on the faculty. Milt [Milton S.] Plesset was my chief theoretical physicist for a time, and then he was offered a job here. Then I followed him about a year later.

But in the course of our experiments on water entry, we got interested in certain phenomenon on boundary layer growth. What happens to a boundary layer? You can't use the ordinary laws of hydrodynamics. When the Reynolds number gets above a certain number, you're going to shift from laminar to turbulent boundary layer. You're dealing with a transient phenomenon; you're dealing with a time-dependent phenomenon. How do you get at this? And there was an attempt to work out procedures for getting at this. But I got interested in the molecular level and started looking for tools that would allow us to make these measurements. You see, you couldn't put pitot tubes on the end of a torpedo that was going into the water at five hundred miles an hour. They'd get ripped off instead of making measurements. So, I got looking into the possibility of using optical tools.

GREENBERG: So, there's some generic connection between getting interested in the transient phenomena and what's going on at the molecular level?

WAYLAND: That's right. The basic thing was that I felt that you needed a probe at the molecular level in order to have something that responded fast enough to follow these rapidly changing events, and that could also stand the tremendous forces that were involved in these rapidly changing events. So, then I began to look at streaming birefringence—the double refraction of light due to orientation of elongated macromolecules.

GREENBERG: This is during the war years?

WAYLAND: No, this was after the war. No, during the war years, we were very much involved with hardware and getting the measurements we needed to make something work. And it was only toward the end, about 1947, '48, that I began to get interested in this—just before I came on the faculty here at Caltech. While I was still with NOTS, I was physically in Pasadena all the time; I was never stationed at Inyokern.

GREENBERG: Can you summarize the impact that the Second World War had on the direction that your career took?

WAYLAND: Oh, it changed it completely, because here I had pretty well gotten out of atomic physics, or nuclear physics, and was feeling my way into applied mathematics. But then along came the war, which forced me into things dealing much more with experimental situations. When I left the degaussing and came up here, I found myself in a completely different field. I found myself in certain aspects of fluid mechanics. I was also trying to organize and administer an organization [the Naval Underwater Ordnance Division] that was heavily engineering oriented, but I felt very strongly that I had to maintain a research competence or we would be way behind and never really make any progress. So, as Fred [Lindvall] told me when I took over, “Now, look, fight every inch of the way to keep basic research going. Keep the research as basic as you can, because that's where the future lies.”

GREENBERG: He was out of the same school that you were.

WAYLAND: He took his PhD here, yes [1928].

GREENBERG: Right, in EE. But I mean, he had the equivalent of a physics major.

WAYLAND: Yes, that's true. EE and physics were in the same department in those days. It wasn't very different; it would depend on your direction of interest and direction of your thesis more than the courses you took.

So, I fought hard to keep the research going, and when it proved to be impossible, I resigned and made the mistake of telling the truth in my resignation letter instead of writing the usual poppycock. I made a very specific statement that I had come to the conclusion it was impossible for me as an individual to develop a suitable career in an organization that was under the direct control of a military officer when the work we were doing should be under the control of a civilian scientist, which is a fact.

I fought hard with [NOTS director L. T. E.] Thompson to try to get him to get the setup so that the civilian side was truly reporting to a civilian head and didn't have this dichotomy of administration. Ralph Bennett, who was the head of NOL at the time, was trying very hard to achieve that. And Thompson killed it. He thought he could make it work. But in my opinion, that has been one of the great disasters of military research.

GREENBERG: So, you feel Inyokern should have gone the JPL [Jet Propulsion Laboratory] route?

WAYLAND: [Nods assent] There is a very real place. There would have been a slightly different direction—in fact, a very strongly different direction. There is a need for military input, but the military input is at the user end. My suggestion was that those military people who came to work at NOTS, if they were professionally competent as scientists—and I thought there ought to be a certain number of them that got involved in the development of things from the very beginning—should get the hell out of their monkey suits. I thought they should work according to their capacity and not according to the number of stripes they have. They should really get to learn what the problems are with their input as ultimate users into the design. But the real place for the military was at the top—military as military as distinct from individuals. This is what we need—work

out with the senior scientific staff a set of goals and from there on out, let the senior scientific staff work down through their regular staff to try to achieve those in the best way possible instead of meddling all the time as they tended to do.

A good example: When we were building the variable angle launcher, we had hired [Caltech civil engineer] Fred [Frederick J.] Converse to work on the stabilization of the hill, because the hill was badly shattered and there was a serious question whether it could ever have withstood the load of the heavy truss that was put on there. Fred made a very careful study of it and gave us a set of plans for grouting, which we did. They changed the construction officer in the 11th Naval District. He came up, and the first thing he did was insist that we fire Converse. And there was absolutely nobody in the Navy department in this area who knew enough about it to have given us the right advice. Fortunately, we had all the advice we needed, and we got the hill stabilized, so that it wasn't a serious loss, but it was because of that kind of attitude that I felt I didn't want to make a career here.

So then, I came to the conclusion that I was going to leave NOTS, and I came over to see Fred Lindvall because Abe Zarem was really twisting my arm to go to work for the Stanford Research Institute. I didn't know whether Stanford Research was a respectable organization or not—I thought frankly no. “Well, sure, it's a perfectly respectable organization,” Lindvall says. “Are you really leaving NOTS?” I said “Yes, I'm absolutely done with it.”

“Well, in that case, how about letting me have a chance? And I'm not supposed to proselyte”—one of the agreements when they split up. “But if you're really leaving, would you consider taking a post here at Caltech?” So, we started negotiating, because just about that time, the man who had been teaching applied mathematics died suddenly, and he was hard up for an applied mathematician.

GREENBERG: Who was teaching applied mathematics?

WAYLAND: I can't remember his name, a man I never knew. He died from polio I think it was—very quickly. Fred knew I had taken some time out when I was working for him to finish up this paper on determinantal equations, so Fred knew I was interested. And he

had inquired around. Of course, Bateman was gone, but he asked Morgan Ward, and Morgan was very positive. So, we negotiated, and I finally said, “Well, I will come only on condition that I come at least as associate professor with tenure.” I was not going to take a job without tenure. They finally came through with it. I took a considerable salary loss. So, in January of ’49, I came on the faculty.

GREENBERG: As associate professor of applied mechanics?

WAYLAND: That’s right.

GREENBERG: What did applied mechanics designate in 1949?

WAYLAND: Of course, there was no applied mathematics here at that time. Applied mathematics was under applied mechanics at that time. It included the basic courses in mechanics, such as [George W.] Housner and Hudson were teaching—you perhaps know their textbooks. Frankly, I didn’t pay much more attention to it. I was very interested in what I was doing. I came here, actually, with the full understanding—a verbal but not written agreement—that I would never have to go outside to bring in a penny of my salary. And I felt that I wanted to be a teacher. I’d always enjoyed teaching, and I felt that I would be willing to make my entire career in teaching. It didn’t take very many years to discover that you can’t be a teacher at Caltech. If you’re out of research here, you’re never going to get beyond the associate professor level. You can be a teacher if you’re willing to sit as associate professor all your life. So, I was forced into research to the point where, toward the end of my career, I didn’t do anything *but* research.

GREENBERG: I asked the question concerning the rubric “applied mechanics” because Kármán apparently wanted to call aeronautics at Caltech applied mechanics—I mean the GALCIT [Guggenheim Aeronautical Laboratory at the California Institute of Technology] program.

WAYLAND: In a sense it would have been a valid use of the term. A recommendation that came out of a committee that I sat on some years later with George Housner as

chairman, was that the engineering division should give only two degrees—engineering and engineering science. Applied mechanics, then, would have come under engineering science. Although Fred [Lindvall] was busy preaching all over the country as president of the American Society for Engineering Education, the glories of engineering science, he would never lift one finger to give us any backing on campus. If there had been a strong enough group to insist on engineering science, I think it might have flown as a discipline. It was considerably undefined at that time, and it still is, as far as I can find out, in the same way as I find bioengineering and biomedical engineering badly defined today. They are not very uniquely defined in any way. But to me, there is a very real distinction between the person who wants to make things and the person who wants to lay the scientific background on which the making of those things is built. To me, that is the real, I think what they call, applied physics, here. Ninety-five percent of it could honestly be called engineering science.

As it happened, [our division chair] Roy Gould came up from physics—of course, I came up from physics, too, but I was somewhat polluted by the war. I just feel that the engineering division would be better off, instead of all of this proliferation of curriculum, being more like biology, with essentially a degree.

In this case, I think there is a real distinction between engineering—the people who build things—and the engineering science, people who are trying to lay the background, either to understand, or on which things can be built. And I felt that was a very sound recommendation; I still think it is. I think it's a sounder recommendation than what's happened. Lindvall refused to raise a finger; he accepted the proposal and I never knew what became of it. Housner was chairman of the committee. And it just died, as these things so often do. There were enough people who had their own little empires, who liked to build their own little empires. I'm facing that in my own current interest, which we can come to later.

GREENBERG: During the war, applied mathematics as an academic discipline began, as two institutes were created, at Brown and at NYU. Were you aware of what was going on? Were you following any of this with any interest?

WAYLAND: I was not particularly interested in getting involved with them. I was aware of them. And, of course, Charlie DePrima, who was my closest colleague here for a good many years, taught the engineering math course with me for quite a few years before Jim Knowles came. I was aware of the Courant Institute. I've had some contact with some of those groups.

In fact, I'd even had contact with Eli Sternberg; he's one of the professors in applied mechanics here, one of the senior people. I first ran into him when he was in Chicago [at the Illinois Institute of Technology]. I tried to get him interested in solving some of our problems. He spoke just like so many applied mathematicians. He virtually wanted you to write the differential equation; then he'd show you how to solve it. But he had no grasp of the physics, just as Morgan Ward failed, though Ward had the courtesy to say, "Well, look, this is beyond me, because there's too much physics here. I don't know what I dare neglect." I used to like to tell my students that a differential equation was like a sweater girl's sweater; you couldn't get anything out of it you didn't put in there; it's awfully easy to put on a falsie and then be surprised when the falsie comes out. And my philosophy in teaching applied mathematics always was to try to put the real emphasis on the formulation of the problem. It's true that they had to learn how to solve them, too, because we didn't have the computers. As the computer became more and more a way of life, I was somewhat outdated. It was either retool there—but in the meantime, I had been so busy retooling as a physiologist that I didn't feel I could retool in a second direction.

GREENBERG: When you came back to the campus and continued with the work in hydrodynamics that you'd already begun, did you have any connections with people in GALCIT? Kármán was gone by that time. Did you have any connection with any of the aerodynamicists?

WAYLAND: The only person I ever had any connection with at all in this respect was Phil [Phillip M.] Eisenberg. I first made contact with him when he was at Cambridge. I gave some lectures there, I met him there, and we had some interest in the problem of spheres moving near a wall, and whether they would move toward the wall or away from the

wall—which became important when I got interested in blood flow. He was the only one I ever had any real contact with. And most of my contact was before he ever came on campus.

My closest contact with anyone on the faculty here, actually, was with Jerry [Jerome] Vinograd. I was very close with him. And I always sent my students over to take his course in macromolecular characterization, because that came out in my time in Strasbourg [as a Guggenheim Fellow, 1953-54], more than anything else. As in dealing with streaming birefringence, I got very much involved in how molecules behave in a hydrodynamic field.

GREENBERG: Did streaming birefringence come next?

WAYLAND: That really is the next thing, because I came to the conclusion that that was probably the only way I would ever be able to study this boundary layer development. So, if I were going to understand how effective this could be as a flow visualization tool, I had to understand more about the phenomenon. So, I discovered that Charles Sadron had been here before the war, working with Kármán—I didn't happen to know him, though we were here at the same time—on the use of streaming birefringence as a means of observing hydrodynamic fields. He had established after the war the macromolecular research center there in Strasbourg. It was the Centre d'Étude de Physique Macromoléculaire at that time, and it became eventually the Centre de Recherche sur les Macromolécules. It was there that I went on my Guggenheim, in 1953-1954, with the primary idea of trying to see to what extent we could use the double refraction induced by flow as a means of deducing what the flow pattern was like.

GREENBERG: Nobody in this country was studying anything?

WAYLAND: Very little. There was, a little later, a group in the South which got very much involved. But, unfortunately, they just didn't understand the theory. And this is where, I think, I was very fortunate in having chosen to go to a place that was very strong in theory. The setup was very, very good there. The problem was to unscramble those things that involved simple orientation of the molecule, which when it interacted with the

electromagnetic field, gave you the optical properties—or a distortion of a matrix of the fluid. In one case, you're dealing with something that is essentially only associated with the strain, with the velocity differences. In the other case, you were dealing with something associated with the stress. Now, you have two extremes, one which is pure stress, one which is pure strain, but most things are in between. The trap that most people fell into was they would get something that had very strong birefringence, but it was not linear with concentration, which showed that you were dealing with micellar organization and not with just simple orientation. And then there was the distortion of the micelle, and even the breaking up of the micelle that changed the birefringence. So that, if you were on one extreme or the other, the analysis was straightforward. The piece of work I did in Strasbourg was on a system that was strictly at the molecular level, not on the big, long molecules, but strictly on the molecular level. And then, when I came back here, I did some work on—I also did some work there—orientable systems. But they were systems that were a mixture. Then I had my students here work with tobacco mosaic virus, which was a long rigid particle, and in dilute suspensions there was little enough interaction; we could treat that as a simple orientable system. And Sal [Salvatore P.] Suter, who later became chairman of mechanical engineering at Washington University in St. Louis, did a very nice thesis on that², in which we were actually able to map the flow in a journal-bearing configuration, including the backflow and everything else. But, when we tried to get the velocity above a certain level, we found that the relaxation time of the particle was too long. And we began to see not what was happening at that point, but an integral of what was happening there, plus something that was happening upstream. This gave us a clue as to what was really happening.

And I still think that the major paper I wrote on the possibilities and the limitations has never been superseded. It's a fairly big paper that was published in a polymer journal.³ It has been republished by the journal of rubber research or something of this sort. As far as I can see, every once in a while somebody asks me about it or sends me something to review, and I really don't think there's anything more to be said.

² Suter, S. P., "Streaming Birefringence as a Hydrodynamic Research Tool," California Institute of Technology (1960).

³ "Streaming birefringence as a rheological research tool," *J. Polymer Sci. Part C*: 5:1 (1964).

GREENBERG: It is interesting you had to go to Europe to get started on all this. GALCIT by this time is renowned for studying aerodynamics, but nobody is working on problems at the physics-hydrodynamics interface.

WAYLAND: I couldn't find anybody. There are people now in this country, but there wasn't anybody at that time who was really strong. And this institute in Strasbourg was the leading institute in the world. Nick [Nicholas W.] Tschoegl has worked there several times; Bob [Robert F.] Landel has worked there. It's really very outstanding in polymer science; it was the outstanding institution at that particular time.

In the meantime, people flare up every once in a while in the field. In particular, chemical engineers try to do something with the subject, but generally it's a bunch of crap. There was a group in the South, but I don't think that work was ever worth anything, as far as I'm concerned.

GREENBERG: Does that bring us up to the physiology?

WAYLAND: Almost. That comes pretty close. As I correlated that, because of the theory of streaming birefringence, what we're doing is treating the rotary diffusion coefficients of these particles. We're taking what really is a stochastic process and writing a continuum equation, a linear continuum equation. Also, when we take the flow of viscous fluid and we take the Stokes approximation—the creeping flow approximation—we're also dealing with a linear approximation to a very complicated nonlinear equation. Although nobody's ever derived it using a stochastic approach, to my knowledge, still the complete Navier-Stokes equation is supposedly based upon the fact that the molecules are random motion.

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GREENBERG: Is there anything you would like to add to our previous discussion?

WAYLAND: I suppose we could go on from that particular point. In considering the theory of streaming birefringence, and the fact that here we were combining the rotary diffusion equation with the creeping flow approximation to hydrodynamic flow, I was always surprised that the Peterlin-Stuart theory, which was based upon combining these two equations, really worked for molecules. They seemed to me too small for these equations to apply. I was wondering, what really was the lower limit of size of object, for which we could make this kind of combination and have it really work. So, I had some students work on trying to get some measure of what the local hydrodynamic field was that was being felt by an elongated particle, which was being oriented by a shear flow and was being disoriented by molecular motion.

One thing that brought this to my attention was a very close association with Jerry Vinograd, over in biology, who was very much involved with the use of hydrodynamic techniques for the characterization of macromolecules of various sorts.

GREENBERG: This is during what period?

WAYLAND: This was in the late fifties, after I came back from Strasbourg. I used to sit in on some of his classes on macromolecular characterization; I got interested in it when I was in Strasbourg. One thing that struck my attention was a statement he made. He pointed out that if you built up the bulk viscosity of a fluid which you were going to put in the ultracentrifuge, and then had macromolecules go through it, you built up the bulk viscosity by sucrose and then built it up to the same bulk value with serum albumin. You found that you got a different sedimentation rate of a big molecule in the two cases. In

other words, the tiny molecules of sugar interacted in a different way than the scarcer population of big molecules of the serum albumin. A broken field run, for example, allowed you to spend some time at a low viscosity region, and yet it was being interfered with by these big molecules scattered around, but if you have a lot of little molecules, you couldn't make a broken field run through it. So, I got interested in how we could measure the local viscosity.

It was on this basis we started a program of studying the interaction of spherical particles with elongated particles. By measuring viscosity, we could mix them. And we couldn't tell whether the change in viscosity came from one or the other. But, if we could look at it optically, we would only see the long particles; the spherical particles wouldn't show up except to scatter the light. So, we started with a study of the interaction of rods and spheres using tobacco mosaic virus as the rods, and to begin with, polystyrene latex spheres.

This worked fine for the viscometric studies. But the polystyrene latex spheres, which were 880 angstroms in diameter, were too big for the optical studies. They scattered too much light; we couldn't get a good picture of the birefringence, due to the orientation of the tobacco mosaic virus. So, we moved over, then, to southern bean mosaic virus as the spherical particle. I know I got my fingers green, because I had to grow all of the virus over in the plant laboratory here. They had a special section set aside for plant virus work. I'd have to infect the bean plants and then, after they had been properly infected and I collected the leaves, I brought them over here. My technician, Paul Knust-Graichen, then isolated the virus. He was very, very good. I know Vinograd borrowed some; I let him have a sample. He said it was the best sample he ever had of that particular virus—mono-disperse. So, we did have, then, a spherical particle, which did not scatter too much light, which we could then put in with the elongated particle and study the interactions.

GREENBERG: Let me just interrupt here for a second, so I can make sure I'm clear about two basic things. Your interest in micro-hydrodynamics probably goes back to the transient phenomena that you were working on during the war—torpedoes.

WAYLAND: I was interested in trying to find an optical tool to measure these transient phenomena, because I felt that any of the mechanical tools were too big, too clumsy, too slow, or would be torn off by the forces when things were changing so rapidly. So, I felt we had to go to something that was molecular. So that is where the interest arose. Then, I felt that particularly, in the course of the thesis that Sal Suter did on flow visualization by the orientation of macromolecules—again, tobacco mosaic virus. I worked out the theory for the general two-dimensional flow because, prior to that, we had only the theory for simple shear flow. And so, I had to extend the theory for a two-dimensional flow. There was no point in three dimensions, because we had to have the integration along one axis, where everything was the same.

GREENBERG: But now you're talking about a problem in biology.

WAYLAND: But I moved. I got interested in this through flow visualization. I felt I needed to understand the fluid mechanics of what was going on at the molecular level. And I felt that by the time we finished Suter's thesis, we had pretty well wrapped up the understanding of the use of streaming birefringence for flow visualization studies. Altogether, in the work I did when I was in Strasbourg with ethyl cinnamate, the work that Sal did here with the tobacco mosaic virus, and my study of the literature of work that was done with some other materials, I felt that we had pretty well identified those phenomena which were pure orientation phenomena—such as the orientation of a low concentration of tobacco mosaic virus—and those phenomena which required a change in essentially the structure of the fluid, due to the mechanical forces, which also changed the optical properties.

And the one side, where it was strictly strain-related, the theory was well developed—I think largely by [Roger] Cerf in Strasbourg. That which was strictly orientation-related had been developed first by Peterlin-Stuart and extended here in this lab. I thought we had the thing pretty clearly understood, and I wrote a major summary paper for that, which was published in a couple of places.

This was in the late fifties. Then I got more interested in the phenomenology, and particularly interested in the biological applications, because of my contact with

Vinograd. So, I set my students to work on this interaction study; [Marcos] Intaglietta, who is now professor at UCSD, and [Daniel] Collins, who is now professor at the Naval Postgraduate School. We got to working on the basics on the experimental side, and then [George S.] Argyropoulos did a theoretical study that fitted along this line.

But eventually, I came to the conclusion that the effects that Intaglietta was observing on the orientation and the modification of orientation due to the presence of the spheres, might well have been partly due to the rods sticking together. We didn't have them isolated; they tended to stick together and were acting as dimers and not as single particles, so I didn't feel we could really explain our results. We had the results, and we had some theories, but we never really got them together. At about that time, I began to get interested in blood flow.

GREENBERG: When, and why was that?

WAYLAND: Well, it was about 1960, and I had been interested, for a long time. When I was in Copenhagen, Melvin Knisely, who was one of the grand old men of microcirculation, was working in August Krogh's laboratory. Krogh was a Nobel laureate for his work in microcirculation. Knisely and his wife and Virginia and I were living in the same pension. I was in Bohr's institute; he was in Krogh's institute. And he felt that they needed physicists in this kind of work. So, he talked me into coming over and looking through the microscope and seeing these crazy patterns of flow and so on. But in 1937 I knew no physiology; I knew I couldn't do it alone. He was living in Chicago and eventually moved to South Carolina. In those days, we didn't have easy air access, and it was just foolish for me to try to get involved. So, my interest toward the biology end got stirred up by Jerry Vinograd, and I actually was supported by the National Institutes of Health for the research that we were doing on these interactions.

GREENBERG: But as far back as 1937, you saw that there was something there?

WAYLAND: Oh, yes; definitely. And then in about 1960, Wallace Frasher—who was an MD physiologist working on the distensibility properties of the pulmonary artery in the Cardiovascular Research Laboratory, which was then part of the Loma Linda University

but at the County Hospital—wanted to go back to school to retool. He'd started out life in social science, had put himself through USC on their prize debate team—he has a wonderful gift of gab—and then he went into the Army. During that time—I think he worked as a medical technician—he decided he wanted to study medicine. So, he came back and studied medicine. At no time had he been really trained in the physical sciences or mathematics, but he realized the need for it. He'd done a lot of study on his own, so he was really quite capable of understanding problems even when he couldn't do them *ab initio*. His boss, Sidney Sobin, didn't want him to leave, to go off to spend this year retooling. Wally had a career development award from NIH, so his salary was paid by the government. So, Sid called Anthonie Van Harreveld here in biology. And Van Harreveld knew of my interest in Knisely's work, because I had shown some of Knisely's films here and when Knisely came to give seminars, I was always there talking to him, and so on. And Van Harreveld knew Knisely, so he knew I was interested. So, they said, "Well, why don't you have Wally talk to Harold Wayland?" So, Sid sent Wally over—they called and made an appointment—and Wally and I just hit it off beautifully. I was getting a little bored with what we were doing. The idea of getting into a physiological problem where I had somebody with whom I could talk and who was willing to teach me as I was willing to teach him seemed to me a very good idea.

