NORMAN R. DAVIDSON
(1916-2002)

INTERVIEWED BY
HEIDI ASPATURIAN

August 17 and 19, September 3, 1987

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area
Chemistry, chemical biology

Abstract

An interview in three sessions, August and September 1987, with Norman R. Davidson, Chandler Professor of Chemical Biology, emeritus, in the Division of Chemistry and Chemical Engineering. He received his BS (1937) and PhD (1941) from the University of Chicago and a BSc from the University of Oxford (1938). He came to Caltech as an instructor in 1946, becoming a full professor in 1957 and Chandler Professor in 1982.

He recalls growing up in Hyde Park, Chicago; his years at the university; his Rhodes Scholarship to Oxford. Discusses his wartime work: with Anton Burg at USC; recruitment by Harold Urey for uranium isotope separation at Columbia; stint at University of Chicago's Metallurgical Laboratory on the plutonium project under Glenn Seaborg. Postwar move to RCA Labs, Princeton, working on electron microscopy with James Hillier.

Recalls the chemistry division, Linus Pauling, and Robert A. Millikan, among others, during his early years at Caltech. His interest in organometallic chemistry, gas-phase reaction mechanisms, formation of complex ions in solution. Recalls

serving on the Freshman Admissions Committee; designing flash-lamp photodissociation apparatus; work on dissociation by shock tubes with grad student Tucker Carrington. Growing interest in molecular biology; attending 1958 NIH biophysics conference, Boulder, CO; the evolution of chemical biology.

Discusses work of 1968 presidential search committee and Harold Brown's selection; advocacy of an enriched humanities curriculum; his support for proposed affiliation with Immaculate Heart College. Recalls three of his outstanding postdocs/graduate students: James C. Wang, Phillip A. Sharp, Ronald W. Davis. Offers his views on Linus Pauling in an appendix.

Administrative information

Access

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ORAL HISTORY PROJECT

INTERVIEW WITH NORMAN R. DAVIDSON

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

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Pasadena, California

Session 1	August 17, 1987
Session 2	August 19, 1987
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Begin Tape 1, Side 1

DAVIDSON: Before you start asking your questions, I'd like to say two things that I've thought of, which I'd like to have in the record for my sake and for my future use of these transcripts. I've thought quite frequently that as a member of the National Academy of Sciences, when I die the academy will follow its custom of getting some unfortunate soul to write a biographical memoir, which I regard as a major imposition on that person. I've often thought that when I did have time, I would like to write my own biographical memoir, so that all somebody would have to do would be to edit it down to an acceptable length—it will probably be too long. So I appreciate this opportunity to get things sort of sorted out, partially, for this purpose.

One other thought I've had is that I'm far from really retired at present, because even though I'm emeritus I'm engaged very, very actively in a research program as a co-principal investigator with Henry Lester [Bren Professor of Biology] on the molecular biology of certain kinds of genes in the nervous system. So I really wish this was all happening four or five years from now, when hopefully I wouldn't be doing anything else. But as I've tried to think back to recall various things, I realize they're slipping away, and so it's probably better we try to do it now.

ASPATURIAN: Tell me a little bit about your family background and the circumstances in which you grew up.

DAVIDSON: My father was a Russian Jew who came to this country in the late nineteenth century—I don't know exactly when—because he said he wanted to escape from the rabbinical atmosphere in the village where he lived, which was somewhere in the Kiev area, but I don't know exactly where. We never had any knowledge of his family background.

ASPATURIAN: He emigrated alone? No family?

DAVIDSON: He emigrated alone.

ASPATURIAN: Your mother was born here?

DAVIDSON: My mother was from a large Lithuanian Jewish family, centered around Philadelphia. She was born here, the second generation. Her parents came, and that was a large family. My father came over at something like sixteen or eighteen. With regard to his profession, I recall several things about him. One was, he said that he was really quite good as an insurance salesman. In some respects, he regretted in his later life that he didn't persist in that, because he said if you become really good at that and you're dealing with wealthy clients and you play golf with them and things like that, it really is an easy way to be well off and lead a satisfying life. But what he did do to find a profession was to take a pre-dental course, which was a way for a person with his status—without a college degree—to get into a profession. The course was only two years, and then he got into some dental school in the Philadelphia area. After a few more years, he was able to practice dentistry.

Then my parents came to Chicago. He had his office in the area known as "back of the stockyards," which was really quite prosperous in the early days. And he had a very prosperous practice. I recall that during the Depression, life was difficult but we were never in very bad shape. Of course, during that time he had a lot of clients who couldn't pay their bills, and it was long, hard work.