Also, about that time, my older girl was already through college and was in graduate school. Both girls went to Bryn Mawr: Ann had a fancy scholarship, and the younger one had a Seven College Conference Scholarship. But as a Caltech professor with something like \$7,000 a year salary—I had my full professorship by then, I guess—Bryn Mawr thought I was making enough money that I could pay their tuition. They gave the girls practically no financial help, even though they had the top scholarships the school offered. So, I had to do something to pay for their tuition, so I continued to consult—for Aerojet General on mine warfare—to bring in enough money to pay their tuition. But I was getting fed up with that, and I no longer wanted to be involved with military research. When this opportunity came, as soon as the younger girl finished college in '62, I just dropped all military research, asked to be relieved of all of my clearances, and said the hell with it. I wanted to put my time on things that had hopes for

doing something for people, rather than killing them off. And this contact with Frasher gave me the break.

We managed to get some funding out of the L. A. County Heart Association to get it started. On our first attempt at getting funds out of NIH to shift from the molecular work I was doing to something more physiologically oriented, they turned his half of it down completely and gave me only half of what I asked for, which was a bare start. At least we got a start. And we still had some money from the Heart Association, which helped us get going. I worked with them some on the distensibility properties of the pulmonary artery, and then we got interested in blood flow, blood rheology, because I felt that's where I could make contributions.

GREENBERG: Was the fact that a lot of what you asked for you didn't get a commentary on biomedical research then?

WAYLAND: The thing it says the most is something I've learned by sitting on the NIH study section. Things get funded that have been proved. More and more people say, "Well, after all, you have to have the research 90 percent done before you can get funded for it. Then you slip in the new ideas, *sub rosa*, and get those done, in order to get the money to pay for that legally, although you have actually paid for it with federal money." It's very difficult to get innovative ideas funded. I'll come to that in connection with other things in my laboratory, because virtually none of the innovative things in my laboratory were initially funded by NIH. They funded it *only* after we had proved that it worked.

GREENBERG: You were going to talk about rheology, but before we get into that, let me ask a question. Once you got into the microcirculation problems in organisms—

WAYLAND: I didn't start in microcirculation. I started in blood flow, the flow properties of blood—rheology of blood. Microcirculation came later.

GREENBERG: All right. What exactly is rheology?

WAYLAND: Rheology is the study of deformation and flow, and it can deal with either solids or fluids, but we usually eliminate simple Newtonian fluids—fluids like water. We usually include only those things that have odd properties, where the shear stress and shear strain are related in a complicated way.

I had felt that when people took blood out of an animal and put it in a pot and measured its flow properties, they were modifying it chemically in such a way that they weren't really looking at what we wanted to know. So, I had tried to persuade Knisely to put in some sort of a device where he would implant a tube permanently, or chronically. He could then allow the animal to recover and have the blood flow through that tube and make the measurements with the tube *in situ*. Knisely never caught on, and Frasher did. So, we tried to work out procedures for taking blood directly from the animal, measuring its flow properties as it left the animal, and then throwing it away so it wasn't put back in to disturb the physiology. We started with a system which proved to be quite an abortion. We had a young thoracic surgeon working with us there at the Cardiovascular Research Lab. I've got a block on his name—maybe it's just as well. Anyway, my suggestion was that we put in some sort of a shunt, which took the blood from one place to another. The bulk of the blood went back into the animal, but then we would take out what we wanted to measure. He suggested that we go into the descending aorta, go right into the chest, and hook in a device—a sort of a standpipe tube, which was where the flow went straight through. If we had the standpipe coming out, we would then take the blood out of it. It was a very, very difficult operation, and Frasher became very expert at handling it. But, of course, the animal had to be on a heart-lung machine during the operation. I remember one day when the heart-lung machine let go, and poor Wally got soaked with about five liters of blood. It just was such a traumatic operation, had to be acute, that we just couldn't make it work.

Then we moved over to the original idea, which was to put a shunt between a carotid artery and the jugular vein. Now, it happens that in the brain of a dog—I don't know whether a dog is just stupid or whether he has just so much blood flow there and so much collateral flow—you can completely interrupt, you can plug off, one carotid artery and you won't affect his mental capacity; his brain still functions quite adequately. So, what Wally did was develop a carotid-jugular shunt, where he came into the carotid

artery, tied it off so that the blood came out of the carotid artery through a plastic tube which was carried across the neck of the animal and fed back into the opposite jugular vein. And it was designed so it could be separated, and then we could hook our equipment into this separated part. So, we had this nice, fast flow of blood coming across, and it took some careful hydrodynamic design to allow the blood to get into this tube without stirring up eddies and starting clots and God knows what. It took several months of development. But eventually this shunt was developed.

And the shunt has been used for several purposes. Others have used it to get access to blood, to study clotting phenomena. When we wanted to make a measurement, then, we would clamp off the feed to clamp off the two tubes connected into a special tube, which allowed the blood to continue in a nice, smooth path. And then at right angles, we had the fine tubing, with which we wanted to make the measurement. We worked out a technique where we took a nylon fiber—which, incidentally, gets stronger if you pull it and make it smaller. So, we would put that nylon fiber across and lay it in. Then we would make a tiny hole in the device, the mold for the casting, and then cast the tube of the right diameter with the nylon fiber going across. Then the nylon fiber would be pulled until it reduced in size enough so that it could be pulled out. Then, the mold would be pulled out, which was a metal tube. Then we had this fine tube; we used 200 microns and 80 microns for the measuring tube. Where the thing came out the other side, we cast in a little stainless-steel tube, which was then used to measure the local pressure; we hooked it to a pressure gauge. So, the major blood flow was coming across this way. For the blood that came out of this thin tube, we had the pressure measured just upstream, just across from it. Then we let it bleed into a chamber that contained saline. We would measure the rate of flow by looking at the rise, the displacement, of the saline up a thermometer tube; we could measure how far it rose and get the rate of flow.

We got some interesting data on it, because we would then modify the blood in the animal as much as we could and still keep the animal alive—take out the platelets by means of glass wool. We would change the fibrinogen content by dropping out the fibrinogen and seeing what the effect was. But we found that the strong pulse meant that we couldn't get a very small pressure difference. So, then, we still have an argument: Who thought of it first? It doesn't matter. Wally Frasher and Herb [Herbert J.]

Meiselman, who spent five years here with him, a chem engineer graduate from MIT, and I worked out a system where we took the upstream pressure and used it to drive a servomechanism. This modified the downstream pressure, so it essentially followed the upstream pressure. The pressure difference across this small tube remained virtually constant. Then we put in a withdrawal syringe and withdrew fluid at a known rate from the chamber. Of course, the pressure on that side would go down, and we would measure the pressure drop. There was only a very small modulation due to the pulse, so we could get the pressure drop very small. If we tried to make the pressure drop too small, we'd often get regurgitation; then the saline would go back into the bloodstream and raise hell. So, with this, we made a whole series of measurements by what we called outflow viscometry. This was our entry into the field of measuring the viscometric properties of blood as it came out of the animal.

The basic idea of outflow viscometry—as distinct from having a tube implanted and just measuring the pressure across it—came from a paper that was given by Eiichi Fukada, a Japanese scientist, who is now executive director of the Institute of Physical and Chemical Research in Tokyo. He has become a very close friend of ours. He had built one where he was going to try to use it to measure the viscosity of human blood by just putting in a hypodermic needle. Then he had essentially a bulb which had been squeezed, and you let go of it, and the blood came squirting out. But he had no constancy of flow rate. He presented this at a meeting in 1963 at Brown University. So, Eiichi and I discussed this and decided that they needed a better way of measuring. That sort of gave us the lead which led us to this other system when the implant in the descending aorta didn't work. The whole thing worked out quite well, and we had quite a long series of papers on that.

In the meantime, Sid Sobin arranged to have various people spend time here in this lab, thinking that they'd like to spend time at Caltech, and rubbing shoulders would be useful. Merrill Spencer was the first. Merrill was more interested in getting a new job, which he eventually did—and did practically nothing the year he was here. The next year, it was Benjamin Zweifach, who is sort of one of the grand old men of microcirculation. He got Marcos Intaglietta very interested, because up to that time, Marcos had been working more on the engineering side of things. Marcos got very

interested both in the improvement of the pressure gauges for measuring pressure in micro-vessels, and also, in measuring transport across the microvascular wall. Back in the thirties, Eugene Landis had worked out a technique; what he did was to occlude a capillary, in his case a capillary in a frog mesentery. And then he would watch the movement of the red cells. If the fluid was leaking out, the red cells would be moving toward the occluding probe; if the fluid was coming in, the red cells would spread apart and move away. But his work was terribly crude. Zweifach got interested in it, but he's not a very good physicist; he's a good physiologist.

Marcos got interested and helped him refine the techniques so that they were able to get much more accurate data than ever had been gotten with the method. It became sort of a standard method for measuring the movement of fluid out of micro-vessels. Also, they got interested in measuring the mechanical properties of soft-connective tissue. And Marcos remodeled his old streaming-birefringence equipment to put a torsion on a thin piece of tissue and measure its mechanical properties. They worked with Bert [Yuan-Cheng] Fung on that. The three of them went down to La Jolla one day, got to talking with Sol Penner, who was head of AMES, the Aeronautical and Mechanical Engineering Sciences Department at the University of San Diego. Sol said, "Well, why don't you come down here?" They got very interested in the possibility of going *en bloc* to UCSD.

GREENBERG: Is what they were doing what you refer to as bioengineering research?

WAYLAND: That's right.

GREENBERG: How does one distinguish that from your first research on the blood flow?

WAYLAND: Well, it's interesting that at that time, biophysicists in this country were primarily interested in molecular biology. And although, to me, what I was doing was perfectly in biophysics and what they were doing was perfectly in biophysics, the whole business of blood rheology, in particular, and eventually the mechanics of tissue, had been taken up more and more by people from the engineering science background. The chemical engineers were the ones who moved into the blood rheology with a bang.

Meiselman for example, worked with Ed [Edward Wilson] Merrill at MIT, who developed the so-called GDM Viscometer, which is still the best viscometer of its type for measuring small differences of viscosity with a guard ring that gets rid of surface denaturation. But chem engineers had moved into it pretty strongly, so it got the engineering flavor, from that point of view. And the physicists thought this sort of thing was sort of *infra dig!* If they weren't working with DNA, they didn't think they were working with anything, just as in our so-called physiology and biophysics over here. [Robert L.] Sinsheimer was supposedly a biophysicist, but what he was interested in was perfectly good biophysics, but it was a different type. And there was no interest in this type of biophysics among the professional biophysicists, as far as I can find out. So, it got into bioengineering. Though in Europe, people doing the same thing would call themselves biophysicists. So, it's just a matter of semantics.

Well, anyway, they decided they'd go down there. Zweifach had to go back to NYU for a year, because he had been on sabbatical, and then he came back out. Intaglietta stayed here another year and then went on down. And Fung left and went down [1966]. I tried to persuade Fung to join forces with me, but he felt that if he was going to make a major change, he ought to change institutions, and I think he was right. [GALCIT director] Hans Liepmann was so violently opposed to any work on living animals that it was just terribly embarrassing for Fung to try to get involved with animal work and still maintain his ties in aeronautics. I mean, Liepmann is just so emotionally involved in this. He's as bad as these people who got involved with releasing these dogs recently. He may have changed in the meantime, I don't know. I stay away from him, because I know that he doesn't approve of the sort of thing I do. But he's been very emotional about it. And it was very funny when he had to bring greetings from one of my friends in Germany who's been involved in this thing. [Laughter] But, Fung just didn't feel that he could change that drastically within the Caltech milieu. In principle he could have, but emotionally it would have been too difficult. I can appreciate his problem. So, we just kept going here.

The year after Zweifach was here was the year Paul Johnson came. He was interested in control, particularly in what we call myogenic control, the effect of pressure on control—at what pressure level do the muscle cells control the flow of blood, and

when do you get out of that pressure range; where is getting out of it—when you have too much pressure or too little, too much stress on layers, too much strain on the muscle cells? We thought at the time we were dealing with sphincters; now we think that sphincters aren't these nice little valves; they're spread out over a distance in this particular tissue, although on the bat wing they have beautiful little valves. So, it depends on the tissue completely.

But at the time, [Curt] Wiederhelm had developed a technique for measuring pressure in micro-vessels. It was based on the technique that Gene Landis had developed. What Landis had done was to take a micropipette, put a dye in it, and then put it into the vessel. He would look at where the meniscus was. He would change the pressure upstream till he could get the meniscus at a given position, and then by measuring the pressure, he had a measure of the pressure in the micro-vessel. Wiederhelm did this electronically by putting a saline solution in the micropipette which had a different conductivity than the blood plasma. So, then he put this in the arm of a bridge and maintained the resistance of that arm of the bridge by modifying the pressure and then measuring the pressure it took to keep it at the fixed position. He could follow fairly high frequencies this way. And Marcos Intaglietta, who knew a lot more about electronics than Wiederhelm did, made some improvement on that. In fact, the commercial unit that is sold all over the world now is an outgrowth of Marcos's modification.

Anyway, we knew that there was a means of measuring pressure. Johnson had worked out a flying spot method for taking a cathode ray spot, which he then de-magnified and let the de-magnified image move across the blood vessel. By looking at the absorption pattern, he could get a measure of the diameter. And this went by twenty-five times a second. So, he got good dynamic information on the response at one location of the diameter. Of the major parameters that you needed to know what was going on in microcirculation, you needed pressure difference, you needed diameter, and you really needed it in more than one point. We had pressure at one point, diameter at one point, but we didn't have a good method of measuring velocity.

GREENBERG: Up to now, you're still working with blood all the time?

WAYLAND: I'm into animals now, but I'm working with blood. I'm interested in how the blood is flowing in the animal now as distinct from flowing, just the general properties. I came to the conclusion that the exciting area in blood rheology was not taking a pot of blood and measuring it. It was down where you had to worry about the deformability of the red cells and the distribution of flow. And I felt we had to get down to the living system. This has been sort of my hang-up. I want everything done in the living system that can possibly be done. And Johnson was working on control of flow, due to modification of pressure. So, the question was, what did we need to know?

There were two things we didn't know. We didn't know the velocity. So, I went back to Indianapolis and Johnson and I got together and discussed in detail what parameters we needed to be able to understand what was going on and came to the conclusion that we needed a good method of measuring velocity. Later we realized we also needed a measure of the number of red cells going by in proportion to the plasma, as well as the speed with which they went. So, I felt, just from some crude experiments I'd performed, that we needed a time-of-flight method, where you used two photometric pickups. If these are close enough together and if a light goes through and strikes any sort of photometric pickup, the light is going to be modulated. And if you have two of them close enough together, the modulation in the two will look nearly identical, but they will be shifted in time. So, then by finding out what the time shift is and knowing the magnification, you could measure the velocity.

When Johnson saw my first pictures, he was ready to throw his hands up in horror, but he decided to go ahead with it. And, of course, it worked; it has become the standard method. There have been modifications to make it usable from television instead of just putting in two probes in the microscopic field. But the basic technique has become *the* technique. We got it worked out within less than half a year, and then he spent the rest of the year using it, measuring with it. Then he went back to Indiana for a year and came back to be chairman of the Department of Physiology at the University of Arizona. And frankly, he's had as many as five dual-slit systems going at once there at Tucson. When we first presented this in 1966 at a meeting in Cambridge, Peter Gaehtgens, who was a German, got very interested. He came up afterwards and said, "Would it be possible to come and spend some time in your laboratory to learn the

techniques?" I said, "Sure, if you can find the money." So, he got money from the Max Kade Foundation and came here in 1967. After one year, I had found the money to keep him a second year. He did quite a lot of work on measuring velocities in the cat mesentery.

Begin Tape 2, Side 2

GREENBERG: Genetic engineering has a connotation of going in there and fixing things. Has the point been reached where you can use the basic knowledge that you've acquired to repair damage on microcirculatory networks caused by disease?

WAYLAND: In the sense that you speak of it, I would say no. But there has been considerable progress. For instance, it is now recognized where some of the problems arise in sickle cell disease. For example, the inability of the red cell to deform properly when it's in the reduced state is very important. So, anything that can be done to increase the flexibility is important. Recognizing the parameters that are influencing the behavior has begun to be reflected back into trying to design means of improving the flow. In this sense, I think that this sort of thing is leading to some progress.

GREENBERG: I just want to be clear about microcirculation. Microcirculation is the study of circulation within the organism.

WAYLAND: Yes. You can make models of the microcirculation. And there are people such as Joe [Joseph F.] Gross, who is at Arizona, who are trying to do theoretical modeling. But to me, the study of microcirculation is the study of the circulation at the level at which the exchange processes take place. This is where the physiologic action is. And there's been an awful lot of misconception about it; such things, for example, as the three- to five-minute brain death when people drown. The brain doesn't die in that length of time; this is asinine. What happens is that you do not get reflow. There are certain areas in which you do not get adequate reflow of the blood. If you could get the blood reflowing within an hour, I don't think you would have any mental damage. But there

are certain areas that get plugged up. Just take a simple model; take two tubes in parallel of the same diameter. The flow stops, but something gets lodged in one of them. Then if the pressure goes up slowly, all the flow will go in the low resistance vessel, while the other one will stay plugged. There's just no way you can get it started. But if you hit it with a bang, you may knock the plug loose. And this has been demonstrated beautifully in some work done with cats in Cologne—that brain death doesn't occur in three to five minutes. But other irreversible processes may take place in that length of time, and we have to learn how to reverse those processes. It may be possible to do it chemically. Certainly, the Cologne group has succeeded in doing it mechanically in some instances, so that at least they're beginning to realize that a lot of the lore is still valid lore as far as normal medical practice is concerned. It is not valid as far as physiological comprehension is concerned.

The other thing, of course, that we're trying to get at and where my work has gone more recently, is in understanding the transport processes, the things that go from the bloodstream out into the tissue, and vice versa. After all, just because the stuff's running through the pipes doesn't do any good; it's got to get into the cells that need the nourishment or pick up the junk that has to be taken out to be disposed of. And this is where the real excitement comes in. But if you don't have the flow, this is not going to happen. So, this was the direction that the work took later. At first, we were looking at the flow problems. Then we realized that until we understood what I had called the dynamic morphology—the way in which the flow actually takes place, what goes through the pipes in life—we will never understand what the real problems are and what the real function is.

GREENBERG: When did you come to this conclusion?

WAYLAND: Oh, this was certainly by 1970. I don't know when I first published the idea of dynamic morphology, but that's when we first began to hit it. And that's about the time that Frasher and I published the morphology of the cat mesentery, which, outside of the kidney, was the first real identification of a repetitive system in microcirculation, to my knowledge.

GREENBERG: Are we ready to talk about the precision animal table and intravital microscope?

WAYLAND: That was developed just about that time. In fact, it first went online in December 1968. Fairly early in our work, Frasher and I realized that it was impractical to do work with the usual microscopic setup. And we kept dreaming about some improved system. It was as early as '66 when we were thinking about it, because I remember talking about it in Iceland at the Biorheology Congress. So I finally talked to the Sloan [Alfred P. Sloan Foundation] group here at Caltech, which was administered by the provost, who at that time must have been [Robert] Bacher, and of course, Arnold Beckman [chairman, Caltech Board of Trustees], whom I've known since I first came here, and [Caltech president] Lee DuBridge. They came through with a munificent sum of \$15,000 to work on this idea of a precision animal table and intravital microscope.

GREENBERG: Do you compare DuBridge favorably with Millikan and [Harold] Brown?

WAYLAND: As far as Brown is concerned, DuBridge is just an absolute giant compared to Brown. Brown was never interested in being a Caltech president. He was here only to while away his time. I think he was almost a disaster. You have to look at people in their time. Millikan was great in the early days. Toward the end, he was somewhat weaker. I knew him quite well. I knew him not only as my thesis advisor but also as a life member of the board of trustees of the Neighborhood Church. I was president of that for a time and used to take him to the meetings. I got to know him pretty well that way. In the early days, it was hard to believe there could have been anybody who could have put this place on the map the way Millikan did. He was hard, he was tough. But at the same time, he saw what needed to be done and he did it. He was hard on people. He was willing to milk everybody for every damn thing he could get. DuBridge never had that kind of attitude. DuBridge looked at things in a far broader sense. And I'd say, the first ten years DuBridge was here, he was absolutely the greatest president probably Caltech ever had. The second ten years, I think, he began to slip a bit. I think he made some pretty bad mistakes—nothing serious. This damned library [Robert A. Millikan Memorial Library; opened 1967] is one example. And he wouldn't listen to the faculty;

the faculty screamed bloody murder about that. One reason the man who headed the faculty committee dealing with construction left Caltech was because he just felt it wasn't possible to work with DuBridge on anything like that. And now he's worth a hell of a lot more than DuBridge ever was. But that was one place where DuBridge was completely blind, no sense of aesthetics. But he certainly was a damn good president and he did a very good job for Caltech.

GREENBERG: Did DuBridge become involved with what you were doing?

WAYLAND: Well, only in a secondary way. But at least he was permissive. I would say that probably Arnold Beckman was the one who knew most about what I was doing. At least they were permissive, and they did come through with the money to get started. We had to have more money before we finished, partly because of some booboos over in Central Engineering, where somebody—when I was away for three months—spent several thousand dollars of my money without my permission. The Institute finally came through and made it up, thank God.

Frasher and I sat down and laid out the principles. Then, Bruce Rule [director of Central Engineering], who had had a lot of experience in building telescopes, sat down with the designers over there. And we went through every step of it, thinking of various alternatives such as taking Instron testing machines and moving the whole animal up and down. We finally came to the conclusion that the only thing to move was the optics. I finally hit on the idea of using infinity-corrected optics, largely because of experience I'd had during the war with some instruments that Ike Bowen had designed. When I found that they were available, it seemed to me silly to use anything else. And that allowed us to do all of our servo-ing optically; we could tie our cameras down; we could tie our animal down; the only thing we had to move was the optics.

We set criteria for the size to handle animals as big as dogs or goats or mini pigs. We still hadn't recognized the need for low light level recording, and a few things like this; that came later. But the basic framework we built on a tripod basis because I could understand tripods. Since I didn't have much experience in indeterminate structures, I was doubtful about putting four legs on it. But it did also give us better access. We made

the basic table so that it could be moved up and down, but we eventually locked the table in one position and never moved the table. From then on, the only thing that moved was the optics. So, for any future one to be built—two more have been built—I recommended they didn't even bother to move the table. Others, particularly the one at USC, were built with far better optical adjustment in mind; but, of course, they had a chance to second guess, too. And the one built for the University of Michigan had some improvements over ours, though it missed certain things because they didn't have the money—of course, costs had gone up by then.

So, actually Gaetgens was the first one to use it, for his measurements of velocity in the cat mesentery. And then Frasher and I worked on the morphology of the cat mesentery. We began to realize that it was rather peculiar, and we were able to work it out in detail. And then a group in the East did the theory on it to show that, indeed, this peculiar morphology did behave differently than the classical morphology. That had been worked out by [Robert] Chambers and Zweifach way back before the war, and everyone just assumed that all micro-beds were built like that—you had a pipe coming in and then a branch that branched and branched and then came back together again.

In the cat it doesn't work that way. You have these loops, and the loops are interconnected in peculiar ways. As a good example, one of the men from UCLA came over, Gordon Ross, with an electromagnetic flowmeter, put it on the superior mesenteric artery, measured the flow into the mesenteric region. And Peter Gaetgens put the little slits on an image of a capillary. Then we clamped down on the artery, cut the blood flow to the mesentery in two. The capillary could do any of three things. It could speed up; it could stay stationary; it could slow down.

Later, when we understood the morphology, we could understand this. There were so many parallel paths that it could be going any way; the particular local flow was only secondarily dependent on the total flow. Stop it completely and, of course, it would stop. You cut it down, and some loci might stay the same, or they might even speed up. And it was only by understanding the overall morphology, that we could understand this. Because had it been the classical morphology that Chambers and Zweifach had found in the rat mesentery, then if you clamp down on the feed, damn it all, they all had to slow down. So, this sort of opened our eyes to the fact that—others had recognized this, too—

of course there is no such thing as *a* microcirculation or *the* microcirculation. Each organ has its own, and each organ must be looked at separately in order to understand the relationship of morphology to function. But this is where I have been pushing hard—I think the first time I pushed it in public was in my Japanese tour in 1973—the great importance of recognizing the dynamic morphology. It took time and lots of people don't recognize it yet, but it's coming.

GREENBERG: In 1968, you helped found a new journal, *Microvascular Research*.

WAYLAND: Well, I didn't help found it; I was just asked to be on the editorial board. Actually, Zweifach and [George P.] Fulton were the real founders of that. They got Dave Shepro to be the active editor, and Dave still is. But I was asked to be on the editorial board from the beginning, and I was for a good many years.

GREENBERG: And the journal is aimed at fostering communication among physical, engineering, biological and clinical sciences?

WAYLAND: Ostensibly.

GREENBERG: Did it turn out to really be that kind of an interdisciplinary journal?

WAYLAND: There's less interdisciplinary in it than I'd like to see. It is very good in the basic microcirculation. I think it's still better than the so-called *Microcirculation: Experimental and Clinical* that's being put out by the European Society. But it is pretty heavily scientifically oriented. There are some more engineering-oriented things. Very little, though, partly because the engineering group has moved more over to its own journal. And they each like to publish in the journal of the societies to which they belong. To me, one of the greatest weaknesses of *Microvascular Research* is this almost complete lack of clinical things. And this is one reason the European Society decided to publish its own journal, because clinical things were mostly not accepted by *Microvascular Research*. But when it comes to the basic scientific work on microcirculatory studies, it is still the most useful single organ.

Of course, the *American Journal of Physiology* and the *Journal of Applied Physiology* and *Pflügers Archiv* in Germany still publish a certain amount in the field. But the physiology meetings, particularly the international group, give very short shrift to microcirculation. There was a great deal of complaint about the Sydney meeting this year, that there was practically nothing on microcirculation except one satellite, which was held in advance of the meeting. And there's still hope that there will be more at the Vancouver meeting in a couple years. But it doesn't take a very big place there.

GREENBERG: So, is it hard to compare the research that goes on abroad with what goes on here?

WAYLAND: No.

GREENBERG: Is it better here? Or is this country lagging behind?

WAYLAND: Well, there has been more microcirculatory research in America until recently, at least. I'm not sure whether America is going to continue to keep its lead. But still, in England, the number of groups is very small. Charing Cross Hospital is doing beautiful work. But [Peter Alexander George] Monro at Cambridge is virtually out of the picture now. And there's nothing there that I have seen that's worth worrying about. They have competencies that we ought to be exploiting.

There's a very good man at Oxford in Charles Michel. But Charles is moving to [Imperial College] London now, and I don't know whether there'll be anything at Oxford when he leaves, except a little dermatology. Terence Ryan will continue his work in skin microcirculation. But he's more of a clinician and doesn't put much time on the basic side. Charles is so 100-percent basic that he sometimes misses the breadth of the field. But it will strengthen the London group, because [Laurence J.] Smaje is in London. And Michel will now be in London, along with Geraldine Clough, who was one of Smaje's students who then worked for Michel, came back to Smaje, and is now going back to work for Michel again. She will act as liaison between the two groups; she has proved to be an excellent electron-microscopist, which has added a lot to the field. Born seems to be very strong there in the pathology side of microcirculation. So, the London area now

is building up, whereas at one time it was practically nothing. Now it is becoming a center of real strength.

There's one man in Heidelberg, the man I worked with, who is very, very good, and is doing very good work. There is another man there who's doing quite a bit of tumor microcirculation, and hopefully that will develop. He's moved from Munich and it takes time to get reestablished. That group has real potential, but two of the people don't talk to each other, which is one of the problems.

There are two or three groups in Sweden now. There's one strong group in Copenhagen. Russia lost its strongest man by death, though there is a brain microcirculationist by the name of Mchedlishvili from Georgia, who is continuing to be active. He hasn't done brilliant work, but he's done good solid work over the years. But that's not much for a big country like that.

Japan, however, is burgeoning. I would say that Japan is the country that could easily become the leading country in the world. When I went to Japan in 1973, I spent three-and-a-half months there and gave twenty-five lectures in twenty-one institutions. I was there officially for ten weeks. I stayed for three-and-a-half months because I wanted to do some traveling. There was no society of microcirculation; there was relatively little going on except odd bits here and there. In fact, in 1966, when we tried to establish a U.S.-Japan cooperative seminar to be held in '67—I wanted it on microcirculation—it was impossible to find enough Japanese who had enough clout to make that go. So, we had to have it on the biophysics and bioengineering of the peripheral circulation, including such things as venous return in the legs.

And when we had the return seminar here in 1970, it was essentially the same group. But I found scattered all over Japan—Hokkaido, Akita, Morioka, of course Tokyo, Hiroshima, Kumamoto—all the way from the northernmost to the southernmost island—puddles of interest. But nobody talked to each other. I don't know whether it was my stimulation or what, but in any event, within a year they had their own society. And today, their society is the biggest society in the world, in terms of numbers. And they are strong.

And here's where one of our interesting problems arises. We've had a commission on microcirculation that was supposedly for the International Union of

Physiological Sciences, IUPS. I worked hard to get that established when [Caltech geochemist] Harrison Brown was still involved with the UNESCO agency that runs such things. We wanted to make it an inter-union commission. Finally, it got set up within physiology. Unfortunately, the chairman, Christian Crone from Copenhagen, is so hipped upon physiology as the only science in the world, that he has refused to allow it to broaden beyond physiology. And yet, microcirculation is a true multidisciplinary activity.

GREENBERG: Is this one of the problems of interdisciplinary research that you lecture on?

WAYLAND: Absolutely. But, in Japan, some of the people who are primarily pushing microcirculatory research don't even call themselves physiologists: Norio Ohshima, bioengineering. Masaharu Tsuchiya, internal medicine. It's true that Azuma, who is very active in promoting microcirculatory research, is a physiologist. However, as dean of medicine and now as head of the board of trustees of a major university, he's not going to do much more science; he's never done a piece of microcirculatory research in his life, though he realized the importance.

GREENBERG: What is the current situation in this country? A moment ago, you didn't sound too optimistic.

WAYLAND: We have a few centers of excellence. The group at La Jolla, I feel, is in a state of transition. Fung, who is primarily in bioengineering and not microcirculation, will be having his sixty-fifth birthday party this summer; he's got five more years with the University of California. But he's not so interested in microcirculation. He hasn't even come to the latest meetings. Zweifach has finally retired. The strongest young man there is Geert Schmid-Schönbein. And I just haven't had a chance to see how he's going to develop. Intaglietta is still going; in fact, Intaglietta is getting back to work. He had allowed himself to get sucked off onto too much work on buying and selling real estate. But he's getting back at it. I don't know what's going to happen; his wife died very suddenly, and I gather he just absolutely doesn't know where he's going. And until he

gets his feet back on the ground, it's going to be hard to say. They have the potential of being one of the great establishments.

GREENBERG: In 1978, you transferred your intravital microscope to the University of Missouri Medical School. Why?

WAYLAND: Because there was no possibility of carrying it on here that I could see. I don't know whether you know the whole story. The story's an interesting one. I felt that it was wrong to see the thing cannibalized and torn apart: "A" wanted the television camera, somebody else wanted something else; and somebody else something else; it would have been cannibalized in ten minutes had I allowed it.

GREENBERG: This would have dismantled the apparatus?

WAYLAND: Unless there's somebody to carry on, and there was nobody to carry on. So, I sat down and talked it over with [David W.] Morrisroe [Caltech vice president for business and finance], and he agreed with me. It was absolutely silly to see this thing torn apart; too much had gone into it. We were in an interregnum then, because about that time our president had gone, so [Robert F.] Christy was acting president. Christy agreed. He felt that anyone who took the equipment ought to be prepared to put up enough money to prove that they really wanted it. He was more interested in their showing faith than he was in the amount of money that came back to Caltech. After considerable discussion largely with Morrisroe, but with Christy's blessing and with the blessing of my division chairman—or at least the permission; I don't know that there was much blessing there—it was decided he was obviously not interested in carrying it on here.

GREENBERG: Is Caltech particularly ruthless in the way that it phases out its research programs?

WAYLAND: I've seen this happen on several occasions, yes. Other times they carry on because they've found somebody to carry on. But I've seen this happen. I think a

beautiful example is when Dan Campbell died. He was, in my opinion, a far greater immunologist or immune chemist than anybody we have on the faculty today, in spite of the fact they're trying to make that a big thing. He was a great man. Today we have some good people. His assistant, a girl, they just threw out. She is actually better than anybody they've got today, too, in my opinion. But they couldn't stand the competition. There's a lot of that around here; it's true of every institution. But I have never forgiven our biologists for the way they treated Campbell. I think it was just absolutely disgraceful. And you'll find a lot of other people around here who agree with me. I'd better not give you names because it might be embarrassing to them. But I'm not afraid to say my say on it.

We finally got the agreement that we could let it go, that it ought to go unbought. Now, I actually sent letters to the chairmen. [John D.] Baldeschwieler was chairman of chemistry; I think [Norman H.] Horowitz was then chairman of biology, and Bob [Robert H.] Cannon was our chairman in engineering. I felt those were the only places where there would be any sense in trying to put it, because physics wouldn't be interested; it would be more likely chemistry or biology. Well, Horowitz was in sort of an interim basis and didn't want to make a long-term commitment. And this was about the time Baldeschwieler was in his struggle. Actually, later it turned out that [chemistry division chairman] Harry Gray would have loved to have had the equipment, but by that time, it was too late. And so, I got definite refusals on the part of the people here on campus. So then, I got the agreement that we could put this out for bid.

I sent out letters to ten institutions that I thought could really effectively use the equipment. Out of those, four came back and said, "We are *really* interested." That was the University of Washington; a chap who works between the VA and UCSD; Johnson, at Arizona; and [Patrick D.] Harris, who was actually here physically working with me on a year's sabbatical—those four agreed. We set a price; I think it was only \$25,000, which was the actual money that had been given to me from Institute funds. All the rest of the money which had gone into the development of my laboratory had come from private agencies. The Sloan money we figured as Institute money. Otherwise, it was various foundations or NIH that put up the money. So, any of that money that was given to Caltech strictly for this biomedical research, they felt should be left as part of the act.

But the money that could come back to Caltech and quite legitimately then be recycled into other activities to give them a stimulus, as they gave me a stimulus, should come back and be put back into a fund where the money could be used.

So, we set this price of \$25,000. We demanded first of all, administrative acceptance: Put up the 25K, show a willingness to establish the laboratory, give them the space—take care of the moving costs, because we weren't going to put up any moving costs—the reestablishment costs, and at least a decent length of time when they would support it. And these four institutions came through with that agreement.

That was the administrative agreement. Then there had to be a proposal, much like an NIH proposal, which was for use of the equipment—what would they do with it? So, each of the four PIs [principal investigators] came in with a proposal. We then had a committee to make the decision as to which of the four institutions would get it. The committee consisted of Jean-Paul Revel, from our biology division; Rachmiel Levine, who at the time was still executive medical director of the City of Hope and with whom I worked quite closely and who knew my work; and Wally Frasher, who at that time was a vice president and head of research for the American Heart Association. So, I thought we had a group of considerable stature to make the decision.

It was a very close call, to be frank. Washington and La Jolla were out very quick. But between Johnson and Harris, it was a mighty close call. In some ways, I think they felt that Johnson was a more established investigator and had already proven himself. On the other hand, Harris was coming up fast; he knew the equipment because he had been here for a year, and his leading technician had been with him the entire time. And Johnson had a very fine going laboratory. And Harris was without anything comparable. So, I think the decision was pushed his direction because they felt it would make a much bigger impact. And I think the decision was right. Because it has not really slowed Johnson down; and it has certainly given Harris a tremendous boost.

GREENBERG: Things have worked out well.

WAYLAND: Very well. And on top of that, they worked out even better than we had anticipated, because he has now moved the whole laboratory with all of his senior staff to

Louisville, Kentucky, and the setup there is really something. I just had a recent report from Frasher, who was there as a Heart Association representative, because Pat Harris is chairman of the Heart Association's Research Committee for Kentucky. And he said he was very impressed with what he saw.

GREENBERG: I'd like to jump back to Japan for a minute. Amnon Yariv [Caltech professor of applied physics] recently talked about how the Japanese had taken his micro-laser chip and run with it, while American companies haven't paid any attention to it at all. I was wondering if you empathize with this sort of problem, because the Japanese, as you say, have caught up, if they haven't gone ahead.

WAYLAND: Well, in some areas they're way ahead. I was very interested in the comment of one of our alumni. I gave a talk to our Alumni Association when I was there this last spring. And one of the engineers there said that he went there with the illusion that the Japanese were just copying. It isn't true. They are indeed in many areas truly on the forefront. And certainly, this has been my impression. In my area, it is hard to say. I am not in an area where "there's money in them thar hills," so that I don't honestly know yet what's going to happen.

You're probably not familiar with this latest idea of mine—the concept of the Intravital Observatory. I feel very definitely that there is a serious need for several centers of excellence. And I use as my model the astronomical observatories, because, after all, what I'm talking about is optical observation—to look at the internal world with microscopes, instead of looking at the external world with telescopes. But the actual recording and data processing are not very different. And they require very high sophistication to do the work properly. I see the crude attempts, like this business I'm involved with at Tucson right now. If we had a really well-instrumented, well-developed center of excellence someplace in this country—where people could go and use the best of contemporary equipment to try out some of these ideas, to find out whether they really can get the kind of information they want—if it's a relatively small project, they could do the whole darn thing right there. If it turns out to be a long-term project, then extract from the tools that they're working with those specialized tools they need and get help

right on the spot to build up what they need for their specific things. Just as, here, I helped [Richard J.] Bing develop a simplified intravital microscope, which he's been using ever since in the Huntington Medical Research Institutes. It's much simpler than the system we had, but quite adequate for what he wanted to do. I think this is the kind of thing that we need desperately.

GREENBERG: Is there any place that's a suitable candidate at the moment?

WAYLAND: Well, it's still very much in the iffy stage. In Germany, there is one man in Karlsruhe who is very interested. He sees the picture with a global view. Most of the German scientists are so involved with their personal careers that they would rather make a name for themselves in an almost trivial area that is theirs than look at the global picture and how they can help move biomedical science forward. This one man in Germany is the only German I have found yet. He does seem to have the right view; he sat down with me and said, "Now, who are the people we ought to bring together? What are the disciplines that would be important for it from the medical point of view? Now, what are the support funds that we would have to have?" We wrote down these lists, because I think we'll have to call a get-together of this kind of group. And he's looking at it in the broad biomedical sense.

In Sweden, there is also a man, a professor of experimental surgery—an American, by the way, who has become very much Swedish—I don't know whether he's a citizen there yet or not—who is actually calling a meeting at the time of the World Congress for Microcirculation in Oxford next year, to discuss the possibilities of such a thing for microcirculatory studies. The Commission on Microcirculation doesn't have enough nerve to carry it out. They discussed it apparently at Sydney. I was on the commission, but I rotated off this year. I didn't get to Sydney, so I wasn't there for the discussion, but they obviously lost their nerve and decided it was too big a thing for them to tackle. Well, that's it for them. I think they're silly. I think they would be a good group to go after it. On the other hand, I would like it broader than microcirculation.

In this country, I'm still trying to persuade Paul Johnson at Tucson to get seriously interested in it, but he's too involved in his administrative duties. And you've

got to have an individual. I'm too old; if I were ten years younger, I'd be fighting for it to get it going personally, but I'm too old to take it on. I'm willing to help; I just can't take on an administrative responsibility. You've got to have the individual.

Also, at Louisville, there's a possibility in terms of Pat Harris—Harris himself, probably not. The problem is, you have to get somebody who's already established who's willing to say, "All right, I'm well enough established; I can give up five years here and see if this new idea will fly." If they're too young, there's too great a danger. In our dog-eat-dog, publish-or-perish climate, it is just too dangerous.

Begin Tape 3, Side 1

WAYLAND: Well, I still have hopes for Japan, partly because of their high centralization of everything they do. I sort of have my finger on an individual who I think would indeed be good; in fact, I know a couple of individuals who would be good. And I think they would be interested. But they're a little afraid. They've got to see some support from higher up, and I'm still waiting to hear from the higher-ups. In fact, I have been asked to prepare a more careful analysis, which I've got to get off within the next two weeks. Professor Takagi—an old friend of mine, in fact he was my co-sponsor of this meeting in 1967 and has moved his way up from professor of physiology to dean of medicine to president of a university and is now a member of the House of Councilors in the Diet—is quite interested, and I think that he will be a very useful tie. He's asked me to prepare further information, which I have to get to him. I didn't want to swamp him with it until after the election. But he wouldn't have been up for election because it was only the lower house that was up for election—but still, he would be involved in trying to get his candidates elected. I don't really know where he stands in the [Japanese prime minister Yasuhiro] Nakasone business. But anyway, he's interested, and he is a very good physiologist, and a man of real breadth.

Also, the former dean of medicine at Shinshu University in Matsumoto, who was the one who brought me to Japan in '73, seems to be interested. But again, he's just taken over a new job as head of a university in Tokyo. And whether he will have the

freedom, the time to do anything effective, I don't know. Neither of those people, of course, would I consider as possible heads of such an agency.

But Norio Ohshima, who's professor of bioengineering at Tsukuba, would, in my opinion, be first rate. And I think he could be talked into it if he saw that he had the backing from people high enough up. I didn't get a chance to talk to his boss, the associate dean, Professor [Motokazu] Hori, who has been really the man who has brought Ohshima along. Ohshima spent some time here, and was, by all odds, the most effective of the Japanese who came here in spite of the fact he was here less than two months. He got more in those two months than the others did in two years.

The other possibility is Professor Koyama, who is from Hokkaido, who I think does have the breadth of view. I think that Ohshima would be better; he's younger, he's more vigorous. He has a better engineering background, which I think would give him a stronger position in terms of the instrumentation side, which is terribly important.

Another man, who is deeply interested, is Eiichi Fukada, who I mentioned earlier in connection with the outflow viscometry, but as an executive director of the Institute of Physical and Chemical Research, he's over his ears in trying to get a laboratory on recombinant DNA established at Tsukuba. However, he was interested enough to go with me to visit some of the firms when I was in Japan that might be useful in providing tools for such an institution. He's very interested in it, but I don't know whether he can give the time. And also, he's about to retire as executive medical director, and whether they will ask him to stay another three years, I don't know. I haven't heard from him; all we had at Christmas was just the usual Christmas card and no news. So, I'm still sitting on pins and needles. As far as all of these places are concerned, it's still very much up in the air. I still think it's something that has a very real possibility.

I would like to see this thing happen in more than one country, because I think it's important. I think these things could be complementary, just as CERN and Fermilab are complementary, not identical. And there's need for this sort of thing, seriously. In image analysis, for example, we did a certain amount of 3-D reconstruction based on electron-microscope pictures here in this lab. Except for continuation of that work by [Frank R.] Galey, who did the work in this lab using some of the algorithms that were worked out by [Gilbert] McCann and [M. J.] Shantz, I don't know of anybody in the real physiology

end. In neurophysiology, yes, they've been doing some work on reconstructing neurons. I don't know of anybody in the sort of end I'm working in who is taking advantage of the computer reconstruction, except for Galey.

[Michael] Steinhausen in Heidelberg had a student do a beautiful reconstruction of the micro-vessels in the glomerulus of a kidney. He got one picture for hours and hours and hours of work! If that data had all been put into the computer, not only could they have gotten all the orientations, but they could have gone in and said, "Well, I want to know what the surface area of this particular substructure is," or what the length of all of these tubes of a given diameter are. The data would have all been there; it's just a matter of programming and punching the buttons. And even the programs exist for that morphology.

GREENBERG: Is there anything in particular that stands as an obstacle in the way of further expansion of research in microcirculation?

WAYLAND: Money!

GREENBERG: OK. But, I mean, it's not a glamour field.

WAYLAND: That's right, it is not a glamour field, and yet it should be. Because, after all, there is absolutely no organ that we are dependent upon to stay alive that isn't dependent upon proper operation of its microcirculation to keep it going. Diseases enter the tissue through the bloodstream, and they get out into the tissue proper largely through the microcirculation. Cancer is transmitted through the circulation, but it doesn't go out into the tissue from the big vessels; it goes out into the tissue in the microcirculation.

GREENBERG: As a result of your experience, you must have some very definite opinions about the funding process—how decisions are made, what gets money and what doesn't. Would you care to talk about that? Can you isolate Caltech as a problem in this regard, or is it a much larger one that goes beyond the boundaries of the campus?

WAYLAND: Oh, much beyond the boundaries of the campus! This is an international problem. I've had a hard time pinning this down to my satisfaction. For instance, in Germany, to a large extent they get long-term support with considerable freedom, but they don't work together. You do find a few major institutions, like the Max Planck Institutes. This is why I've gotten so excited about the observatory idea. The Max Planck Institute for Systems Physiology at Dortmund has had as its director one of the leading men in the development of techniques. And he's developed some beautiful stuff over the years. And very little of that has been applied to real physiological problems. He's had one woman working on the brain, who has done some very good work. He's had another young man who is more or less his number-two man, who for the last three years has finally gotten an institute of his own at Erlangen—Manfred Kessler, who is beginning to apply these techniques, these tools, to the heart. In no cases are they using microscopy, and Kessler realizes the need for this and is very anxious to get into it. He says he's got the money, and he wants me to come over and help him, but he's got to show me the money on the barrelhead before I'm going to spend my money to go to Germany to work with him. It was fine when I had a von Humboldt Award, but it's no good when it's entirely out of my own pocket.

The Max Planck Institutes give their money because there is a person they think is first rate. When he retires, if they don't find somebody, they feel can carry on with the caliber that he has carried on, they will close it down completely. And they'll put the money in an institute doing quite other things.

GREENBERG: Well, it's a little bit like here.

WAYLAND: Yes, in that sense, it's very much like this. I see the money that is spent in Germany going into equipment that is bought in good faith and probably used in good faith for a short period of time, and maybe even used very effectively for a short period of time. But at the end of that time, the principal investigator gets interested in something else, so he puts it on the shelf and that's where it sits until it disintegrates. It is impossible to get it moved to another institution in most cases. There's a beautiful intravital microscope sitting in Giessen, in the department of anatomy; the chap has

absolutely no interest in it, and it's quite obvious he'll never use it. But it belongs to him; he won't let it go. This compartmentalization, jealousy.