He was an extremely intellectual person. He had friends with whom he discussed philosophy. They were quite interested in the socialist movement, and even more, for a while, in the anarchist movement, which they eventually realized the futility of.

ASPATURIAN: Was he a Zionist?

DAVIDSON: No, not at all. He was rather anti-religion, in general. We basically grew up with very little religious background. We occasionally went to a synagogue, because there was a very intellectual rabbi there and my father enjoyed his sermons. I recall two other things—possibly somewhat out of order. He said to me once when he was fairly along in years—he died when I was in high school, because he was really quite old—that in retrospect, now that he could see everything, the most interesting profession to be in was that of a scientist. It would have been quite difficult at that time to have entered that, but he said that that really was the most interesting profession you could be in.

ASPATURIAN: Did he say why?

DAVIDSON: He just said there was really very little challenge in dentistry, and here was something that was intellectually challenging. I can remember what his best friend, Joe Goldman, another philosophical man, once said to me many years after my father died. I had just returned from being a visiting professor at Harvard and had declined an offer of a position there. Joe said, "If your father only knew that, he would be very, very happy."

I was raised in an environment where people respected reading, respected books. My mother was very concerned about my schoolwork and was very supportive. She typed my themes for me; she criticized them. I think I did all my schoolwork independently, but I got maternal-affection rewards for being a good student, and I was a good student all throughout school.

We lived in the Hyde Park neighborhood in Chicago. It was not usual at that time, at least in my social group, for people to go away to college—apart from the fact that you couldn't afford it. A very large number of people—the bright students from Hyde Park High School, in that neighborhood—all went to the University of Chicago. Hyde Park High School was a very good training center for educating students who were very successful at the University of Chicago and went on to careers in math and science. One got scholarships and could live at home, so it really didn't cost very much.

As far as my friends were concerned, it was understood that you went into science, because there was less discrimination there than, at least, in the business world. Of course, one of the things that people in this social group of Jewish parents wanted their kids to do was to become doctors and lawyers—although I don't recall that my parents had much respect for either the legal or the medical profession and I knew very few people who did that. I don't understand why, now that I think about it. All my friends were people from a similar background. We all wanted to become scientists.

The University of Chicago had the first "New Plan" at the time. Robert M. Hutchins was the president, but this was the pre-Hutchins New Plan, with a much greater emphasis on an overview of modern knowledge in general than on the great books, humanities, and philosophy, which was more what the [Mortimer] Adler-Hutchins education centered around the great books was. We had an excellent survey course in biological sciences, which was my introduction to the subject. And I remember a number of highly exciting lecturers—Ralph Gerard and A. J. Carlson, the Swedish physiologist who came to this country at age sixteen and who at age sixty still talked like a Swedish janitor. I was fascinated by the specificity of biological systems and their ability to carry out highly specific complex chemical reactions and organize a complex organism in ways that must have a fascinating molecular basis. I really thought that was the most interesting problem in the world. I should say that as far as general education was concerned, there was a superb humanities and social science survey, from which I learned almost everything I know about philosophy and appreciation of poetry, and a good deal about literature, and received a moderately good education in economics and those fields. There was actually a rather bad survey course in physical science, but I didn't need that.

Also during my freshman year—1933—I was taking chemistry courses and math courses, which were the things to do. When I was a sophomore, two things happened. One, I took my first real biology course—not the survey. It was called Introduction to Zoology, or something like that. And I couldn't pith a frog; it was terrible. Actually, I was sort of clumsy in the lab, and it was one of the few courses I got a B in. I guess it wasn't a very exciting course, in all fairness to myself. I also took a biochemistry course—that was actually a little later—from a famous sex-hormone chemist named [Fred C.] Koch. That was pretty dull, also. The details of what we knew about biological molecules then were not exciting, and in some ways, perhaps, it was more exciting to shift into the field, as I did, two decades later.

At the same time, I had some fascinating chemistry courses. Julius Stieglitz was quite old; he was a distinguished organic chemist of the German style who was a fascinating lecturer. I had a course from him in organic chemistry when I was a sophomore. Frank Westheimer, then a very young instructor at the University of Chicago—now retired from Harvard, a famous guy in the field—came and gave a very exciting course on introducing the new ideas of physical organic chemistry to students at Chicago for the first time. I decided this was fascinating, that I really couldn't understand biology the way I wanted to until I understood chemistry. So I made chemistry my major. I never had officially declared biology my major, and you didn't decide until the end of your sophomore year at that time. Then what happens is that with exciting courses, you get exciting ideas of your own for research. Speaking generally, I got entranced with so many problems in chemistry that I didn't have a chance to get back to anything in molecular biology or biochemistry until much, much later.