And of course, the German promotion system is really pretty atrocious. No one can achieve directorship of a laboratory within his own institution; he's got to move. This is true of [Konrad] Messmer, for example. He was in Munich; they were doing beautiful work in experimental surgery, one of the great institutions in Germany and had some good microcirculation. Messmer could never become director as long as he stayed in Munich. So, he accepted a post in Heidelberg. He's built up a pretty good lab, but they haven't come through with all the things they've promised. The economy is tough in Germany, too, right now; worse than it is here for the moment. It's about like it was here in the beginning of the Reagan era. There, it's just a lack of money; here, it's a lack of foresight. But things have eased up a little here, I understand. There, they're getting tighter rather than looser, not through lack of desire but because they just don't have it. But you just don't get people working together. They're so right; they'd rather do their own little thing in a little area than see biomedical science move forward. And this is what I feel is so sad; and I feel, to some extent, that's true here in this country.

Having sat on an NIH panel—I sat on the cardiovascular-renal study section of what was then the Heart and Lung Institute, now the Heart, Lung, and Blood Institute—we handled primarily those things that involved the relationship of the cardiovascular system with the kidney, although we had some things that were strictly cardiovascular; there was the usual tendency to put the money where you felt the bet was solid. When money is short, it's a great temptation to put the money where you feel there is going to be a real payoff, even though occasionally we would give a pretty good priority to something we felt was a gamble. Because, damn it all, if somebody doesn't take a gamble once in a while, we're never going to move forward. But we need longer-term commitments, and how to get it without letting people, you know, dig themselves into a hole and say, "Well, I got us in here, why should I rock the boat?" as it were. There's no easy solution to it when there isn't quite a lot of money. But the thing that kept my place alive and allowed us to do, I think, a few unique things here, was because of permissiveness on the part of my chief—the fact that it was all right to be a bit of a prima donna doing your own thing, as long as you didn't rock the boat too much.

GREENBERG: Who are you talking about when you say your chief?

WAYLAND: Well, in my case, the chairman of my division, because after all, if he hadn't approved of it, I would have been out. Also, I got no money out of it. As long as Fred Lindvall was here, I got money. But after Lindvall left, I got no money out of the division. I got space.

GREENBERG: Why was that?

WAYLAND: Because [Francis] Clauser wasn't interested in biomedical things in any deep way. He was interested in a secondary way. Clauser was supportive, but not deeply interested. I don't know that Lindvall was deeply interested, but at least he had enough faith in what I was doing to allow me to go in my own direction. But when I went to get the money for the intravital scope, I got support from the top brass. I really think it was largely [Arnold] Beckman who pushed it. Of course, I never had any major funding requirements through [Robert] Christy, but his ignorance of biology was so utterly profound that it wasn't even funny. In fact, from where I sat, he was a bit of a disaster.

Of course, I still think back to [Earnest] Watson, who was a great man.

GREENBERG: A hard act to follow.

WAYLAND: At least Bacher had breadth, which I never saw in Christy, frankly.

GREENBERG: Has the Institute, over the long haul, lost some of its breadth? Are there big differences between the way it is now and the way it was in the old days?

WAYLAND: I don't think so, if you really look at it in a broad way. And being small, I can see why they had to develop certain specific areas and not everything. So that part makes a certain amount of sense.

GREENBERG: What about interdepartmental collegiality?

WAYLAND: From my point of view, I felt a great deal more of it when I first came here than I did later. I empathized with people in other departments—of course, I was close to many people in humanities for a long time, too. But that was on a personal basis. From the scientific point of view, Jerry Vinograd and Dan Campbell were towers. I think, today the scientific community as a whole recognizes them as two of the greatest men they've ever had at Caltech. It took a long time for Jerry to be recognized, but his work on the circular DNA was absolutely outstanding. And there is still a Dan Campbell Club that meets at the FASEB [Federation of American Societies for Experimental Biology] every year or two, essentially to worship in Campbell's footsteps. But he's anathema as far as our biology division is concerned—not as far as chemistry is concerned.

We have another great man in hemoglobin, Walt [Walter A.] Schroeder, who's truly a great scientist. I think he's tolerated and not backed at all by the biology division, though I think he's recognized by the chemists as a man of great competence. Of course, again, these people who were Pauling protégés found themselves in a bad spot when Pauling left. Some of them were able to hang on.

GREENBERG: What about your division, engineering? What kinds of changes has it undergone over the years, with changes of administration, etc.?

WAYLAND: Well, of course, Lindvall was very permissive. I felt that he let down the division in one sense, in that he was preaching as president of the American Society for Engineering Education. He was preaching the glories of engineering science for a long time, and yet he never did a thing to permit it to be advanced here at Caltech. Had there been a strong wave, he would have gone along with it, I'm sure. But he didn't want to rock the boat. I felt very beholden to him for the freedom he gave me, but I felt that engineering science could have been pushed. I think applied physics was a great abortion; I think that should have been engineering science instead of applied physics. And I think it had more meaning and could have established a prototype for engineering science throughout the United States. As it was, it's just another group. I'm not complaining about the quality of what they're doing. They're doing good work now. But I think it was silly to allow materials science and all these little puddles to be built

up, when they could have been carried under a single umbrella and given all greater strength. But Lindvall wouldn't push it.

Then, when Clauser came, he was so in love with the picture of Caltech as he remembered it as a graduate student under Kármán that he tried to re-create that picture, which never could have been done anyway. We can never re-create the past. Also, he got here at a very difficult time financially. He and Christy didn't get along worth a damn. So, the whole thing led to a lot of friction. He was always supportive of my research; I never had any problems in getting my proposals through the front office, and I always had all the space I needed. But I never felt I had real backing. I came here to teach engineering mathematics; by the time he came here, they'd already developed applied mathematics. He felt applied mathematics ought to have the courses I was teaching, so he just took them away from me, like that.

GREENBERG: Were you involved at all with the birth of the applied mathematics department in the sixties?

WAYLAND: Not at all! Lindvall brought me to teach applied mathematics. I came with a very high recommendation from the people in mathematics. But Clauser didn't realize this. I never even bothered to ask, because by the time he decided to take the engineering math out of my hands and put it into applied mathematics, I was so deeply involved in my biological work I was perfectly willing to say, "The hell with it," even though I had put a lot of time in it.

I came here actually intending to be a teacher. I did not intend to get involved in research. I like teaching. I had a good reputation as a teacher earlier; and the first few years I was here, I was always among the top people in the student evaluation. But it soon became clear that if I did not carry on a major research program, I would never get beyond associate professor. I refused to come without an offer as associate professor with tenure—I was in enough of a bargaining position that I wouldn't come without it. And so, they came through with it. So at least I had my tenure. What happened was that I got so interested in research that as far as I was concerned, to hell with teaching.

At long last, I think I'm going to have a chance to teach something that I really like to teach, but it's not going to be here. It looks pretty likely that I'll be a distinguished professor at the University of Delaware in 1985 and will have a chance to teach a really exciting course in organ microcirculation, along with carrying on some exciting research.

GREENBERG: Does that mean you're going to relocate?

WAYLAND: Oh, it'll just be for six months. After all, when you're seventy-five, as I will be by then, you don't relocate. But I think it can be very exciting. It was, at least, flattering to be asked. I'm waiting for the official letter from the dean, but it sounds as if it's going to come through.

Of course, when [Bob] Cannon came, we were all quite excited. But his problem was the same problem that Harold Brown had—he didn't take Caltech seriously for the first two-and-a-half years. He was away more than he was here. By the time he came back, things were in such bad shape that had he not resigned, there were at least twenty or thirty of us prepared to sign a petition asking the administration to throw him out. He just had not taken hold of the problems; things had just gone from bad to worse.

And [Roy W.] Gould doesn't even know what engineering is. I think he's a very good applied scientist. I think applied science is developing beautifully, but he doesn't really understand engineering. And I'm afraid our president [Marvin L. (Murph) Goldberger] doesn't understand engineering; and I know damn well our provost [Rochus E. (Robbie) Vogt] doesn't. So, I don't know that we have anybody who really understands what engineering is. Now, this may seem strange for a physicist to say. After all, I had to live with day-to-day engineering problems from 1941 until the end of 1948. I know what engineering is and I know that there's a difference between engineering and engineering science. There's even a difference between engineering science and applied science as thought of here; and there's a difference between that and pure science.

GREENBERG: What about GALCIT? You mentioned that [Hans] Liepmann takes a dim view of working with living things. Does he understand what engineering is?

WAYLAND: I think he must. Of course, I felt that, as my daughter often put it, when Francis reestablished the director of GALCIT, he filled a much-needed gap, if you catch what I'm saying. Lindvall did his best to clear that job and leave it, and kill it, and it should have been left as a gap. Caltech should be brought closer together. Aeronautics has always been too much of a group; they get together and they make decisions. I haven't sat in on engineering division meetings for quite a while, thank God. They would get together, and they would present a united front. They could walk over all the other groups that tried to run in a democratic fashion. And I felt that it was a mistake to reestablish [GALCIT] then, as a tight little entity.

GREENBERG: Well, it will be interesting to see what happens when Liepmann retires at the end of this year.

WAYLAND: Is he retiring this year?

GREENBERG: No, next year. There may be changes.

WAYLAND: There could be. I think there should be. Well, Caltech has changed its direction. As much as one likes to see one's own work continue, I don't feel badly that my work doesn't continue at Caltech. I would feel badly if it didn't continue; but I see it continuing in a way that it never could have continued at Caltech, considering the philosophy here at Caltech. It's true that there's been more of a leaning toward medicine with the new directions in biology. And I'm hoping that my prejudices—again, about some of the individuals there—are just that, prejudices. But I do hope that they do achieve something strong. I've seen some very great men here, from Thomas Hunt Morgan to George Beadle to Max Delbrück and Roger Sperry. I think Roger Sperry is one of the greatest of the lot. I don't know how well he's received by the people over there, but I think he is one of the truly great people we've ever had on the campus.

GREENBERG: Let me just go back and get a couple of things here for the record. Charles Sadron, in whose lab you worked in Strasbourg, was here in the thirties as a postdoc in

GALCIT working on the streaming birefringence that you later worked on in his lab; he got his start here. And wasn't Arnold Kuethe, your friend, a Kármán student?

WAYLAND: Yes, he was. And then he went to the University of Michigan. I guess he stayed there until he retired. The last time I saw him, I guess, was sometime in the seventies, because the professor of anatomy there, Johannes Rhodin, built an intravital microscope based on the scope that Wally Frasher and I had designed. I went back at the time they dedicated it and found that they were dedicating the laboratory to me. But Johannes had invited a lot of people to come to my lecture and to the dedication in my honor.

GREENBERG: You dropped out of engineering?

WAYLAND: I never really was in engineering. I was trained in physics and mathematics. And the only engineering training I got was accidental. During the war, I was forced to do a certain amount of engineering. I did a lot of electrical design when I worked for the Navy. Then, when I came here, I was working on the torpedo project. There, I was working more on the theoretical aspects of water entry and forces than I was on the structural side.

GREENBERG: Did you pay attention to this problem at the national level, and the efforts to do something about it, like von Kármán and GALCIT?

WAYLAND: Well, I didn't get involved in anything at the national level until I really got into microcirculation. And there, it's been almost more at the international than the national level.

GREENBERG: Yes, and twenty years later, or something like that.

WAYLAND: I had very little contact, except, of course, when we worked with the Navy department, they put us all over the country. It wasn't in any pioneering sense.

GREENBERG: I bring this up because when émigré engineers like von Kármán and [Stephen] Timoshenko first came to this country, they were appalled by the state of engineering they found, for exactly the reasons that you point out. What they found was a recipe-book kind of engineering.

WAYLAND: Of course, Kármán set the pace. And interestingly enough, electrical engineering was part of physics until Lindvall took over the engineering division. And therefore, it retained a very real tie to the basic sciences.

GREENBERG: Electrical engineering was the one branch that was very sophisticated, even from the very beginning of this century.

WAYLAND: That's right. Then, of course, aeronautics became sophisticated because of von Kármán. Then, after Lindvall took over the division chairmanship here in 1945, he felt that all of engineering ought to be brought up to a much more sophisticated level. A small school like this can make no real contribution in engineering lore. It can make its contribution either in basic engineering sciences or in training people who can then go on. They can then learn their lore in the field, the only sensible place to learn that, anyway. You just can't keep up with the lore.

And that was Lindvall's great contribution—to pull everything together and try to make a single division out of engineering and at the same time, to see to it that it was truly scientifically based. I think they've gone too far now. If they want to be a division of applied science, that's fair enough. But the engineering aspect is getting rather badly diluted, I think. Maybe correctly, I don't know.

Of course, what I was doing was not engineering in any real sense. Although, in this country, the sorts of things I was doing would have been called bioengineering, whereas in Europe they would have been called biophysics—a matter of semantics and who's willing to give you a roof over your head and a place to work.

GREENBERG: But it wasn't until Lindvall that the attempt was made, in a very general way, to deal with the problem.

WAYLAND: To bring it across the board in engineering. Electrical and aeronautics had had a high level of mathematical and theoretical background. But the other, mechanical engineering and civil engineering, had not.

GREENBERG: Lindvall goes back to the twenties. When did he take over?

WAYLAND: 1945.

GREENBERG: Why the twenty-year delay?

WAYLAND: He was not made chairman of the division until '45. He didn't have the control. I think he held a dual appointment, didn't he? He had an appointment in electrical engineering, and I think he had another appointment in mechanical or something. He had a dual appointment, but he was in one of the scientifically based fields.

GREENBERG: The basic view that you're expressing toward the engineering sciences was understood right back at the very beginning with Millikan and [Royal W.] Sorensen, and Lindvall was a Sorensen student.

WAYLAND: That's right. But the Daugherties and the Franklin Thomases had a very different view of engineering. And they were involved. It takes time for these things to shake down.

GREENBERG: One last question: Were you involved with committee work?

WAYLAND: Well, I was on the Freshman Admissions Committee for a time, but I gave that up when I went to France. I wasn't heavily involved in committee work here; except I was chairman of the Board of Governors of the Athenaeum for some years. And I wouldn't mind saying a few things about the Athenaeum, but we don't have time for that now.

J. HAROLD WAYLAND**SESSION 3****January 16, 1984****Begin Tape 4, Side 1**

GREENBERG: One of the things that we wanted to devote some time to during this last session was the Athenaeum.

WAYLAND: Yes. I've been deeply interested in the Athenaeum for a very long time. During the early days, as far as I know, Clinton Judy was the chairman of the Board of Governors for quite a while, and then Hallett Smith took over. Hallett was chairman for twenty-five years. And at the time, I felt that some of the things that the Athenaeum should stand for had not been developed—and, in fact, they still haven't, in my opinion, although things have improved. Until we had liquor at the Athenaeum, you couldn't get the [Caltech] Associates to darken its doors.

Sam [Samuel F.] Bowlby, who was one of the presidents of the Associates and a close friend of my brother-in-law's, through the oil industry, was very anxious to get it. He said, "Why did I join the Associates, except to have contact with Caltech?" And yet he had no chance. They would never have the meetings; they'd go down to the California Club, and the same little old cozy group would meet down there, but they had very little contact with the faculty. In fact, they had no contact with the campus itself. But until we were able to get a liquor license, it was just impossible to get very far.

In 1962, I think it was, there was a suggestion that there ought to be a study of the Athenaeum—its function and its purpose. And I was made chairman of that committee. In the course of that study, we got into some of the archives. We got into some of the ideas that [Arthur Amos] Noyes had when the Athenaeum was first developed. It was supposed to be a place where there was much more interrelationship between the faculty and the scholarly world, people who had scholarly interests in the community. However, Millikan, of course, had made the Athenaeum pretty much a place to try to bring money into Caltech. And during Prohibition, this worked fairly well, because liquor wasn't part

of the way of life anyway. I know one woman claimed that she had paid \$1,000 in order to have dinner with Einstein. It was a very good public-relations place. Elizabeth Sprague Coolidge had the Pro Arte Quartet do the Beethoven Quartet Cycle at the Athenaeum. And it was really an intellectual community in many ways in those days. And this study committee felt that the Athenaeum was, at least, dichotomous. It was a faculty club where the faculty could get together, but its primary function there was at lunch; that's about the only time they got together. It was also something that the administration took advantage of, in terms of raising money and putting on an elegant front to the community. And somehow, we had to find a balance between these two.

We felt it was, indeed, the responsibility of the Institute to subsidize the Athenaeum. It was not that the faculty itself would not be able to maintain anything as elegant as that, but if they wanted to pull everything together at the Athenaeum, then, there had to be money from the Institute itself. It couldn't possibly all come out of the dues. We wanted the administration to recognize this dual nature. Well, to some degree they did.

Of course, there was another thing always in the background that has never been proved, but when the [Allan C.] Balches gave the money for the Athenaeum, the story has it—and I'm willing to believe it—that they wanted to endow the Athenaeum. And Millikan constantly talked them out of it, because whenever they would suggest putting up some money, he would say, "Well, but we need it worse here." And I knew Millikan well enough to know that this is very, very plausible. When the Balches died, they left what was at that time a considerable amount of money, because Caltech was the residual legatee. But not a penny of that went to the Athenaeum. The Athenaeum was constantly fighting to keep its head above water financially. Well, then we were finally able to get a liquor license—first we got permission to serve liquor in the private dining rooms, so that it was possible to have private parties with wine.

GREENBERG: This was when?

WAYLAND: Oh, in the sixties. I've forgotten exactly. I don't even remember when I took over as chairman of the Board of Governors. But it must have been in the early seventies, because it was just about the time that Harold Brown came. I had quite a battle with [Caltech trustee] Henry Dreyfuss, because he wanted to put a big bubble over the patio and make it into the bar, and I felt this was going to ruin the feeling of the Athenaeum. I wrote a letter to Harold Brown, saying that I thought that at least it ought to have a careful study and be thrown open to the membership to decide whether they wanted to go in that direction or not. Henry was furious, but in any event, Harold did insist that it be looked at critically. He appointed another committee; Ward Whaling was the chairman. Well, they came up with pretty much the same conclusion we did.

Hallett Smith had decided he wanted to get out from under the chairmanship of the Board of Governors, and he mentioned it to Arnold Beckman. So, Arnold sounded me out on whether I'd take it on. I found out later that Harold Brown was madder than hell about it, because he was hoping they could kill that Board of Governors organization and bring it completely under the aegis of the business office. It was a very traumatic several years when I was chairman of the board, because Harold wouldn't let us move on anything. Anything that cost ten cents, we couldn't touch. It was almost impossible to upgrade the Athenaeum to the level at least many of us thought it ought to be brought to. And finally, it just seemed impossible to get anywhere.

So, I finally talked Jack [John D.] Roberts into taking over the chairmanship of the Board of Governors, and Bob [Robert E.] Ireland took over the chairmanship of the House Committee. We'd had some fiascos with managers who were no good because we couldn't pay enough. And Harold Brown wouldn't let us pay a damn thing; I mean, he wouldn't let us upgrade in any way. It happened that Jack took over just at the time Harold left, but he did not take over until he sat down with Stan [Caltech trustee R. Stanton] Avery and, more or less, got a charter where he could really have some freedom to work. And Bob Ireland really stepped in and acted as manager for two or three years; he hardly did anything in his other job. But they did turn the Athenaeum around, and they got it on to a good solid basis. In fact, I think they've gone too far now in that they're so anxious to make money. For instance, their pricing structure on wine. We set a policy—I was chairman of the wine committee when it was established—of 50 percent

over cost plus a corkage fee, which was either fifty cents or a dollar a bottle. Now, they charge double cost. And most of the wines are a lot more expensive than they were then, too. So, it means that the prices are getting—they're not as bad as the Chronicle—higher than I think they ought to be in a club like that. But they're so anxious to make money, and to a large extent, they've priced the faculty out of the place. We can't afford to go to lunch most of the time.

GREENBERG: You're talking about wine now. What about the food?

WAYLAND: I'm talking about food. You can't get a decent dinner there for less than fourteen, fifteen dollars. And the house wine isn't bad. But, if you buy anything else, you're in ten, twelve, fourteen dollars for a bottle of wine. The one evening that does bring out a few faculty members is Wednesday, because that roast beef dinner is a pretty good buy, but not on other nights.

Also, they somehow missed the point on this business of bringing the faculty together with these other people. When they first announced this Associates' Table, it sounded like a great opportunity for the Associates and the faculty to get together. Now, I find it's only for Associates. I've got to get hold of [Associates' president] Bob [Robert L.] Zurbach and give him a little piece of my mind on that—I know Bob very well. And I really think that they've missed the point, because the Athenaeum was supposed to be a place where there was cross-fertilization. You go there at noon; you sit at the same table practically every noon. I've moved between two tables, so I'm not always at the same table. I sit with my engineer friends part of the time; and I sit with another group part of the time. But basically, it's a little faculty clique, and we don't mingle with anybody else. There needs to be some effort to get more mingling. I put in a lot of effort. I became chairman of the program committee. And we had a very vigorous program. It's true, the Beckman Auditorium took over about that time, too, and it stole some of the momentum, but we had excellent speakers.

I don't know whether you've ever heard of Walter Starkie. He taught mythology over at UCLA; he was a great enthusiast for the gypsies and played the fiddle very well. We had people like Starkie and Elisabeth Waldo, who does ethnic music from the

American Indians and Central Americans. And several times we had a very good flamenco dance group. Pros. We had to pay for them, of course. But now, I find, what few programs they have are dances. I think dances are great, but we ought to have something else.

There's no intellectual activity at the Athenaeum anymore. At one time, it was a real center of intellectual activity. We always had, for at least ten years, a meeting of the Archaeological Institute of America there, where we invited members of the AIA from all over Southern California to come. We made it possible for them to have dinner if they wanted to, but they didn't have to have dinner. Now, for anything that's put on, if it doesn't involve people paying for their dinner, then they're not really welcome. So, I feel the Athenaeum has still failed to meet the goals that Noyes set up.

GREENBERG: It sounds more like an exclusive club for the upper crust now.

WAYLAND: It is. You know it as well as I do. We put on a lot of very interesting wine tastings, and those have sort of gone by the board. They have this fancy gourmet dinner once a year. I went last year and was quite disappointed. It lacked participation; it lacked the interaction between the MC and the audience, because I always made a great effort to bring the people into it so that they really became part of the activity. I would like to see the Athenaeum regain some of this. But times change. There are other ways to do different things. Wine tastings have become a dime a dozen now; there aren't very many very good ones, but they're so common that they've lost some of the glamour they had when we first started them there. Of course, the Beckman Auditorium would feel competition. If somebody like Mrs. Coolidge were to come along and say, "Well, I'd like to hire the Julliard Quartet to give a series at the Athenaeum," the Beckman people would scream bloody murder because we would have cut into their programs. They have a hard enough time getting some of these things. So, it's not a simple problem.

GREENBERG: Does the fact that there isn't very much intellectual activity, fraternizing, or mingling anymore also say something about the academic side of the campus? Is this partially a result of major changes that have occurred at Caltech over the years?

WAYLAND: Well, partly, but not by any means totally. The opportunities are greater now than they ever have been, but it seems to me they're not taken advantage of. For instance, the Associates are coming regularly, and many of them would like to have more contact with the faculty. And some of the faculty, at least, might like to have more contact with the Associates. There are a few people who get invited to every Associate affair. In all the years I've been associated with Caltech and all the battle I did to get the Associates into the Athenaeum, I've been to one Associates' dinner, and that was one where I was speaker. But the hierarchy, you know, the guys up in the top offices, they're at every damn one of them. So, there is something missing, if you really want to get this mingling. Now, I'll admit, at one time I knew virtually everybody in humanities; today, I hardly know anybody.

GREENBERG: And what accounts for that problem?

WAYLAND: Well, it's a mixture. It's partly because of change in direction in humanities—much more toward social science, much more toward hard research rather than interaction. People like Beach Langston loved to talk about their studies; they were scholars in a sort of gentle, old-fashioned sense—not in the hard sense of publish or perish.

GREENBERG: And this is a problem that a better Athenaeum probably wouldn't solve?

WAYLAND: No, it wouldn't solve. But a little different attitude to the Athenaeum, I think, could at least make available to those who would like it, some of these opportunities to cross-fertilize. At one time, the Athenaeum had regular luncheons—every week, I think. Just about every week, they had a YMCA lunch. They had some very good speakers. I remember introducing Martin Luther King at one of the Y luncheons. But it got to the point where they got the prices so high that the Y just couldn't continue to have its lunches there. And then, of course, the crowds have gotten so great; they just don't have space.

There is space that could be developed. For instance, the loggia ought to be developed, but they've got to get an elevator. We worked out the plans for an elevator

system, but it got lost in the days when Harold Brown wouldn't let us even pay the money to have a study. Then, to get the place turned around and get the food going was such a big job that the people who were running it just didn't have time to think about those things. Now I think they should start thinking about it again. We had a very fine architect who was doing some teaching here, Hunt Lewis, and he drew up a very nice plan for developing the east patio. I had a beautiful rendering of it, which I gave to Jack Roberts, but I suspect it's been lost completely now. I see they're beginning to put a little canopy over the east patio. But the east patio's a wonderful place for early evening affairs in the summertime, because the sun is on the side of the other patio. They are, at least, using the patio, but it was an uphill battle to get them to use it at all. I used to be able to get things going in summer, occasionally, out there. But now of course they have to have the space. And necessity helps.

I would like to see a little different ambience there at the Athenaeum. Again, there isn't enough rotation of certain types of activities there. Of course, again, the question is what should we do about things like the scientific lectures at Beckman? We used to have a lecture every week. Now, they have three or four a term. You see, they were a shift from the Friday night lectures in Bridge [Norman Bridge Laboratory of Physics], which were famous in the thirties. I got stuck with the chairmanship of the scientific lecture committee when Beckman was opened. I put the bee on each division chairman; he had to furnish somebody each term. Some of them didn't like it, and sometimes they didn't do it.

GREENBERG: So, while you were in charge, you were still having the weekly lecture?

WAYLAND: Oh, absolutely. In fact, we changed the night. We had people like Fred Hoyle. He used to load the place. But mostly, there were faculty members. Of course, Fred was a visiting faculty member. When he talked on Stonehenge, the place was full, and they were still wrapped around the building about three times. I had to corner him before he went on the podium to ask him if he'd repeat it. He wasn't very happy, but he did. So, we were able to announce when he was going to repeat it in order to let people leave with some sense of happiness.

GREENBERG: The public enjoyed him a lot. I'm wondering how people at Caltech liked Hoyle and his theatrics.

WAYLAND: Well, I think Willy [William A. Fowler] and Hoyle got along all right, but not everybody. And lately, on some of his ideas, he's gone a little cracked, a little potted. The Athenaeum is really one of the great resources of Caltech. And it certainly is doing a lot of good things. I just think it could do more. I still look back at those five, six years I was chairman of the board with great frustration, because it was impossible to work with Harold Brown. He just wouldn't let us move. It took me two years to get Jack Roberts to take over. And I felt, if he took it at all, he'd do a job, and he did.

GREENBERG: Do you see enough indications now that things are getting a little bit better and will continue?

WAYLAND: There's certainly a real possibility now, yes. If I were going to be here steadily, I'd be inclined to run for a job on the House Committee or something in order to get closer to it and see what's happening. But for the next two years, I don't see that I'll be here more than about half the time; it just doesn't make any sense. So, I can't really complain too much when I'm not in a position to do something.

I *am* going to talk to Bob Zurbach about this business of trying to get the Associates together with the faculty—maybe only once a month, if they're going to have this table once a week. But I think there ought to be some time. As I said, after reading about the first announcement, it sounded as if it was a chance for the faculty to get together with the Associates. When the next announcement came out, I was going to call up and make a reservation, and then I found it was for Associates only. Now, I don't know—maybe there wouldn't be three faculty members who would raise a finger to do it. But at least it ought to be given a try. You've probably never seen Noyes' original document on the Athenaeum. I must have a copy someplace. It might not be a bad idea if I dig into my archives on that and send those basic documents, such as Noyes' original proposal, and get them into the Archives. I know I've sent them to the House Committee and the Board of Governors' chairmen since I was involved, and also the copies of the

reports from my committee and from Ward Whaling's committee. I think they are a part of Caltech documentary archives.

GREENBERG: Is there anything else that we might talk about in the time remaining? Any odds and ends?

WAYLAND: What has made a difference: The fact that it is bigger, that people have become less identified with Caltech as an institution than they used to. I don't think you have the same loyalty that you had at one time. And I think it's only reasonable. When I came here, I came here with the full understanding that I would never have to go out beating the bushes for any of my salary. And yet, the last few years, I was bringing in 40 to 45 percent of my salary from my grants, in addition to all my research money. So, you just cannot have the same loyalty to an institution that merely gives you a roof as you can to one that is supportive.

GREENBERG: How important is it to the morale of people at a place like this that there be a Nobel Prize as often as possible? Is this something that people worried about in the old days?

WAYLAND: Certainly, I don't think they thought about them much in the thirties. When a Nobel Prize came along, it was considered a wonderful but rare event. You didn't go out beating the bushes for it.

GREENBERG: Do you sense that there is a preoccupation with that now?

WAYLAND: Well, I wouldn't be surprised in those departments where it is possible to earn one. After all, it doesn't happen in engineering. It's a bit like my daughters at Bryn Mawr, where there was no such thing as Phi Beta Kappa, so they didn't have to worry about making Phi Beta Kappa. So, you're over in engineering; all right, you're out of this mainstream. Very, very few people in engineering have ever become members of the Academy of Sciences. Now we have an Academy of Engineering, but it's sort of an iffy thing, as it has developed so far. It is neither hard engineering nor engineering science;

some of each category get in, but I don't think it's made up its own mind what it's going to do. Of course, I've been out of the running on all accounts, because here I was a maverick in the engineering division, so I didn't have to worry about it. Somehow, I have the feeling that there may well be a strong thinking about Nobel Prizes in biology today; I may be wrong. The direction they've jumped in their research looks like the bandwagonism that goes with this kind of thinking.

We got to talking the other day about the nonscientific work. I don't know whether that interests you or not, because about twenty-five years ago, my wife got interested in the history of playing cards. I've always been interested in wine and food, and I've done a little publishing in that field. But she had an interest in the history of playing cards, and so recently I've gotten involved in that. Over the last ten years, between us, we've published quite a series of articles on early English playing cards and recently one on some early 16th-century German material. It's been rather interesting to me, because here the professionals in art history sometimes miss the boat. Detlef Hoffmann, who's professor of art history at Oldenburg, did quite a major article on this German sheet and somehow, he missed the dating. We got [Edward F.] Maeder at the LA County Museum to help us with the costume dating, and then we found the watermarks and were able to pin down the dating of the watermarks, and by some fortunate fluke they agreed with the dating of the costumes as well, because people can carry costumes down. You look at costumes on a current deck of cards; they're not contemporary in most cases. They can't be earlier than the style of costume, but they can be a heck of a lot later before you get the paper and the costume to fit. It's really rather nice. And now we're over our ears in a book on American Indian playing cards, which we hope will become the definitive work on the subject. So that's one thing that's keeping me kind of busy, in addition to my work in microcirculation.

As chairman of the liaison committee for the European Society for Microcirculation, I'm having to organize a meeting; we're having the Third World Congress in Oxford this coming summer. Then I have to get the group together and try to organize the Fourth Congress—I think we're going to have it in Japan. I've also got to get out from under it, because, after all, I'll have my seventy-fifth birthday this summer and I don't think that I should carry it all alone.

GREENBERG: But, as of the moment, you're still going pretty strong when it comes to microcirculation?

WAYLAND: Oh, yes. It's very probable I will be spending a full six months in '85 on a visiting professorship. It keeps me hopping. But we're still trying to keep this other work going. I had thought that when I retired, I'd just spend more of my time on the more humanitarian interests, but I haven't had time to put my full time on it yet.

GREENBERG: A moment ago, you mentioned that people don't identify with Caltech as much as they used to. I'm wondering if, in your particular case, that has varied over the course of your career?

WAYLAND: Oh, yes, very definitely. I always felt very, very loyal to Caltech. And yet, as years went on and as I had to spend more and more of my time supporting myself and my other research, I would have left Caltech, to be frank about it, if I hadn't been so close to retirement—especially when Bob Cannon was chairman. But I was so close to retirement, it just was silly to think about it. That's why I pushed hard. Since I was retiring, the best thing to do was to see that the research continued and get it someplace else. But, of course, this is always a problem when people retire, and when they are in a field where there's no real commitment on the part of the Institute to carry on. I found a lot of hypocritical talk.

For instance, when we were really just getting going stronger in microcirculatory work, I felt that if we could have a really good physiologist appointed who had a real interest in the field, then there might be a chance to develop it into something really major. I still think it could have happened. And I had my fingers on an individual or two, too. But it was stated very bluntly, particularly by the biology chairman at that time, that we do not hire people for a specific field; we hire people only because they're good. And yet, right and left they were hiring people to fill in gaps in specific fields. So that kind of hypocrisy doesn't make you feel very beholden to your institution. And then, when you have years with a halftime president, as we did with Brown. Of course, I retired shortly after he left, so I have no way to judge our current president.

GREENBERG: So, you feel it's mostly a problem of not having a vision rather than a dollars-and-cents problem?

WAYLAND: I think the money could have been had, yes, especially at that time. It's getting more difficult now. And of course, my biggest push right now is in connection with what I call Intravital Observatories. I think you need this concept. I just got a big letter off to Japan, trying to stir them up. I didn't see any likelihood of its being something that would fit into the Caltech milieu. In fact, everything I have learned from talking to people like Dick— **[tape ends]**

Begin Tape 4, Side 2

WAYLAND: Like Dick [Richard Day] Deslattes, for example, who for a couple of years headed physics for the National Science Foundation—now he's with the Bureau of Standards. He was very much involved with the concept of the synchrotron radiation facility. And he came to the conclusion—their committee came to the conclusion—that such methods-oriented facilities did not belong as part of a university, that they were better off if they were not tied to the university complex. The only real exceptions are those that are set up by consortia of universities. Still, they aren't tied to a single university. You need greater breadth, and you need greater elbow room. And the synchrotron radiation facilities are being set up as separate institutions to a large degree. But there are some very serious problems with that. The high-voltage electron microscopes, for example, I think may be tied to universities. But the organization has missed the point.

You've got to do more than just make the facility available; you've got to help the people use it effectively, and that means helping them financially as well as giving them the opportunity to work there. Any such facility has to have a very careful rethinking of the organizational problems. Some of the synchrotron radiation facilities, of course, are just spinoffs of regular synchrotrons. Then, the people who want to use them for the radiation are tied to the other programs and consequently they don't get what they want a good deal of the time; they're third-rate citizens. Those are real problems of organization along this line.

It amazes me when I see the kind of money that is going into astronomy, the kind of money that's going into certain types of space research—of course, we know the military has an interest in space research, and that brings money into it that it wouldn't otherwise get. But I find the people in biomedical research—except when they go to buy these fancy big machines, which are more show than use, in my opinion—just get frightened if you're talking about something in the neighborhood of half a million dollars. And yet, we're talking about multimillion-dollar facilities in most of these things. I just don't know why biomedicine has such a hard time getting major chunks of funding.

I'd had great hopes that we might get something going with Kroc. But Joan Kroc managed to wangle that one beautifully—you know, Ray died just in the last few days. She managed to get complete control of the research money shortly before Ray died, and now it's all going into glamour stuff where she can shine. The research program is just collapsing completely. Some years ago, Bob Kroc said that if Ray were to turn up his toes the next day, they wouldn't know what to do with all the money they had. And now it's going into glamour-puss stuff—the problem with marrying a young wife who has ambition.

GREENBERG: But there is this general problem as far as funding biomedical research?

WAYLAND: Part of it I see as the lack of vision on the part of the people involved. There isn't a force. You can't build an observatory without a big telescope; so, there you've got a fairly expensive facility. And once you sell somebody on the idea of that expensive facility, it's easier to recognize the need to build things around it. But not when it's built up of a lot of smaller units, even though they add up to a lot of money—like lasers, for example.

I was over at Tucson not long ago in connection with some work they're doing on metabolism and tissue, and they wanted to use ADH fluorescence to study mitochondrial activity. The simplest system is to stimulate it with a laser beam. But they're having a devil of a time finding \$7,000 to replace the tube in their laser. This is the sort of thing that you run into. And yet, what you ought to have is someplace where you can go and have not just one laser but a gamut of laser radiation available. And there you're going to

be in several hundred thousand dollars by the time you get the group of lasers put together. But there's no one thing with lasers that has this big glamorous look of a 200-inch telescope. Sure, the nuclear magnetic resonance devices look pretty spectacular. They can be useful when some good work's going to be done with them, but an awful lot of them are going to be used just for show and to raise our medical bills.

GREENBERG: Are there other places where this is much less of a problem than here?

WAYLAND: I haven't seen it really, no. It's true that in Japan they have developed some major facilities. Their Institute of Physiology, for example, has a very fascinating radiation facility for studying the effect of radiation of different wavelengths on various types of plants. They have just a huge spectrograph with a huge high-pressure arc to put out the voluminous energies broken up with a big grating and spread out on a huge circle. And then at various places in the circle, they have little chambers where they can pick the radiation of that particular wavelength—they can put a plant in there or put various other things in there. They spent a hell of a lot of money on it. I would have gone the route of dye lasers with specifics for each individual tactical experiment. But this other is the route they decided to go. It's quite a spectacular facility. I don't know what's going to come out of it. High-energy electron microscopes are not cheap. The first high-voltage electron microscope I saw was in 1973 in Japan; it took them nearly twenty years before they started applying them in biology in this country. There they were doing it in metallurgy; they weren't using them in biology. They're finally beginning to come into their own here, at least. I haven't followed that field in Japan; the people I've been working with haven't been involved in it.

But I haven't seen any really big, well-organized, well-integrated facilities in any country. There've been these small laboratories where the *Herr Geheimrat* runs the show and, he gets as much of the credit as he can grab. The nearest thing is the Institute for Systems Physiology in Dortmund, and it's a Max Planck Institute. Max Planck Institutes probably come closer than anything else I've seen. But there, they're built around an individual, and when that individual retires, if he doesn't have a good crown prince, they just wipe out the program.

GREENBERG: You're back facing the problem that you have here, basically.

WAYLAND: Yes, absolutely. And yet people wouldn't throw the Schmidt telescope away when [Fritz] Zwicky quit looking for supernovae; it's busy, busy, busy, right now, with other problems. It's an interesting problem, and it's one that gets pretty frustrating sometimes, especially since all I can do is be in a position of salesman and try to persuade people. I'll be most interested to see what reaction I get out of this long letter I sent to Councilor Takagi in Japan. At least he's trained as a physiologist, has been a college president, and now he's a member of the Diet. It'll be interesting to see whether he gets the vision or not.

GREENBERG: As frustrating as it is, it sounds as if you haven't given up.

WAYLAND: Oh, no! I'll give up when they bury me, I suppose, or when I have a stroke and can no longer operate. These things don't happen overnight. I've been used to being ten years ahead of the times. The work that I did for my doctoral dissertation, nobody got interested in until after the war. Then they wouldn't believe anybody did it before the war, so they did it all over again.

GREENBERG: But it was there.

WAYLAND: It was in the literature, yes. Certainly, once I got into the microcirculation field, I couldn't get anybody to pay any attention to the high-sensitivity television cameras. Now, everybody and his dog is taking the ones that we were using before they were even on the market. We were having to build our own cameras. Nobody paid any attention to it. But they aren't the things they should be buying; there are better things available.

GREENBERG: In a number of instances already, you've been proven right. But it took time.

WAYLAND: Oh, yes. It takes time. So, if I think I'm right, I'm going to keep fighting for it. It doesn't matter. I'm beyond the point where I have to worry about publish or perish. And it does make a difference. You can be a little more relaxed about these things, although my Irish blood gets stirred up once in a while.

Caltech is an interesting place. I've enjoyed being here, and I still have a lot of loyalty to the place. Otherwise, I wouldn't write about certain things.

GREENBERG: There's nobody that goes further back.

WAYLAND: Well, Lindvall does. Fred [Frederick J.] Converse, I guess does. There aren't many. Willy Fowler came just a year after I did. Of course, he stayed continuously; I was in and out for some years, which is both good and bad. I think it's good, actually—it was for me. But if you don't have that long period of continuity.... Another one is Vito Vanoni. Vito and I were living in the old dorm here together back in 1931-1932. We were at a party the other night, and his wife said, you know, we were the first people she ever met on the campus. They were married a little bit after we were. They haven't quite hit their half century yet.

Millikan brought Theodore Gerald Soares here to be professor of ethics. But Millikan really wanted him as the minister at the Neighborhood Church. The Neighborhood Church paid half his salary; Caltech paid half of his salary. But the Neighborhood Church had to take on his entire retirement. Of course, in those days, there were a lot of rich ladies who used to come out and spend the winters here in California, and they'd go back to their original homes for the summertime. They gave a lot of money. That church was pretty wealthy in those days. So, the old Chief was made a lifetime member of the Board of Trustees. Nobody else could serve more than so many years, but he was a lifetime member. Millikan died in 1953. Shortly after I joined the faculty in '49, I was on the board. In fact, I was president of the church for a time, and I used to take him to the board meetings. So, we had quite a chance to chat about things. He was an interesting man with his religious convictions.

Soares had been brought up a Baptist, but he couldn't stomach the Baptist faith as it were, the Baptist rigidity. So, he shifted over to become a Congregationalist. I happen

to know this because he married us, and he signed our wedding certificate as a Congregational minister. The church was a mixture of Unitarian and Congregational, but it got a strong influx of Unitarians when people got disgusted with the man who was head of the Unitarian church in Los Angeles. They thought he was too far leftwing, so they moved up to the Pasadena church. Millikan pushed pretty hard to keep that church going. And, of course, during his days, money was a lot easier to come by than it has been in recent years.

GREENBERG: When you got to know Millikan better, after '49, was he still lucid?

WAYLAND: Oh, absolutely, yes. Very sharp. Of course, I went off in '53; he died while I was in Europe, so I wasn't here at the time. But he had [laughter] more or less allowed himself to be ushered into that group at Forest Lawn—the Immortals, or something. Von Kleinschmidt was the one who was running the show. Apparently, it was a pretty disgusting thing. Fortunately, the minister of the Neighborhood Church, Curtis Beach, saved the day. But it was a pretty sorry display. I guess Millikan got some money for Caltech out of it. He was a pretty tight-fisted character!

I think I've probably mentioned the fact that when I came back from Idaho to finish up my doctorate, I hadn't applied for a teaching assistantship in advance. There was nothing to feed me. The Chief offered me the job in the physics stockroom. "Oh, that's only eight hours a day; that leaves you eight hours a day for research." That's the way he lived. To him, that was a perfectly reasonable statement.

GREENBERG: One of the things I'm currently working on is the problem of mathematics at the Institute in the twenties and the thirties. It just didn't seem to work out as well as some other things. And some of the mathematicians who were here complained about Millikan and penny-pinching. I don't know whether or not that really is part of the problem. But they did complain about it. [E. T.] Bell complained about it endlessly.

WAYLAND: Well, I'm not surprised. Of course, Bell knew how to complain. At the same time, I suspect it was very difficult to get people of the caliber that they wanted, because, you know they paid very little. I think it was [biologist Alfred] Sturtevant who

was offered a job someplace else and Millikan finally talked him into staying for quite a bit less than he was offered elsewhere—partly because of his loyalty to [Thomas Hunt] Morgan and partly because Millikan said, “Well, after all, you can afford to live in California for less than you can in other places.” Rather than paying people what they were really worth, he paid them what he could get them for. And sometimes he missed it.

GREENBERG: In physics and biology, he was able to get very, very good people for less. But in mathematics it didn't work out.

WAYLAND: I don't know why either, because mathematicians, in general, have not been highly paid people. But I think they felt the lack of ambience; this would be my guess. They just didn't feel there was anybody here that they really wanted to come to work with.

GREENBERG: You were here in the thirties; you knew Bell, Michal, Bateman.

WAYLAND: I knew Bell, I knew Michal; I knew Bateman and Ward the best.

GREENBERG: You knew the big four. What's your feeling about the department in those days?

WAYLAND: Well, I felt each of them went his own way. I couldn't see any evidence of any group activity. Ward was a number theorist, as was Bell; in that sense, they fitted together, although Ward was forced to teach analysis and taught a splendid course. He was absolutely a tremendous teacher. Of the three years of advanced mathematics I had, two of those years I took under Ward, and one under Bateman. But Ward was splendid, a wonderful teacher, and a wonderful person. I knew very little about his research or the quality of his research, because it was so far from my own field of interest. I became much closer to Bateman in that respect because Bateman's interest in analysis was closer to my interest.

GREENBERG: Was mathematics as dismal here in the thirties as I've somehow come to think it was?

WAYLAND: I just don't know what was going on elsewhere. Certainly, you didn't find the young mathematicians lapping at the heels of the older people like the young theoretical physicists lapped at Bob Oppenheimer's heels. Oppenheimer would come down here for his term from Berkeley, and all these little longhaired theoreticians were just following him, almost breathless. And you didn't have any of that.

GREENBERG: Nobody with that kind of charisma here in the math department?

WAYLAND: No, not at all. I knew a few of the mathematics students. They were good solid citizens, but as far as I know, none of them really developed into anything very great. Lawrence Botsford went up to the University of Idaho where I did my undergraduate work and spent one year teaching. He went up there and stayed until, I guess, he died. That's not exactly a place to shine. And, of course, Bob Martin died quite young from TB. He was probably the most brilliant of the students.

GREENBERG: Well, the math thing is hard to deal with. Sometimes I get the causes and the symptoms mixed up in trying to work out this story.

WAYLAND: Yes. Bateman—I was interested in your comments in this article on Kármán about Bateman, because to a large extent they did get the right atmosphere about Bateman. Bateman was really an applied mathematician. One of his major works was in hydrodynamics, and his interest was in the application. It's true, his interest in application was somewhat different from that of a Kármán. Kármán looked at every differential equation as a way of expressing certain physical concepts. This was one of the things that I modeled much of my teaching on—on Kármán's philosophy. I felt that, after all, the big problem is to set up the equation and to know what it means physically.