Now, specifically, I can recall that somewhere in my junior or senior year I heard about the substance B_2H_6, diborane, which was being studied by Hermann I. Schlesinger, a famous old German inorganic chemist at Chicago. It has this unexpected property of being B_2H_6 rather than BH_3, which is what you would have expected from classical electronic considerations. Also, trimethylaluminum existed as a weak $Al_2(CH_3)_6$ complex. Boron with three methyl groups did not form $B_2(CH_3)_6$; it had the "normal" structure, $B_2(CH_3)_3$. So I got this bright idea that there ought to be such a compound, and that at least boron trimethyl and aluminum trimethyl ought to pair with each other. I decided that I was going to do some graduate work on it. I didn't quite know what I was going to do, but anyway that was an exciting idea I had, which we'll come back to later.

I was a very good student. I was not involved in any activities, particularly, at the University of Chicago and certainly not in any significant leadership role. I knew a lot of people and discussed things with a lot of them. But then somebody said, "Why don't you apply for a Rhodes Scholarship?" Here I am, a financially poor and inexperienced guy who had no idea of what he's going to do for graduate school. In fact, I even took the exam for Annapolis. Thank goodness somebody did a little better than I did, because I don't know what would have happened to me. I might not have had the guts to turn it down, and I don't know if I would have had a strong enough personality to have survived an Annapolis education.

I interviewed for this Rhodes Scholarship thing. They asked me why I was interested in science and what other interests I had. I knew I was highly articulate and effective in expressing my ideas. But here were all these other guys, from all over the Middle West, who were majors in political science or were athletes. As it turned out, I was one of the two Rhodes scholars selected from the Midwest. The way that works is that there are two stages in the interviews. There's a group of six states and there are two nominees from each state—namely, twelve—from which a second interview selects a group of four. There are thirty-two Rhodes scholars altogether. I was one of those.

So, in 1937, I get on this boat with a bunch of other guys and go to Oxford. I was sort of timid; I didn't really feel I could cope with the world as well as some of them could. I was fairly inexperienced; I'd never left Chicago, significantly. I had always been at home, had never managed my own money, anything like that, and certainly hadn't learned to spend money the way my kids did later. In general, scientists chose to continue their graduate work at Oxford, whereas people going into nonscientific fields generally took a second undergraduate degree so they could take advantage of the special features of the Oxford undergraduate education, where you did a lot of independent reading, had a lot of discussions with your tutor, and wrote a lot of papers. For scientists, the first degree is equivalent to a master's degree in this country, but they call it a BSc. British students at Oxford do it for their fourth year of undergraduate study. So I just did that.

There were several exciting people at Oxford. There were a couple of very good people in reaction kinetics—C. N. Hinshelwood and Ronnie [Ronald P.] Bell, whom I met and talked with a lot. But the tutor in my college was Leslie Sutton, and he was a protégé of N. V. Sidgwick. Sidgwick's research wasn't all that great, but he was a leader in synthesizing much of what was known about chemistry and interpreting it in terms of structure. These were the days when the revolution in applying molecular structure ideas and quantum mechanical ideas was introduced into chemistry, with the leading figure by far being Linus Pauling. Sidgwick had good intellectual relations with Pauling, appreciated his work, and used it. He also used the work of some of the more qualitative, non–quantum-mechanical English theorists, who thought of things in terms of similar qualitative ideas. Chicago was notably deficient in that area of modern chemistry, so this was very, very exciting for me.

I was a terrible research student to begin with. It took me nine months before I could do anything in the lab. But I eventually became quite good at it, because I learned to keep thinking about what's important to make an experiment work. I think I became quite a good experimenter in spite of not being manually gifted. I did some dipole-moment work with Sutton and then started out on a structural problem related to my interest in the trimethylaluminum-trimethylboron problem, because he was doing electron-diffraction structural work.

So we went through 1938, the last year before World War II actually started. First there was the Munich scare, and we all didn't know what to do. You just stayed home and worried: Would there be an air raid? Would there be a war? I came home for the summer of '39 just to see my family, with the intention of going back. I had something like a September sailing on a boat back, and that was the first sailing that the State Department canceled. The situation looked so threatening that the State Department wasn't letting people go. My friends who had stayed in Europe for the summer did manage to have one more year of work with very little interruption, until the very serious air raids started the following spring, along with the debacle in France.

I came back to the University of Chicago. They actually offered scholarships to any Rhodes Scholar who wanted to come, so I had a scholarship to continue my graduate work, and I could continue at the chemical level exactly what I had been doing at the more structural level at Oxford.

ASPATURIAN: What was it like to be in Britain just before the Second World War, and what were your impressions of your fellow students and teachers?

DAVIDSON: I have very warm impressions in immediate response to that. I don't have much of a feel for the sense of concern about air raids or the war. I had very good social and intellectual relations overall with a number of my fellow students—some in medicine and some in humanities. I had a number of good acquaintances. I had a very close friend who was a humanist, a very good Catholic, and we went bicycling together in France. While he would pray in the cathedral, I would look at it, and we'd both go in and look around and have a marvelous time. In general, my experience was that the most articulate and noticeable political sentiments—there must have been some strong sentiments that were not identical to these—were expressed by people who were very left-wing, very anti-fascist, and quite frequently Communists. And in fact, our attitude toward what was happening in Nazi Germany and in Europe was consistent with that general evaluation. I went to a few meetings of a Communist Party chapter there. In fact, I remember being quite concerned later, whether this would be something that would be dug up by Senator [Joseph R.] McCarthy, who would then ask me what other Americans were there. That would have been a very difficult situation for me, but it never happened. I guess I was pretty discreet for a while because of my concern about that. I participated in the general left-wing, intellectual attitude toward Hitler, toward the British government, and toward the general approval of the Soviet Union, and an ability to excuse the defects in Communism, which, in retrospect, were obvious.

Begin Tape 1, Side 2

DAVIDSON: When I came back to the University of Chicago, I started to do at the chemical level what I had been trying to do at the structural level at Oxford. And Schlesinger, by that time, had a research fellow, Herbert C. Brown, who had actually started two years behind me as an undergraduate but had rapidly gotten a PhD [1938] while I was more leisurely doing things at Chicago. So he ended up with his PhD and became Schlesinger's assistant. Any chemist will know who Herbert C. Brown is. He was a tremendously innovative and effective chemist, who deservedly was awarded the Nobel Prize [1979]. He's at Purdue now. I don't know if he's a year older or a year younger than I am. [Brown, born in 1912, was four years older than Davidson—ed.] He also annoyed quite a few people, including me at that time. He used to annoy Jack [John D.] Roberts [Institute Professor of Chemistry, emeritus] and his friend the famous chemist Saul Winstein at UCLA. Brown was a thorn in their side, because they were trying to recognize things as complicated and sophisticated and Herb always saw it simplistically. He would come up to me and say, "Dimethylaluminum chloride is a dimer because of a chlorine bridge. Why isn't trimethyl a dimer because of a methyl bridge?" I would say, "But Herb, methyl doesn't have extra electron pairs, and chloride does." That didn't affect him, and his argument didn't affect me. Eventually he was right. So the guy just has magic instincts.

ASPATURIAN: Was he frequently right when he thought of simpler explanations?

DAVIDSON: There are three or four cases in this. In this particular case, you know the theory's right or wrong. It's a structural question, and eventually you get a definitive answer. In mechanistic theory, it's all interpretive and you can never prove you're right or wrong. Unfortunately, Saul Winstein came to an untimely, early end [1969]—a heart attack or something. But even if he had died with time to think about it, he would have said, "Brown was wrong." But Brown did some beautiful things. He did some highly creative things, which are objective, in terms of designing new synthetic reactions, and designing new methodologies, which are not directly relevant to the work I did but which followed from the line of research that he and I were doing.

Anyway, my idea didn't work, but Herb said, "Why don't you do these other things that are more straightforward?" I did do them, got a reasonable thesis from them, by measuring the polymerization of compounds like dimethylaluminum chloride and dimethylaluminum mercaptan.[1]

That was, I guess, '41. We did have some kind of a war effort. I had a commitment for a regular academic postdoc at Ann Arbor with a guy named Lawrence Brockway. He was a famous structural person at the University of Michigan. But there was a six-month period in between, and I was asked out to the University of Southern California by Professor Anton Burg, who's probably emeritus now. I think he's still alive and out there still. Burg had been at the University of Chicago as Schlesinger's assistant and eventually left because of his intense dislike of being dominated by Schlesinger. I guess everybody who's a boss eventually becomes a pain in the ass. That's the way Burg felt about Schlesinger at the end, and he wanted to be on his own. He came to USC [1939], where he was quite a good professor in the field but not a leading scientist, I think, in the judgment of the modern world.