I think I mentioned to you the fact that one time, Morgan Ward just bailed out of a problem that we had during the war because he said, "I just don't know enough physics to know what I can neglect." Of course, today, with the computers, you don't have to

neglect so much. You can try it out and see whether these terms are important or not. But in those days, you had to simplify it, or you just couldn't touch it.

GREENBERG: But did Bateman have a sense for what things meant physically?

WAYLAND: He certainly didn't have the sense that Kármán had. But I think he had some sense of it; his problem was to communicate. He was the most gentle person, the most willing to explain things if you were not over-awed so much that you wouldn't ask him. It was only after I'd worked with him a couple of years after I had my degree that I got to the point where I could really communicate with him. And then the war came along, and he died before I got back. I would have looked forward tremendously to a chance to interact with Bateman, after I'd gained some maturity and had learned not to be afraid of him. But that wasn't in the cards.

GREENBERG: I know that Clifford Truesdell, at Johns Hopkins University—who was also Bateman's student here in the late thirties—feels the same way about him. He holds him in tremendous esteem but says that he could be very intimidating, certainly at the beginning, when you were first encountering him.

WAYLAND: Well, Truesdell has made a pretty good name. I think he's probably made as big a name for himself as any of the people who came out of here in mathematics. I'd forgotten about him, but I think he has done some quite interesting work.

GREENBERG: Do your interests overlap in part with his, insofar as you're a rheologist?

WAYLAND: At one time I was very heavy in rheology, particularly up until the early sixties. I am a member of the International Society of Biorheology, but I really don't do much in it; I haven't for a long time. A lot of people still have me categorized as a rheologist. For a long time, I was getting things to review, but I was too far away from it to handle it sensibly. They don't send things anymore. I've turned them down too often. But, again, Truesdell is thinking in terms of application, not in terms of the pure mathematics. And I would say that's where Bateman's real forte was. But it was at a

different period and a different ambience than we now have. On the other hand, when I first started working with him—and I didn't quite understand what he wanted at the time—he was interested in the use of optical techniques for doing certain calculations. He was hoping that with my background in physics, I would be able to run off with some of these things. I got off onto another sidetrack, and the time disappeared.

GREENBERG: I saw some correspondence between you and Bateman. He was going to try to use this to solve differential equations.

WAYLAND: Yes. I dug through my archives here and sent what little bit I had. I didn't have much in writing, because we saw each other mostly personally. But I sent all I had over to the Archives.

GREENBERG: He also seems to have had an interest in the information-retrieval problem.

WAYLAND: He needed it!

GREENBERG: Right, with his shoe boxes!

WAYLAND: Of course, he had beautiful information retrieval. It was absolutely astounding. You'd go to that man and ask him a question, and he'd say, "Well, you'll find that discussed in the *Proceedings of the Royal Society*; it was probably 1887 or '88—oh, it must have been '88. And would be on page so-and-so in the upper righthand corner." And it would be there! But that didn't get him out of the shoe boxes. And when they hired this group of [Arthur] Erdélyi and [Wilhelm] Magnus—and there were another one or two—to come and try to pull together the Bateman volumes, they couldn't get anything out of those shoe boxes.

GREENBERG: That's right. They started from scratch.

WAYLAND: Yes. They just produced something in honor of Bateman rather than pulling together the things that he had started, really.

GREENBERG: And the shoe boxes, fortunately or unfortunately, are gone!

WAYLAND: That's sad. I don't know if anybody could get anything out of them or not. I would like to have tried.

GREENBERG: The frightening thing was that his inventory seemed to go back at least as far as Newton. [Laughter]

WAYLAND: Yes, it's funny what you run into. For instance, one of the methods that was published in the twenties, in this paper I did under his aegis on determinantal equations, I found went back a century. Things get lost.

GREENBERG: There's a lot of rediscovering.

WAYLAND: Oh, Lord, yes.

J. HAROLD WAYLAND**SESSION 4****January 3, 1985****Begin Tape 5, Side 1**

PETERS: One of the things we were interested in knowing more about was the math classes you took with Morgan Ward.

WAYLAND: Yes. Well, in 1931, when I came as a first-year graduate student, the course in mathematical analysis had not been taught for several years. Apparently, E. T. Bell had taught it, and then Morgan Ward was asked to teach it. Morgan wasn't very enthusiastic, because he was really a number theorist and mathematical analysis was not exactly his bag, even though he was obviously very well equipped. So, he took a class of about thirty of us through Whittaker & Watson's *A Course in Modern Analysis*. The class attracted really quite an outstanding group of graduate students. I remember in particular among those who were working closely with Linus Pauling were Sidney Weinbaum and E. Bright Wilson.

PETERS: Pauling was on the faculty at that time?

WAYLAND: Yes, Pauling was already on the faculty. And he was already beginning to work on his book on quantum mechanics as applied to chemistry. He took Bright Wilson in as his coauthor. I sat in on that course—although not taking it for credit—the first time it was given, essentially from notes, before the published version came out [1935]. It was quite obvious that Bright Wilson leaned very, very heavily upon the methodology of mathematical analysis that had been developed by Ward in that particular course. We had some other people who went on to do quite distinguished work over the years. I remember Dwight O. North—better known as Don—who became the principal theoretician for the RCA laboratories, first in Harrison, New Jersey, and then later in Princeton, where he retired a few years ago but is still active as a consultant.

PETERS: What kind of a theoretician?

WAYLAND: He took his doctorate in theoretical physics. I don't remember with whom he was actually working, but I know that Wilson was very close to Pauling. And Weinbaum was very close to Pauling, although I thought he took his degree in physics. He stayed around for quite a few years doing work with Pauling on molecular structure, including the use of the old IBM computer, which was a pretty crude thing compared with contemporary computers. That was in the thirties. It was almost more of a sorting system; they couldn't handle very complicated problems. And it was through their interest in solving very complicated determinantal equations that show up in determinant form, that when I was working with Bateman as a research fellow, Sidney rather twisted my arm into working on the problem of getting the determinantal equations into polynomial form, because the computers available at that time were completely unable to handle large matrices and invert large matrices. If you could get it into polynomial form, then there were many techniques for getting the roots, and getting the maximum root; or you could invert the thing, so you got the minimum root. Often, the biggest or the smallest real root was very important in their molecular-structure work.

PETERS: So, they didn't need all the roots.

WAYLAND: They didn't normally need all the roots. But once you get one root, you can extract it. You see, you can factor it out if you have it in polynomial form. Now, of course, if it's an even-order polynomial, then there may be a pair of complex roots. If it's an odd-order polynomial, there will always be at least one real root, and sometimes factoring out roots can help. Of course, the root with the maximum absolute value, even in an odd-ordered one, might indeed be a pair of complex roots. And it was important to unscramble these. Today, those techniques seem very, very crude; but at the time, they were about the only thing we had available.

PETERS: Were there other students in that Morgan Ward class that you remember particularly?

WAYLAND: There was a chap, I think his name was [Ralph] Hultgren. If he's the one I remember, he went to Berkeley. He was head of some major department there. And then there was a chemist by the name of West, who was also quite outstanding. But I don't know where he went. I have a feeling that people like Bob Martin, who was one of the most brilliant of the mathematicians of that time, were not in that course.

PETERS: Were you just aware of these people being in the course, or did you study a lot together, or work a lot together?

WAYLAND: Well, I saw a fair amount of Sidney. Otherwise, I didn't work very closely with most of them. Although Sidney tells me that the last term of the course, students were working primarily in groups of three on special problems. I don't remember that. I was talking to Sidney just this last weekend, and we were discussing our time in Morgan's course. It was a superb course. And I think partly because Ward did not feel that it was at his fingertips, he put a tremendous amount of work into it. And I don't think I have ever heard such excellent lectures on mathematics as he gave.

PETERS: Now, this was the time at which quantum mechanics was just becoming the current thing. What was the status of quantum mechanics?

WAYLAND: The Schrödinger wave mechanics was already pretty well established. And it was this that Pauling and Wilson used in their work on the application of quantum mechanics to chemistry. The Dirac approach to quantum mechanics was just coming in, and [Paul] Epstein in one of his courses—this was in my third graduate year, the spring of 1934—spent the third term going over Dirac quantum mechanics from Dirac's papers, because nobody had written anything in book form by that time.

PETERS: And was this stuff really controversial? Were people excited?

WAYLAND: There was a lot of excitement about it, but nobody could quite see how it was going to fit into the overall picture of physics. The Schrödinger wave mechanics was

already being accepted as something that had potential usefulness. As I say, the very fact that Pauling felt it worthwhile to give a course in this, primarily to chemists—

PETERS: It was already moving out of physics and into chemistry?

WAYLAND: That's right, at least in his hands. This was where, I think, he began some of his really pioneering work in applying mathematical methods and physical methods to chemistry.

PETERS: Do you remember, at that time, people saying, "This stuff is never going to work?" Not necessarily Pauling's work, but Dirac, and so on.

WAYLAND: There was a certain amount of iffy-ness about the Dirac approach, as to whether it would really have any application in understanding physics. Of course, von Neumann's book on quantum mechanics was a very major conceptual bridge. I think this was dealing with the Dirac theory; I'm not really sure; it's been too long since I've been involved in this. But he was able to show, by using a sophisticated mathematical approach, that it was possible that some of these things that had been thought of as just sort of empirical were mathematically sound. Now, it's true that the Dirac theory was based on spinors, and I don't think that was involved in the von Neumann business so much.

PETERS: Von Neumann was taking more of a purely mathematical approach?

WAYLAND: Yes, he was.

PETERS: What was the attitude of the physicists?

WAYLAND: I would say fairly skeptical. We were still in a transitional stage in physics, between the classical approach and the quantum-mechanical approach. There was still a lot of detail work being done in spectroscopy, for example, that really hadn't begun to use quantum mechanics for the explanations.

PETERS: It seems like you get these things coming along in a fragmented way, and then somebody's got to sort of unify it. When do you think this kind of unification took place?

WAYLAND: Well, it was definitely taking place during that period, in the thirties. I was not so much in contact with that aspect of it. I felt that the whole problem of attempting to pull together the various approaches to relativity was being attacked with a much more systematic approach at that time. Of course, Tolman was one of the great people, both in statistical mechanics and in relativity theory. And I think he was more responsible than any other individual at Caltech for seeing to it that people like Einstein, the Abbé Lemaître, and Professor de Sitter were brought together on the Caltech campus.

PETERS: Where was de Sitter?

WAYLAND: He was a Dutchman. I don't know just where his university was. And Lemaître was Belgian.

PETERS: Tolman had a conference, or did he have them come for semesters?

WAYLAND: Of course, Einstein was in residence for a time. That would have been in 1931-1933—I know he was there then, because it was in the spring of '33, at the time Virginia and I were married. I don't know how much longer he stayed, but I think it was during that spring that Lemaître came out to spend a few days or a few weeks—I'm not sure how long. But I remember very definitely, a two-hour theoretical physics conference at which Tolman was the master of ceremonies. And he flipped back and forth between French, English, and German, with the greatest of ease. Einstein refused to speak English in public. Lemaître, I'm not sure how well he knew English; he certainly spoke very little in the seminar. Many of us were there to see the great men, because we were having a devil of a time hanging on. That was the occasion when they were talking about models of the universe. As we walked out of the room, Harry Bateman turned to me and said, "I wonder if the gentlemen ever stopped to consider the assumptions that went into their equations?" I think it's a very cogent remark, and it was important to me,

when I eventually started teaching applied mathematics—to try to get the students to look critically at what they put into their equations.

PETERS: I wanted to ask you more about quantum mechanics. You were talking about relativity theory, but did quantum mechanics affect the kind of work that you eventually did for your dissertation?

WAYLAND: No. I was doing purely experimental work on atomic collisions. Later, when I was in Copenhagen, we tried to look at some models, but even those were more classical. We tried to look at essentially a model where, when two atoms bumped into each other, you get sort of a boiling phenomenon, a sort of an energy transfer, and finally, one electron would pop off if enough energy was there, and enough of it got concentrated in one position. This had been used effectively by one of Bohr's students in discussing the particles coming out of nuclei. But we couldn't make it come even close to predicting the kind of energy transfers we found in atomic collisions.

PETERS: How were you making your collisions happen?

WAYLAND: Well, what I did was to use *Umladung*, or charge transfer—I'd learned this from Otto Beeck, who had taken the work of a pair of Germans, Kalman and Rosen.

That was in my second graduate year at Caltech, when I worked as an assistant in Beeck's laboratory, to learn the techniques. And what we did was to accelerate a beam of argon ions and then we would collimate the beam, we would pass it through a slit into a chamber and out the slit on the other side. The pressure in the chamber was higher than the vacuum, about 10^{-4} millimeters of mercury. A certain number of the particles going through would bump into atoms of argon that were in the chamber. And sometimes, they would pick up an electron to neutralize them, and if they didn't get deflected significantly, then they would have lost virtually no momentum. So, those few that got out the far end would have about the same momentum as those that had entered—only a small fraction got out; but with careful manipulation, we could get about a 10-percent efficiency—then the beam that came out would be a mixture of ions and atoms. Then we

would sweep the ions out of the road with an electrostatic field, and only the atoms would go on. And that gave us a probe.

It was Zwicky who proposed this problem. Say you take argon bumping into argon. Now, if you have an argon ion, when it bumps into a argon atom, if it hits it hard enough, it's going to knock an electron off, maybe. Whatever the number of electrons on the argon atom, it is enough to make a neutral atom. But your ion has one more positive charge or one less negative charge, so that the net forces holding the electron cloud in the neighborhood of these two nuclei are going to be greater than if you have two atoms bumping into each other. So, the atoms should take less energy to strip off an electron when they bump than an ion and an atom. He had suggested this as a possible mechanism for maintaining the gas discharge.

Millikan was very interested in gas discharges—or had been. He sort of lost his interest by the time I came along, but he still had some work going along this line because no one had been able to predict properly the mechanisms of how a gas discharge is maintained. Later, it was found that enough photoelectrons would be emitted, enough radiation would be emitted, so that the radiation would strike the cathode and knock off electrons. And those photoelectrons were really what was responsible for keeping this thing going. It was not the improved efficiency of ionization by atoms; it was a completely different phenomenon. But we didn't know this at that time. So, this was an attempt to explore that.

PETERS: When you went to Copenhagen to work at Bohr's institute, did you find the whole attitude toward quantum mechanics markedly different than at Caltech?

WAYLAND: Well, of course, Bohr himself had developed the Bohr atom many years earlier, and that became very important in terms of the whole wave-mechanical theory. I wouldn't have said there was such a big difference, except that there was a bigger concentration of theoretical people in Bohr's institute than there was at Caltech. So, from that point of view, I think there probably was more interest. But there was great interest in cosmic rays. The problem I worked on was neutron absorption, when we found that we couldn't make good theory of this atomic-collision business. That paper was never

published, because it got lost in security during the war. By the time the war was over, it was sort of out of date, although Placzek told me it was used a lot at GE, where he worked on things related to the A-bomb.

PETERS: So, this growing acceptance of quantum mechanics was fairly uniform both here and in Europe.

WAYLAND: Well, that was the impression I got, yes. The real burgeoning came in connection with such things as the development of the A-bomb, and the people who had worked with that were in a position to move on from there, whereas those of us who got out of atomic and nuclear physics at the time of the war, found ourselves so far behind that it didn't seem worth even trying to return.

PETERS: You said you sat in on a course of Pauling's on quantum mechanics. Was that the only real contact that you had with what he was doing at the time?

WAYLAND: Yes, that was the only contact I had with him. After all, he was in chemistry and I was in physics.

PETERS: I'm always curious about interdepartmental cooperation—how much people talk to each other across department lines.

WAYLAND: Well, you did it with your graduate-student friends in other disciplines, but that was about the maximum amount of crosstalk. Of course, I talked a lot with the mathematicians, particularly Ward and Bateman, because after all, that was my minor. And I sat in on one of Bell's courses for a while but found it a little boring. He was fun, but there wasn't much meat in it. I'd been over virtually everything that he'd covered and in somewhat greater depth, so it just didn't seem worth continuing. I sat in on von Kármán's course in applied mathematics, and that was exciting—not that there was anything particularly new mathematically, but Kármán had that ability to see the physics in a mathematical equation. When he'd write down a term of an equation, he would tell

you what that term meant physically. So, you had a feeling for what physics had gone into that particular equation.

This reminds me of an experience with Ward during the war, when we took him a rather difficult problem in fluid mechanics, involved with water entry of torpedoes. He said, “Well, frankly, I can’t really help you. If you have an equation, I can help you solve it. But what you need is help at the level of formulation of the problem, and I don’t know what terms I can neglect.” If you don’t know what you can throw away for physical reasons, then the equation becomes so complicated that it was beyond the capability of solution at that time. Today, with computers, this is not so true, but at that time if you couldn’t come up with an equation sufficiently simplified that it was amenable to an analytic solution, you were in real trouble. And, of course, the whole business of nonlinear mathematics was just barely being developed.

PETERS: Let’s go back and talk about Sidney Weinbaum.

WAYLAND: Well, of course, I was very close to Sidney. We had taken Morgan Ward’s course together, and he was one of the first people I met on campus.

PETERS: He was several years older than you.

WAYLAND: Oh, yes, quite a bit older—about ten years, I’d say. He had come from Russia [in 1922], had quite a time getting out. I don’t think he liked the Russian political system in a certain sense, but at the same time he had sort of an idealized feeling about it. There was a lot of exploration being done—well, we were in the middle of the Depression. Things were pretty tough financially. It was pretty obvious that our political system had come a cropper. And there was a lot of starry-eyed vision about the possibilities of the Russian system.

PETERS: This was about 1932?

WAYLAND: Yes, the early thirties. There was a lot of thinking going on. Several American scientists went to Russia. Many of them came back rather disillusioned,

because they felt that it was so tangled up with bureaucracy that regardless of the philosophy, it just wasn't working. Yet the whole philosophy was very appealing to anybody with any sort of ideals. I mean, the dog-eat-dog aspect of strict capitalism is not very attractive to anyone who is idealistic at all. So, we couldn't quite see the weaknesses. First of all, there wasn't much information coming out. And we certainly saw plenty of weaknesses in our own system. So, a lot of us were interested in trying to find out more about other social systems and how they would work. There were a lot of discussion groups going on at that time. I don't know why I was never asked to join the group, because I felt pretty confident that there was a "Communist cell" being developed on the campus and I was very close to a lot of the people involved in it.

A lot of them were involved in the Bach chorus, which was started about that time. I was actually the first accompanist to the Bach chorus, but it soon got beyond my capabilities and they had to get a pro in. We used to go over to Sidney's to listen to music. Of course, we loved to hear Sidney play the piano. He was the one who introduced many of us to Scriabin; he played Scriabin's preludes, in particular, on the piano extraordinarily well. We would listen to phonograph records together—things of this sort. Shortly after we were married, Virginia and I used to go over to the Weinbaums, 1933-1934.

By the way, Sidney was a very enthusiastic stamp collector. And we also got interested in stamp collecting together. He used to help us with ideas about stamp collecting, so when we were in Copenhagen, we used to buy stamps for him, things that he had a hard time getting in this country but that came into Denmark more readily. He was collecting Russian states, as it were, that no longer issued stamps because they had been absorbed in the Soviet Union—like Azerbaijan; he was interested in the stamps issued while Azerbaijan was independent. As soon as a country quit issuing stamps altogether, then he'd get interested in them. When we came back, we didn't see quite so much of them, because shortly afterwards, in the spring of '38, we went to Redlands.

PETERS: He was married when he came?

WAYLAND: No. I think they met just as he was on his way out of the country. She [Lina] was some distant cousin of his. They just fell in love and decided that as soon as he could arrange it, and she could arrange it, they would get her out. They finally did and they were married over here. She was redheaded, very vivacious; they had one child, Selina, sometime in the thirties. Lina was very intense. Sidney was somewhat intense, but not so intense as Lina.

As I say, I'm pretty sure that there was a Communist cell developed on campus. Calvin Bridges, one of the biologists and a close collaborator of T. H. Morgan's, seemed very interested in it. And Herman Muller, who didn't stay here but was one of Morgan's people—I think he went to Indiana. I think he actually spent some time in Russia but was somewhat disillusioned by it. He also felt that we needed a little overhauling of our social system. There were some pretty intense discussions at various times. I pretty well lost track of Sidney during the early part of the war. Then, of course, the thing that interests everybody about him was this trial, when he was convicted of perjury.

PETERS: When was that?

WAYLAND: That was after the war [1950].

PETERS: Was that in the McCarthy era?

WAYLAND: Oh, definitely; either in the McCarthy era or the lead-up to the McCarthy era. It was a time when there was an awful lot of witch-hunting. At the time, Oppenheimer was under tremendous pressure. Sidney was working at JPL or was just about to get a job at JPL—I think he'd been in the East and came back here to work at JPL. And when he came up for clearance, one of the questions was whether he had ever been a member of the Communist Party, and he said, "No."

As I understand it, Gus Albrecht, who had been a very close friend of Sidney's, and I'd known Gus quite well—his wife was the daughter of the first bass player at the L.A. Philharmonic. We used to see a lot of Gus. And then I lost track of him completely, though he stayed on at Caltech as a research fellow for a long, long time—in fact, I think up to his death, which wasn't too long ago. And I don't know why Gus took such a

strong stand as he did, because he took a terribly strong stand, and I think he was one of the most damning witnesses. I had several chats with Ernest Tolin; I can't remember yet whether he was the prosecuting attorney on the case, but he had great sympathy for Sidney. He felt that Sidney had gotten caught in a web. He was convinced Sidney was guilty of perjury, and he wondered why Sidney never said that he had been a member of the Communist Party. He said there was absolutely not a whit of evidence that anything Sidney had ever done was anything but completely loyal to the United States—that if he was a member of the Party, it was during those idealistic days of the thirties.

PETERS: When it was relatively OK to do that.

WAYLAND: Yes. After all, there was nothing in our laws at that time that was against it at all, and yet it caused a lot of harm to a lot of people, like Frank Oppenheimer. Sidney, of course, was convicted of perjury—not of doing anything against the United States government, or anything of the sort—but pure and simply of perjuring himself on this application. And most of his friends feel that it was Lina who pushed him into it. Lina began to get a bit barmy, you might say. We didn't see much of her; we only had reports. But she got to be a very difficult person as time went on. It's not surprising, knowing how high-strung she was. Sidney was very loyal to her, and we feel that she was the one who kept pushing him not to give in to these people and admit that he was ever a member of the Party, though the evidence seems to be pretty definite that he was. But again, in a perfectly innocent way, and he had done no harm whatsoever. To me, it was a great travesty of justice.

He spent three years, I think, at McNeil Island [Washington]. I had some interesting discussions with him when he came back. He said that frankly, it took a pretty sane person to come out of there sane, or to come out of there anything but a criminal—because it was a beautiful place to learn the tricks of the trade. And in fact, Ernest Tolin said that in his experience, anyone who is sent into a penal institution for longer than three years is almost certainly going to be a criminal the rest of his life. If you cannot do rehabilitation in that length of time, or they can't hang on over that length of time, they're gone. There's practically no chance of ever pulling them out of it. He just felt these

long-term sentences were completely meaningless; in fact, they were dangerous to society. It's true, you may want to put them away to get them out of society; but if you want to rehabilitate them, you've got to do it within the first three years, or they'll never be rehabilitated.

PETERS: Of course, there's some feeling about the differences between people who are in prison for political reasons as opposed to people who are in prison for crime.

WAYLAND: Oh, very definitely. We saw this with Charles Sadron. Now, Charles was here before the war. I didn't know him until I went to Strasbourg in 1953. But Charles had been picked up by the Germans and thrown into a concentration camp. He knew why he was there, and he stood it. He came out—well, he's still alive; he's eighty-one, eighty-two now, and has had a very productive career. He did a lot of sabotage while he was in Dora [the concentration camp]. But he said that people who had no idea why they were there just fell apart. But those who knew why they were there, they could stand up under a good many years of abuse.

PETERS: There wasn't anybody else like Sidney associated with Caltech who went through this witch hunt?

WAYLAND: Well, of course, Oppenheimer went through a terrible witch hunt, but it was at a different level. Frank Oppenheimer did, too. Frank was never sent to prison or anything, but he virtually had to divorce himself from scientific work. I believe he's back in science now. Sidney, when he came out of prison, felt there was no sense in trying to go back to science. But he had this mathematical skill, visual skill, that made it possible for him to go to work for a tailor firm, and he could get about one more suit out of a bolt of cloth than anybody else could. He could just see how to put this jigsaw puzzle together and made himself quite a nice career out of that, and profitable. He's been retired a good many years. I think he and Lina were divorced. I suspect she's dead now. He still sees a lot of his daughter—a very brilliant girl, with a bunch of kids and a bunch of grandkids. In fact, he's got several great grandchildren now. But just from this

one daughter. He's been married to Betty for some twenty-five or thirty years now, and they seem to have a very happy life.

PETERS: Do you think that Caltech was lucky as far as not having problems with this?

WAYLAND: I think part of it was because we had Earnest Watson as dean of the faculty. Earnest was very close to all of the personnel problems during the war and then after the war, he became dean of the faculty. And Earnest was a man who was not going to allow witch hunts, if he could avoid it.

There are some interesting stories about some confrontations between Earnest and some members of the Board of Trustees, for example, who were after Pauling. Pauling was careful to avoid the— There were plenty of appearances, but there was no documentation against him. I think some board members left the board, because either Pauling was going, or they were going. They went. Watson told me one time that, well, he wasn't very fond of Pauling and his position. But he also felt that Pauling's position had to be defended, because there was nothing wrong with his position, even if he disagreed with him. And Watson was that way. We haven't had a man in the top administration since who had the integrity and moral honesty that Earnest Watson had.

J. HAROLD WAYLAND

SESSION 5

January 4, 1985

Begin Tape 5, Side 2

PETERS: Could you tell me something about your family background in Idaho?

WAYLAND: Well, my mother's parents came out from Iowa in the beginning of the last quarter of the 19th century—I'm not sure of the exact date. They were both schoolteachers and settled down in the Boise Valley, down near what is now Meridian, Idaho, somewhere in that neck of the woods. Then they tried going back to Iowa, but my grandfather had such bad asthma, he just couldn't live in Iowa. So, they came back to Idaho. I know when they came out, they could come only as far as Salt Lake by train and then had to come by wagon across to the Boise Valley, because the train hadn't come through yet. And just when they moved to Boise, I don't know. I rather think that mother was born in Boise. She was the oldest of the six children that lived. But certainly, they moved there when she was very tiny or before she was born.

PETERS: And they were sort of one-room-school-type teachers?

WAYLAND: Yes. They were sort of one-room-school teachers for a time. I don't know when my grandfather moved into other things. He was never very tightly oriented towards any particular profession. He sold insurance, and for a time he was a probation officer and did a variety of things of that sort. And my grandmother, as long as I can remember, always took in boarders.

PETERS: Was that at the Hays Street house?

WAYLAND: That was at the Hays Street house. They had a house earlier, over near the center of Boise. But from the time I can remember, and in fact, long before that from pictures we have of the family, by the time my Uncle Micky, who was the youngest of

the family, was maybe four or five, they were already in the Hays Street house—1404 Hays. And my parents, then, built their house just a block and a half away. So, we were all fairly close there.

PETERS: Did your mother teach or do anything like that?

WAYLAND: No. The family wanted everybody to go to college. But at my mother's time, there just wasn't money in the family. So, she went to work. I think she worked in the telephone office. Of course, she was married in the early 1900s, because I was born in 1909; my brother was born about 1903.

PETERS: And your father's background?

WAYLAND: Well, he was born in Boston, and then the family moved to Wisconsin. I remember his telling about the rigorous winters there in Eau Claire, Wisconsin. I remember seeing some report cards from the schools there. And then there was a time when they lived on a farm farther south in the Midwest.

PETERS: So, your grandfather on that side was a farmer?

WAYLAND: As far as I could find out, after they left Boston—I don't know what he did in Boston—he was indeed always on a farm. My dad bought a farm for him in the Boise Valley for a time, and they lived there for some years. Then they moved to Hoquiam, Washington, and he got a piece of stump land there and did a small amount of farming. They raised herbs. I remember that ginseng was one of their principal crops; that seemed to bring in quite a bit of money from the Chinese population. It wasn't a lot of money, but it kept them going. And then Dad's sister moved with them into the Hoquiam area. It was there that my cousin Elmer became the local operator of the movie machines in the local theatres and set up a shop for repairs of electrical equipment. In fact, he's along in his eighties and is still doing shop work, repairing and rewinding motors.

Dad came to Idaho as a draftsman and then took over the architectural firm when the head of the firm skipped the country with money that came from the government as a

payment towards the federal building that was being built there on the corner of Eighth and Bannock. Then he got in touch with Jim [James A.] Fennell—where and how, I don't know, but they went into business together [1904, as Wayland & Fennell]. They decided that they would either stay in Boise or go to Butte, Montana. So, Jim went up to Butte, Montana, to look over the situation there, but just about that time there was a big strike in the copper mines—virtually wiped out the economy of that area. So, Jim came back to Boise, and Wayland & Fennell stayed in partnership until Jim's death.

PETERS: Tell me what it was like going to school in Boise. What do you remember of your elementary school?

WAYLAND: Well, I went to an elementary school that was within easy walking distance of home. We had an awfully good first grade teacher, Mrs. Wickersham, who actually part of the time lived at my grandmother's. She took great care of me and my cousin, Carol Allen, who was just about my age—he was just six months younger. She was a very understanding teacher; she knew when to look the other way when kids got into a fight, and when to step in and separate them. She recognized that I was able to pick up reading fairly quickly. So, they pushed me ahead, and I ended up a grade ahead. After six grades there at Whittier Elementary School, we had to move over to Central School, which was a good mile and a quarter from home. But I always walked it, both ways. And there we had what we called “the department”—that was the seventh and eighth grades. Then our high school there was a four-year high school with quite good facilities.

PETERS: Just one for all of Boise at that time?

WAYLAND: That's right. The second high school wasn't built until long after I left.

PETERS: Can you remember how many were in your high school class?

WAYLAND: Well, there were pretty close to 2,000 students in the high school all told. My class was probably, I'd say, about 500. It was a unified school district, and it brought people in from surrounding communities as well. We had good facilities; we had a good

shop. I remember taking auto mechanics. We had a good forge shop, because I remember using that when Dick Hollingshead and I were preparing the zirconium. We had to reduce it in an iron bomb, which we made out of a piece of gas pipe, and then we had to evacuate it. Then we heated it in the forge over in the forge shop. They had a good old-fashioned coal forge where you could blow air through the coals and get them very, very hot.

PETERS: They taught blacksmithing there?

WAYLAND: They taught elementary blacksmithing, yes. We had a printing plant where you could do a little printing. And you could do a little typesetting. The actual printing of the school paper was done by one of the local newspapers, but we would make it up and lock it up in the forms, and then send the forms to the local printer.

PETERS: So, you set your type.

WAYLAND: It was handset. I never set stories, but I did use to set headlines sometimes. I worked on the paper quite a bit, because at one time, I thought seriously of going into journalism.

PETERS: When did you start working on the paper?

WAYLAND: I think I was working on it the last two years I was in high school.

PETERS: Was that due to just general interest, or did an English teacher or somebody encourage you to do that?

WAYLAND: I don't remember. I know I got interested in the possibility of journalism as a possible profession. And I worked as sort of a reporter on the paper and did some headline writing, and as I say, occasionally we'd even set up a headline when we had to meet a tight deadline.

PETERS: When did you start studying music?

WAYLAND: Oh, gosh, Mother got me started on the piano when I was still in grammar school.

PETERS: Did she play the piano?

WAYLAND: Yes, she played the piano, somewhat—not well, but she played it. She wanted me to take piano. She wanted my brother to, but he couldn't get interested. My favorite time to practice was when the dishes were being done. I would always practice diligently until the dishes were all done, and then I would quit. And then, I was still in grammar school when I got interested in the pipe organ. My piano teacher—Ora Long was her name—started me on the organ, but it soon got beyond her.

PETERS: How did you find out about it?

WAYLAND: Well, I was very active in the Methodist church there through most of high school, until we got into a hassle with the administrator of the youth program. She allowed us to dance one time at a youth party. And then the next Sunday, she came back and put on a real confessional, how she was absolutely wrong to have permitted this and whatnot. And it appeared to many of us that this was so hypocritical that, frankly, it broke our respect for the church.

PETERS: She'd probably been told from above.

WAYLAND: Oh, yes, it was obvious she had been forced into it from above. Although she claimed it was completely her own volition and she had realized that she had done wrong, we were all convinced that it was strict pressure from the elders. Of course, the Methodist church there was pretty straitlaced. And several of us just felt that we couldn't stomach it any longer. That was really when I broke with the organized church and didn't take it up again until after you kids were going to Sunday school here in Pasadena.

PETERS: Were you very interested in Bach or anything like that?

WAYLAND: Not especially at that time, no. I was more of a romantic, with Chopin and things of that sort. But once I got on the organ, I got very interested in Bach because of the contrapuntal structure. It was fascinating structurally. In fact, I would still rather play Bach than to listen to it.

PETERS: Because you become so aware of the different voices.

WAYLAND: Oh, and especially when you could bring in that extra voice with your feet. I used to like to say I just love to play with my feet, like lots of children do. Mrs. Long started me on the organ, then Frederick Boothroyd, who was a very fine organist, had come from England and was the organist at Saint Michael's Episcopal Cathedral. And that's where I actually practiced, because that was a good little organ. He finally agreed to take me as a pupil. Of course, that was a great thrill to me. And I worked with him until he left. I think he went to Colorado Springs from there and made a fairly good reputation for himself when he got into a bigger place. Then some other chap took over—I can't remember who he was; he was nowhere near the musician, but he worked with me quite a lot and persuaded me to do a solo recital.

PETERS: What kind of music were you preparing for that recital?

WAYLAND: Oh, it was mostly romantic. I think I had maybe one minor Bach thing in it; otherwise, it was pretty much romantic and some transcriptions. My Aunt Leora had quite a nice voice. I transcribed an accompaniment or two for her, so she was going to sing. This recital was to come just after I was graduated from high school, 1926. And then I came down with a mastoid infection, and we had to cancel the recital. Then I had to have a mastoid operation, which was a pretty rugged thing in those days; we didn't have the antibiotics. We had a very skillful surgeon there by the name of Jones. He pulled me out of it all right.

PETERS: But you were convalescing from that for quite a while.

WAYLAND: Well, I was always slight and not very athletic. So, we decided that it would be better for me not to go to college the next year, so I laid out for the year.

PETERS: What did you do then?

WAYLAND: I took a job as an office boy in an insurance firm. It was actually Idaho State Life, which was then taken over by Occidental Life, just before I went to work. So, they were transferring all the records from the Idaho Life forms onto the Occidental forms. I spent a lot of my time just proofreading on these transfer records with one of the older employees. The manager seemed to think that I had a certain amount of ability, so he checked out my handwriting, because he thought he might put me into accounting. But my handwriting was so bad that he decided that that was impossible. And also, about that time, I decided that I needed to know typing, so I went to business college. I quit the job and went to business college and took typing and shorthand. I got into a real competition with a girl in the class as to who could get through the shorthand course the fastest. We'd take an exam, and you either moved on or you stayed where you were. I put too much of my time in on shorthand, and not enough on typing, so that my typing never got terribly good, and I never used my shorthand. So, it was a bit of a hollow exercise.

PETERS: Did work at that firm get you interested in law?

WAYLAND: No. The interest in law came earlier. In fact, when I entered high school, I was interested in law and had thought seriously of going into law. So, I started going in for debate and public speaking, and things of this sort. I remember as a freshman entering the extemporaneous speaking contest. I came out last. They would theoretically take one from each class. Then it turned out that they were going to take two from each class. Well, I was not only last, I was second, there were two. So, I found myself in the finals. And I'll never forget my fright at having to get up along with these other seven students to give a talk in front of 1,500 wriggling kids. I was still in short trousers. Fortunately, I came second, but we had to draw a subject. The subject I drew was rural education. And we had an hour, I think it was, to go to the library and sort of get our wits together. Then we had to get up and give this extemporaneous talk. I was glad I was

second, because I was having to hang on to the chair to keep my knees from knocking together. Once I got up and started talking, at least I didn't stutter. But I'm sure, if it had been recorded, it would not be something for posterity to want to listen to. But I did work on the debate team through a good share of high school, never making any very great record, but we got along and had interesting times. Then I got interested in school politics and was on the student council for a time.

PETERS: When did the law interest evaporate?

WAYLAND: Oh, I think it was taken over by science. I was interested in the radio business. In fact, I had already started with the interest in radio when I was still in grammar school. I used to go over there at odd times of day and night to hang around the radio station.

PETERS: The school had a radio station?

WAYLAND: Boise High School had a radio station. Well, Harry Redeker really built it, and I'm sure he put a lot of his own money into it. But he scrounged a lot of equipment out of the Signal Corps from World War I, and he had a spark set, and then eventually got some vacuum tube equipment for voice, and then built the first broadcast station in that area. He managed to get an experimental license also, so he was allowed to do a whole variety of things that weren't normally permitted to an ordinary licensee, which was good for the students, because we could horse around with different things. We built the sound studio down in the basement, had to put the partitions in by drilling holes in the concrete and putting bolts in with their heads down and filling the holes with molten sulfur to hold them in place and then putting the two-by-fours down and putting up the risers and lining it, and then putting in the soundproofing and so on. We got a lot of good experience in a variety of things. But he wouldn't let me take the formal course until after I'd had physics and chemistry. So, I started with physics as a sophomore. I always found mathematics easy. I hated Euclidean geometry; I found it a terrible bore.

PETERS: Well, with your radio, you were involved in putting on broadcasts?

WAYLAND: Yes. A lot of it was phonograph, of course. But we put on live programs whenever we could get the talent—if you want to call it talent; some of it was pretty bad. But we had a regular program. In fact, at the time we had a class-A license, which required that a certain percentage of the time be given to live programs. We couldn't just give canned music. We used to give the stock reports—I don't mean the stock market, but the reports on livestock, because that was important to the local farming community. I remember reading those things by the hour, it seemed to me.

PETERS: Sometimes you had outside speakers on your radio, too?

WAYLAND: Occasionally, yes. I remember occasionally we would have.

PETERS: You said something about [Charles A.] Lindbergh?

WAYLAND: I think that was 1927; that was after I had finished high school but before I went off to the university. Lindbergh came through on a barnstorming tour and we broadcast his talk; it was given at the fairgrounds. We carried our equipment out to the fairgrounds, and we mounted the monitoring system underneath the platform. I was doing the monitoring, adjusting the sound levels and whatnot underneath the platform where he was talking up above. And I still have, I think some place, my Lindbergh coin that I picked up at the time of that.

PETERS: What other kinds of cultural events were available to you in Boise?

WAYLAND: Well, of course, we had a live theatre, the Penny Theatre. And the Moroni Olson Players from Salt Lake used to come through on a regular repertory. They'd give about four plays a year. We had a certain amount of things; like there was the so-called La Scala Opera Company. My first opera was to hear *Il Trovatore* from the peanut gallery.

PETERS: Was that a repertory group?

WAYLAND: It was a repertory group that barnstormed around the country. When I was in the eighth grade, Anna Pavlova was dancing in Boise. My family wasn't interested in such things, so they didn't make any effort. So, I slipped out of school—I must have been about the eighth grade. It was about four blocks to the theatre, I got in line to get a ticket for the peanut gallery for that night—they wouldn't be reserved, but at least I would have been in. But the last ticket was sold about ten people ahead of me, so I didn't get in.

PETERS: Did you have opera singers come around?

WAYLAND: Yes, occasionally. I don't remember how many of these I heard at Boise and how many I heard when I was at the university, but I know I heard Louise Homer several times. And I'm sure I heard her at least once, if not twice, in Boise.

PETERS: What about instrumental music?

WAYLAND: We had our own little orchestra in town. We had a very good chorus, and they always gave the *Messiah* every year. One year, they got very ambitious and did an outdoor concert performance of Gounod's *Faust* with a pantomime in the foreground, and the chorus and singers doing it oratorio-style in the background.

PETERS: You weren't involved with the chorus.

WAYLAND: No, I was involved with the theatre at Boise High School, mostly on the stage crew, but I was never involved with any of the choruses as a singer. I couldn't sing with an owl.

PETERS: Were there things like string quartets or anything like that?

WAYLAND: I don't remember string quartets. The first string quartet I remember hearing was the Flonzaley Quartet, in Spokane, when I was in college. I remember that very distinctly, making a special trip from Moscow to Spokane to hear the Flonzaleys.

PETERS: Was that because somebody you knew was going?

WAYLAND: Well, I had begun to be very interested. I'd gotten interested in classical music and I was buying records. Of course, first of all, they were opera. The first record I bought—\$2 was a lot of money in those days—was Geraldine Farrar and Enrico Caruso in the garden scene from *Faust* on an old Red Seal record. I still have the record. So, I was gradually collecting records. About 1926, when we began to get electrical recordings, the quality of recording went up very, very rapidly, but we didn't have good reproduction facilities yet. But even on the old phonographs, the electric recordings were just better than the old acoustic recordings. So, I began to pick up a record library. Then when I got to the university, I had a chance to hear a lot more music. I had very few friends in my own age group. Most of my friends were faculty members. And I would go to their homes and listen to their records. I remember being crazy about Richard Strauss—*Ein Heldenleben* and *Also Sprach Zarathustra*.

PETERS: Very romantic.

WAYLAND: Oh, absolutely. I'm still a romantic. And I continued my music studies.

PETERS: Were you still doing your organ?

WAYLAND: Yes, I was still playing it, but less seriously. By that time, I had decided I wasn't going to be an organist. Although one of my teachers at the university thought I ought to continue in music. When I came down here to graduate school, I practiced some. I used to go up to the corner to the Holliston Methodist Church and practice some. When I went to Redlands to teach, I started taking organ again from Pratt Spellman, because we had an excellent Casavant organ there. He was always trying to get me to appear in one of his recitals, but I never got around to it.

PETERS: What about lectures and things like that—back to Boise?

WAYLAND: The Chautauqua was very big. In fact, I used to always help put the tent up and take it down again so I could get free tickets to the Chautauqua. And I remember [Arctic explorer Vilhjamur] Stefansson, for example, coming through. We nearly always had some sort of a musical affair, like a Gilbert & Sullivan operetta, with the Chautauqua. And in the winter, we had the Lyceum course. And we had some very good speakers on that—anthropologists and explorers and people of that sort. I don't remember anything very profound politically; there may have been.

I remember very definitely hearing old Bill Borah sound forth. He was one of the great politicians from Idaho. He was in the Senate for years and years and years. He was chairman of the Foreign Relations Committee for a good many years and had never been outside the United States. But he was very well known. And then I remember particularly hearing William Jennings Bryan orate. He was “the silver-tongued orator.” He was down on a platform in front of the capitol building, and I remember being up on the steps. Certainly, there was no need for a public address system for him.

PETERS: I can't imagine how you managed to do all of those things—do all of this radio, putting tents up, etc.

WAYLAND: Well, I did my studying in spare time. Competition wasn't all that stiff. I used my time between classes. For instance, if there was ten minutes between classes, I'd usually get half of an assignment done in that ten minutes. So, I learned to use that time very efficiently.

PETERS: And you didn't have particular chores at home or anything like that?

WAYLAND: No, I had no real chores at home, except to practice the piano when the dishes were being washed. And I would stay after school to do these other things. Some of the teachers would assign what they thought was a fair amount of homework, and for some kids it was. But it wasn't hard for me to do.

PETERS: And then, going to Idaho, probably the competition wasn't that stiff there either, was it?

WAYLAND: Well, I had to work like the devil up there. The shock between high school and the university was almost as great as it was between the university and Caltech. Part of it was because I took a very stiff course. Chemical engineering required a very heavy load of time. And then I was taking music on top of it. In the College of Letters and Sciences, they didn't want anybody to take over eighteen units. I was generally carrying twenty-two for credit; and I think I got only two B's in my entire four years. When I moved into the College of Letters and Sciences—I was making straight A's in a course through the middle of my junior year, and essentially just changed colleges—the dean made me drop four units for credit, part of which was music. My music teacher got very angry at me because I wasn't continuing to take the course for credit and wouldn't take me seriously after that. But it was just a stupid administrative deal, of which there was a great deal at that university.

PETERS: Tell me about the nutmegs. This is high school, right?

WAYLAND: This was in high school. Dick Hollingshead was my lab partner in chemistry during my junior year. His brother John was interested in chemistry in a sense, and he built a little laboratory. He took an old greenhouse at their home and built a little chemistry laboratory. Their father was a wholesale grocer, and they got a bunch of nutmegs that were wormy, so they couldn't sell them. We decided to find out what was in those nutmegs, so we got an iron retort and distilled the darn things, got more interesting goops out of them. And of course, the place stank of nutmeg, and so did we for weeks, as we distilled these nutmegs. I don't know how much of that was distilled worm and how much of it was the essential oils of nutmeg. We never did identify the fractions, but we had great fun fractionating the various oils and so on that came out of the nutmegs, smelling them and collecting them. Well, of course, we also did such things as make pipe bombs.

PETERS: Now, Laurance Hollingshead was one of your best friends.

WAYLAND: Yes, Laurance was another brother of this family. There were five children in the family, three boys and two girls. Laurence was the youngest. He and I were involved together in debate. So that's where we got acquainted.

PETERS: And then Rupert Kent was—

WAYLAND: Well, Rupert Kent was another one of our cronies, and he was very much involved in radio.

PETERS: I had the feeling the three of you were sort of a threesome.

WAYLAND: I worked with Dick on the chemistry, but socially I spent my time with Laurance and Rupert Kent. Rupert's father was a blacksmith, and Rupert was a very good blacksmith himself. But he got involved in selling radios, and he knew the radio ham business very well. In fact, he was the only one of us who was really a licensed ham; I never got my license.

PETERS: You guys used to go hiking and stuff like that?

WAYLAND: Oh, we went hiking a lot, yes, not only with them but with other sidekicks. You see, my folks had a cabin up in the mountains on Daggett Creek. Once in a while we'd go out and sleep on the ground, but more often we'd go up to the cabin where it was more comfortable. My dad loved to camp. And we used to go camping as a family a lot, and he was a good fisherman.

PETERS: I remember you saying you always liked to drive in the mountains.

WAYLAND: That stems from that time, because Dad would take us back into real mountain territory, where the roads were just exactly one car wide. He preferred to drive at night, because he could see the other guy's headlights and then hopefully the right person would decide to wait at the turnout until the other one came by. Sometimes you'd go a couple of miles with the road only just one car wide and a 1,000-foot drop below. It

was pretty wild country. And Dad hated horses. My grandfather had a horse, but Dad got a car pretty early. I can't remember when we didn't have a car, frankly. But I can remember when my maternal grandfather had a horse and buggy in Boise. He had a barn in back of the house, and he kept his horse there and I remember riding in the buggy. But that was gotten rid of fairly early, because the town was small enough you could walk anyplace, except out to the swimming pool, which was far enough that we usually took the streetcar when we went to that.

PETERS: Did you go to hot springs?

WAYLAND: Well, of course, there was a hot springs out by the penitentiary. The hot water was brought in, first to this big swimming pool, and then many of the houses along Warm Springs Avenue, which was on the east side of town, were heated with natural hot water. Often, they used natural hot water for baths. Of course, it stank of sulfur and stained the bathtubs pretty badly. But Boise was one of the earlier places to use natural hot water for heating.

J. HAROLD WAYLAND**SESSION 6****January 7, 1985****Begin Tape 6, Side 1**

PETERS: At the end of the last tape, I was asking you about photography. Can you remember what your early experience was with photography?

WAYLAND: Well, I got interested in photography through owning a Brownie box camera. I remember my most impressive photograph that I took as a youngster was a picture of my father flipping flapjacks in camp. And I caught the flapjack absolutely at the top of its flight, when it was just about a quarter curled over, so it was virtually stopped. Otherwise, I couldn't have stopped it with that Brownie camera. My parents had friends who had a photographic business. So, for a time, they did my developing, and then I took it up on my own, when I was in high school. Eventually I bought an Argus, but I don't remember when that came, my first 35mm camera. I think I was using the Brownie format for a long time—127 or something like that.

I went into flash photography in the days when we used flash powder. We used a magnesium powder. I took some pictures of the radio studio with that, but I could only take one picture. There would be so much settling of the magnesium dust that I couldn't take another picture for an hour or two.

PETERS: You mean because it would fuzz up the air?

WAYLAND: Yes. And then I tried using magnesium ribbon, which was better, because it didn't disperse all over the place. So, I had a little experience with some of these early forms of flash before we had our flashes nicely encapsulated in a bulb. But I never really got into careful photography until much later, when I took up color photography after I was in Denmark in 1937. I bought a Leica and started doing color photography with the Agfachrome, which was one of the first color films for diapositives that was put on the market. I took some Kodachrome with me, but I didn't have much luck with it. But I

had very good luck for the period with the Agfachrome, which was coming out of Germany at that time. I've kept up an interest in photography in an amateur way ever since.

PETERS: But not connected in any way with your professional work.

WAYLAND: Well, yes. Of course, in connection with my work, while I was still at NOTS [Naval Ordnance Test Station], we did a lot of photographic work. Of course, there I had pros to do most of the dirty work for me. We took a lot of high-speed photographs there, with flash and with magnesium flares, and with special rotating disc shutters and things of this sort. So, I did have an interest in photography, but I was more interested in designing the experiment and choosing what we would do, but letting the professional photographers do the actual work. That was pretty much true when I set up my laboratory in microcirculation, the microscopic work. We did quite a lot of high-speed cinematography—up to 400 and 500 frames per second. And again, I was more involved with choosing the equipment and seeing to it that the electronic interfacing was properly done. But I didn't do an awful lot of hands-on work.

PETERS: Another thing I've been wanting to ask you about was the group of people that you lived with when you were in graduate school.

WAYLAND: Oh, yes. Well, this was an interesting group. I had been in class with some of them. Henry DeVore came the same time I did in 1931. And Dwight North and Everett Cox had been here for two years, I believe. That was in the days when you could finish a doctor's degree in three years if you were gung-ho and had a good background. Don and Everett had lived together for a time. And then they were able to find a house on the northwest corner of Catalina and Del Mar; I believe a faculty member now owns that house. Anyway, Everett and Don found they could rent this house for all of \$40 a month, and half of that was more than they wanted to pay. Well, they talked Henry into joining them. So, then they wrote me—I was in Idaho at the time, visiting my family—and they wanted me to join the group. Part of the price of admission was not only my \$10 a month but the fact that I had to do my share of cooking. So, I agreed and had my

mother give me a master course in cooking for three weeks. When I came down and joined the group, we had an arrangement whereby we worked in pairs. Henry and I made a pair, and Don and Everett made a pair. And the pair was responsible for dinner on a certain night and lunch the next day—breakfast was catch as catch can. And whoever did the cooking was responsible for washing his own dishes, so that you weren't tempted to dirty everything in the house. The name, Cowanode, came from Cox, Wayland, North, DeVore. And of that group, there are just two left. Dwight North—better known as Don—has retired as a theoretical physicist with RCA and is still living in Princeton. And myself. Henry died several years ago, in the middle of a folk dance from a massive heart attack. He was a very lively person. He was best man at our wedding in '33.

PETERS: So, you were only together a year?

WAYLAND: We were only together a year. Well, I got married in the spring, and Don and Everett left. Henry stayed around. Henry had come without a fellowship—he was the oldest of our group, actually—but he did very well and was able to get a teaching assistantship after the first year.

PETERS: So, what other activities did you do together, besides cooking?

WAYLAND: Well, of course, we liked to entertain the girls. We used the food as a bait, as it were. We had a lot of contact with what is now Pacific Oaks. It was then Broad Oaks—I remember we used to talk about the oak broads at Broad Oaks. Not only the girls, but Don, in particular, got to know the headmistress quite well.

PETERS: The girls were the teachers?

WAYLAND: No, these were students. It was sort of a training school for nursery schoolteachers. So, it was at the university level but at the earlier university level. Although I haven't kept in touch with any of the girls from there, Don sees some of them every once in a while—he tells me about that. And I met Virginia that fall, and from then on out, she was my date most of the time. The boys had a car; that is, Don and Everett

had a car, which was called “The Cnorx.” I used to borrow it once in a while, but I couldn’t afford the gas very much. So, I used to travel around mostly by roller skates. I used to roller skate down to Alhambra to see Virginia, for example. I would usually try to persuade her to drive me home, because she had a car, and it was easy enough to skate downhill, but it wasn’t much fun to skate back uphill.

PETERS: Would you go on picnics and things like that?

WAYLAND: We went on picnics, and we went to dances. There were dances at the Civic Auditorium, in the flat hall behind it. And every week, there were very good dances there. We used to enjoy that very much. I remember one picnic, when I was to bring the potato salad. And I had made a bit of a reputation for my onion soup. Well, I figured, for onion soup I used one onion per person, so for potato salad, maybe half an onion per person would be right. [Laughter] Well, it was a memorable potato salad. I never got over being kidded about that. And then the first lemon pie I made. The lemon custard didn’t cuss, and it ended up by being lemon soup with meringue croutons. [Laughter] It looked beautiful. Sort of a lemon floating island.

PETERS: And what about your literary activities at the Cowanode? Was that just sporadic?

WAYLAND: Well, that was mostly Don and Henry; they were very interested in rolling off verse and so on. Henry also did some versification with Bob Martin. I remember probably the most famous of all the verses they produced was the one about Aimée Semple McPherson. Let’s see:

From North LA to Pedro Bay, they come to hear that person;
 All souls contrite, you’ve guessed it right, our Aimée S. McPherson.
 No Magdalene, wipe off that grin, not fond of fleeting fashions;
 She did her best to keep suppressed the basic human passions.
 One summer’s day her fancies stray, desire becomes specific.
 To shut up shop and gaily flop about the calm Pacific.
 A swimming lark at Ocean Park, for lost her flocks bemoan her,
 When lo, a track of Amy Mac turns up in Arizona.

“I’ve been shanghaied,” poor Amy cried, twas all that she could utter,
Despite the doubts of ribald louts, whose minds were in the gutter.

PETERS: This refers to her having—

WAYLAND: She disappeared. And they suspect she took off on a bit of a lark with one of the men in her church—I think it was the controller, or something of the sort. Well, I’ll have to try to remember the rest of that, because I don’t think it’s ever been recorded in the Caltech annals, and it ought to be.

PETERS: Well, the other one I always remembered, was your limerick for Lavoris.

WAYLAND: Oh, yes. There was a contest for Lavoris mouthwash. We had to complete a limerick: “There was a young man from St. Paul, caught a cold each year in the fall. He took some Lavoris, da *dada* da *dada*, da *dada* da *dada* da *dah!*”

So, we finished it in two different ways: “He took some *Lavoris*, but withered before us, and they carried him away in a pall.” And the other one: “He took some *Lavoris*, but his system was porous, and he dribbled all over the hall.” [Laughter]

Somehow or other, that was in the days when comic advertising was not very acceptable. We didn’t get very far with it.

PETERS: You actually submitted it?

WAYLAND: Oh, yes, we submitted it. [Laughter]

PETERS: I also want to ask you some questions about your time in Copenhagen, like the pension you stayed in.

WAYLAND: Well, we found this Pension Bonde, on Fridtjof Nansens Plads. The Plessets had stayed there. Milton had been in the Niels Bohr Institute ahead of me. So, they told us about it, and we were able to get in all right. It cost us all of about \$56, \$57 a month for the two of us for a room and full board. Of course, we didn’t have a private bath. I could have had five meals a day. I mean, I could have had a mid-morning snack and a

mid-afternoon snack. Although I wasn't far from the Bohr institute, I normally carried my lunch. So, I'd go over in the morning—it was dark, of course, in the winter when I went over, and dark when I came home. We had bicycles, but it was too difficult in the middle of winter to ride the bikes. It was always great fun; I would walk past the little lakes and watch the ducks try to land on the ice and skid into a three-point landing, and then get up and shake themselves and try to appear as if they hadn't done anything out of the way.

PETERS: What other kinds of people were living in that pension?

WAYLAND: Well, the one who had the most influence on me was Melvin Knisely. Melvin was there working in August Krogh's animal physiology laboratory. August Krogh was a Nobel laureate for his work in microcirculation. Mel had come over from the University of Chicago, where he was in the department of anatomy, and he and his wife were living in the same pension. He had been trans-luminating tissue by using a quartz rod which would act like a light pipe. He felt that, well, here was an application of physics; physicists ought to be able to be of some help. So, he tried to persuade me to get interested and even got me into Krogh's laboratory one day—his lab and Bohr's were back-to-back. He showed me the microcirculation in the glomerulus of a frog kidney. It was very fascinating, and I felt that this would be a fascinating area in which to work. Mel wanted me to work with him, but of course, in those days, we didn't have the kind of money for research that is available now. Travel money was virtually unattainable. And then, of course, the war came along shortly after that. Mel spent most of the war years working on the problems of the microcirculation in *P. knowlesi* malaria. And he and one of his students, Edward Bloch, did some very, very fine work on the way the parasite gets into the blood cell and how it propagates there and so on, and the influence of that on the flow of blood in the microcirculation.

But then, after the war, Mel got paranoid about the A-bomb, and he decided that Chicago would be a ripe target. So, he got out of there as soon as he could and went to the University of South Carolina. Well, even though transportation began to open up, that made it even more difficult to make the connection and make the trip. I was down a

couple of times to give lectures, but otherwise I'd see him at meetings once in a while. So that never really developed, but it did stir up interest to some degree, and in fact, in the long run it proved to be a strongly determining factor. Because I used to borrow films that Mel and Ed Bloch made to show to various people. In fact, I gave a barnstorming lecture tour, just before I left the Naval Ordnance Test Station, at the end of 1948, just before I went on the Caltech faculty. And I had three lectures: One was on the hydrodynamics of water entry, one was on the problems of constructing the variable angle launcher out at NOTS, at Morris Dam, where we built this big bridge girder affair. I was the primary instrumentation specialist on that development—not the structural, but I was pretty well acquainted with the structural part. And the third lecture was on blood flow and the problems of deformability of red cells and whatnot. I didn't know much about it, but already in '48 I was trying to stir up interest in that field. It was some years later that I actually got involved personally.

PETERS: It's amazing how long those things cook, isn't it?

WAYLAND: Yes, it is. But it was just the happenstance of Wally Frasher coming along and wanting help—wanting advice, first of all—and we decided to work together. And that's where it developed. Of course, I'd already gotten involved in biological fluid mechanics before that. With my time in Strasbourg, when I was working on streaming birefringence, I began to realize that the tool of using fluid-mechanical methods to get information about the size and shape of large molecules through a combination of fluid mechanics and optics had great power, both in streaming birefringence and in light scattering. And then when I came back from that time, we continued the work on flow visualization using streaming birefringence. But I got acquainted with Jerry Vinograd, and Jerry was very interested in the use of hydrodynamics as a tool for interpreting macromolecular behavior. So, I started attending Jerry's classes, occasionally giving a lecture for him, and got very interested in this whole question of how you characterize a macromolecule by using these optical hydrodynamic techniques. Also, the question of sedimentation in the ultracentrifuge, because Jerry pointed out that if you take a very large molecule and study how rapidly it moves in a given centrifugal field, with a given

viscosity of fluid in which it's moving, if you achieve that viscosity as measured in a viscometer by using sugar, you get one result. If you achieve the same bulk viscosity by using a large molecule like serum albumin, you get a different result. So that the local viscosity of the particle fields is not the bulk viscosity that you measure in the viscometer. And we're running into exactly the same types of problems in blood flow. We're finding that the forces required to move the flow of red cells in tiny, tiny vessels are very different from those you would interpret if you took blood of the same concentration of red cells and measured its viscosity in a viscometer. It's very interesting how these things carry on into quite different fields. But it was from that I got going on trying to understand more clearly the local viscosity felt by one population of molecules in a solvent of very small molecules, if you also add a third population, you might say, of molecules of intermediate size. That actually got me my first NIH grant, and I was actually working on that grant when Wally came along.

PETERS: Well, I want to go back to Copenhagen again. Tell me about the Green and Happy Frog.

WAYLAND: Oh, the frog wasn't in Copenhagen. That was in Stockholm. We went up to Stockholm; we took a three-week trip into Sweden, partly because we wanted to see other parts of Scandinavia—I was there as an American-Scandinavian Foundation Fellow—and partly because Virginia was very interested in weaving and was doing a lot of weaving in Copenhagen. And of course, Sweden is famous for its weaving. So, we made contact with the Swedish Traffic Association, and they set us up with contacts in various parts of Sweden to visit weaving establishments. But they also set me up with a contact at the university. So, we talked to some of the people on the science faculty there at the university. And they said, "Well, look. This is just the time for Walpurgis Night, the 30th of April. We're having our big party." So, we were invited to come. Well, I didn't even have my tuxedo with me. Though I'd been wearing it a lot in Copenhagen, I didn't think I'd need it in Sweden. And they said, "Oh, that's fine." Well, Virginia and I came in very informal clothes. And she was taken over by the president of the honor society, the Society of the Green and Happy Frog—in tails, mind you. And I had a nice young

Swedish girl that I was supposed to escort—though her English wasn't as good as Virginia's escort's English was. It was the first time that I'd ever had a chance to get involved in standing on the chairs and singing drinking songs. We couldn't sing them, but we could pretend we were. We'd wave our glasses. The science faculty was the biggest, so they were in the student union, but each group of students had its own party. But they all came together—that is, those that could still stagger—sometime after midnight to join the party in the student union. Well, we had to catch a train at eight the next morning, so we left about two. Well, we couldn't get transportation back to our Salvation Army hotel. It was interesting to watch some of the people having difficulty even climbing one curb high on their way back to the main party. And I guess it went on until well after dawn. But it was quite an experience. It was sort of like a party out of *The Student Prince*. It was fun.

PETERS: When you were in Copenhagen, at the Niels Bohr Institute, did you work with Bohr himself at all?

WAYLAND: Very little. No. He was gone a good deal of the time. He was in Japan a good deal of the time. I worked most closely with George Placzek and to some extent with Victor Weisskopf. Those were the two I had the most contact with. I talked to Bohr occasionally about problems and so on. But he was not there continuously enough.

PETERS: So, he didn't run his institute with a particularly tight hand?

WAYLAND: No. People were completely free to do what they wanted—at least if they showed any capability. [George de] Hevesy was more or less the acting director when Bohr was out of town. He was a famous Hungarian atomic chemist. He was a very wonderful person; everybody liked him a lot.

PETERS: Do you feel you gained a lot in terms of attitudes toward doing science from your time there?

WAYLAND: Well, it made me realize that I was probably not cut out to be a pure theoretician. You see, I came to Caltech with the expectation of doing theoretical work. [Gustaf Wilhelm] Hammar [physics professor at Idaho] wanted me to do theoretical work, because I was strong in mathematics and was the best theoretician they'd had around Idaho. Of course, that didn't show much competition. But Millikan didn't believe in people doing theoretical work, unless they were—well, I always liked to say that there were two criteria. If you were an obvious theoretical genius, like Bob Oppenheimer, sure, you could do theoretical work. Also, if you broke so much equipment that you didn't dare work in the laboratory, like Bob Oppenheimer, you were also allowed to do theoretical work. Well, I wasn't that clumsy or that bright, so I got pushed into experimental work, which I think was good. And when I moved into blood flow, I expected to go into theoretical work, because I felt that that's where I could make the greatest contribution, since I didn't have the experience with animal experimentation, which I thought was important. But I soon realized that there was so much ignorance, we just had to have a stronger background in experimental data. A whole bunch of chem engineers had jumped on the blood-flow bandwagon about that time—the late fifties, early sixties. And they were putting out more trash. Well, a good many of them were people who just weren't good enough to make a go in their own field. There were a few who were and did very good work, like Ed Merrill at MIT. But an awful lot of them were just trying to find someplace where they could get an outlet. Well, they could get papers published, because nobody knew enough to criticize them. But now they're in the trash barrel, because they didn't contribute anything. They were based upon completely false assumptions—had no connection with reality at all.

PETERS: But then in Copenhagen, that's where you decided to stick with theory?

WAYLAND: I decided to try theory. And I spent the eight or nine months I was there doing theoretical work, but it was essentially mathematical grinding. We didn't have the computers. Everything had to be done by hand. I had an assistant by the name of Smed who did most of the calculations for me, and Placzek seemed to use the stuff we did pretty consistently through the war.

Bohr didn't get out of Denmark until sometime during the war. I guess he had a pretty close squeak getting out. I had an interesting experience with him, actually. I was traveling east, going to Washington on the train. I was sitting in the dining car with Fred Lindvall—Fred was looking down the length of the car and I was facing the door. Some people who had gotten on at Santa Fe or someplace up in there had finished dinner and were walking out. And I said to Fred, "That looks like Niels Bohr." All I saw was his back. And he said, "Well, I didn't know him well enough; I wouldn't have recognized him." So, then we went back into the club car, and there were just Fred and I there. I was writing a letter to Virginia telling her that I was sure I'd seen Bohr on the train, when in he comes. So, I went back to talk to him, and he was very chary about admitting his identity. But finally, there were just the three of us in the car, and I said I was working at Caltech now, where Charlie Lauritsen was, and whatnot. And finally, he said, "Yes, I'm Niels Bohr, but don't tell anybody you saw me." So, I tore up the letter I'd written Virginia and never said anything about it until after it came out officially that Niels Bohr and "Mr. Baker" were one and the same person. He was in this country working at Los Alamos.

PETERS: I remember your telling some little stories about the food you encountered in Copenhagen.

WAYLAND: [Laughter] We were invited to the home of the American vice consul for dinner, shortly after we got there. He was a Californian, had been on the faculty of the University of California, and his wife, Carmen Bland, was the daughter of the former conductor of the Pasadena Symphony. They invited us up to dinner, and they had a Danish maid who had produced the dinner. When the dessert was served, we noticed what looked like fly specks all the way through the frosting on the cake. We knew the Danes had great reputation for cleanliness, and we were sure our American friends would, but it did look a little bit disconcerting. We finally learned that they used bean vanilla, and the tiny little seedlets from the vanilla were strewn all through the frosting.

PETERS: Tell me a little bit about your time in Redlands, and what it was like working at that institution.

WAYLAND: Well, the big problem there was that it was tied to a very solid Baptist group. They didn't believe in alcohol in any form. Well, I thought they were hypocritical, because we were supposed to take some sort of a statement of allegiance to the Baptist version of Christianity and whatnot, but I had friends on the faculty who were in the religion faculty, who said, "After all, Roger Williams believed in real freedom of thought. And although it's been badly corrupted by later development of the Baptist church, if you really go back to the origins, it's about as free in principle as any faith within the Christian group." The president couldn't quite say it in so many words, but he implied that some of these things didn't have to be taken as literally as many of the board of trustees thought they should. But it was always an uneasy feeling about that.

PETERS: Did it impinge more on your personal life than on your academic life?

WAYLAND: Oh, yes, entirely. For instance, if we had any wine or liqueurs or anything, we always took our empty bottles in to Pasadena and put them in Bill Pickering's trash bin. We didn't dare dispose of them in our own trash bin there in Redlands.

PETERS: But you did encounter some interesting faculty friends.

WAYLAND: There were some very interesting friends there. Earl Cranston, who eventually went on the faculty at Claremont Colleges. And Neal Klausner, who went to Grinnell and was there for many years. All of them are retired now. Dick Beaman, who came to teach art. And then Jeanne Hayes, who came to teach dance—she'd been premiere danseuse with the San Francisco Ballet, just as a youngster; and then she decided that the life of a professional dancer was just too strenuous and went back to college and finished at Mills, and went on for her master's, and then came to Redlands for her first teaching job. And she met Dick [Beaman] there, and they were married. Dick jumped into the Navy—he was more or less supposed to get a commission—but he had to jump in as an enlisted man. And while I worked for the 11th Naval District, I arranged to have him assigned to us as an artist. So, he did a lot, while he was still wearing an enlisted man's suit. He did the illustrations for the booklets we were putting out in connection with the degaussing station where I was, at San Pedro. And then his

commission came through and he became a photographic officer. I gave him something that kept his fingers into something near to his own field until the commission came through.

PETERS: And there was a Nadine—

WAYLAND: Oh, Nadine Cragg. She was the PE instructor. And she was Jeannie's boss, as it were. We still keep in touch with Nadine. She's pretty well gone mentally now, we understand; we haven't seen her for a year or two.

PETERS: And Earl Cranston's field was what?

WAYLAND: He was an historian. We had an interesting discussion group. Dick wasn't in it, but Neal and Earl were. And the Unitarian Universalist minister in Riverside, and some of the people from the citrus experiment station at Riverside—we used to get together about once a month for a discussion group. Somebody would give a paper before supper, and we'd carry on the discussion through supper, so we were through fairly early.

PETERS: About how many people were involved?

WAYLAND: About fifteen, something like that. We used to go over to Riverside quite often through the contacts we made, largely through this group. In fact, one of my University of Idaho colleagues and his wife, who was one of my high school classmates, were at the citrus experiment station there.

PETERS: And a lot of these people you've kept contacts with ever since.

WAYLAND: Well, the ones at the university, yes. Of course, I kept up contact with Earl until he died—he's been dead some years; his widow's still alive, though. And the Klausners we see virtually every year. The Beamans we see nearly every year. He left

Redlands to go to Carnegie Mellon, and then he retired from there. And Jeannie was teaching at Pitt.