I went out to USC with him, and he had a poison-gas project. I made the compound S_2F_{10}, which the Chemical Warfare Service wanted to test as a poison gas. It was sort of a fun project. It turned out, thank goodness, not to be that bad and was never used. But that was a chance to be around this area, to meet people from Caltech, to make some friends who were probably instrumental in my later getting a position at Caltech.

ASPATURIAN: Whom did you meet out here from Caltech at that time?

DAVIDSON: There was a graduate student named John Wiggins in physics. We were both sort of lonesome, so we did a lot together. He was very interesting as somebody to do things with. He wasn't a very good scientist, but he appreciated good scientists. The active scientific crowd — the young people around Linus Pauling I remember most—were Verner Schomaker, who stayed

[1] Norman Davidson, "The polymerization of organo-aluminum compounds," PhD dissertation, University of Chicago (1941).

here for many years, and Eugene Eyster, who went to Shell Development as part of the structure crew and was a lively guy. There were trips to Mexico, and drinking and singing and all that kind of stuff. Also there was Norton Wilson, who was a very analytical and intellectual person who was more quantitative than anybody else, and who eventually went to Shell Development. They were all protégés of Linus Pauling in one way or another.

ASPATURIAN: Did you meet Pauling at that time?

DAVIDSON: Yes. In fact, I think they invited me to give a seminar about my thesis, and he came.

After my six months in California, I went to Ann Arbor and was doing work there that I thought was ridiculous. Brockway wanted to do certain exchange reactions using a mass spectrograph, so he wanted me to build a mass spectrograph for chlorine isotopes. And I said, "Look, there's good radioactivity methods with chlorine. We ought to use that instead." But he was really more interested in the mass spectrograph than in doing good mechanistic studies. He wouldn't let me change.

ASPATURIAN: You weren't doing what you wanted to do?

DAVIDSON: Yes. And I wasn't too happy. This may all seem a little inconsistent, since I was also very concerned about the draft. But all of a sudden, there's a telephone message: "This is Harold Urey. I need you for a very important war project." And Urey is running a part of the Manhattan Project at Columbia University which is devoted to separating uranium-235 from uranium-239. Specifically, they were devising the gaseous-diffusion method based on uranium hexafluoride, which is a highly corrosive but in other respects reasonably usable gas for the isotope separation.

ASPATURIAN: How did Urey get hold of your name?

DAVIDSON: I don't know. Good ear to the ground: Who are the bright young kids around?

ASPATURIAN: Had you known anything about what was happening with the Manhattan Project?

DAVIDSON: Nothing whatsoever. I was very naïve about nuclear physics. I went to Columbia. I guess between the draft and being disillusioned about my postdoc, this was a marvelous excuse. The idea was to find out how stable is UF_6. The separation process through porous metal foils was dependent on whether the uranium hexafluoride could be relied on to not totally corrode these porous metal membranes.

So Tony [Anthony L.] Turkevich, a radiochemist I'd known at the University of Chicago—he's probably now retired—and Nick [Nicholas C.] Metropolis, a theorist at Chicago, and I set up a system for making measurements. Earl Long, a thermodynamics guy, was in charge of our measurements. The head of this whole group was Bill—Willard—Libby, quite a famous physicist, who was involved in both the plutonium project and lots of radioactivity stuff. We learned how to handle UF_6, and we made appropriate thermodynamic measurements. We showed that in theory it should be reasonably stable, and we showed that in practice, if you use dry metal apparatus, it was in fact stable. So that part of the project was, in its own small way, successful. But it was intellectually a somewhat disappointing experience, because the whole thing was run in proper army form, with a lot of compartmentalization and very little sharing of information.

ASPATURIAN: How much did you know about the rest of the project?

DAVIDSON: Very, very little.

ASPATURIAN: Did you know that they were building a bomb?

DAVIDSON: Yes, I knew it had something to do with how to make a bomb, but nobody explained the physics to me. I knew that our process would be used for enriching uranium-235 from uranium-239; it was fissionable and it would somehow make a bomb. But this is in marked contrast to what I'll say in a little while about my experience at the Metallurgical Laboratory project, the plutonium project at the University of Chicago.

Urey is the world's greatest person on diffusion theory, and he never even tried to educate us about that, which would have been interesting. So it was just a job. In fact, one of the greatest accomplishments of that stage of my career was that as a lonesome bachelor, I wandered down to the coffee shop in International House one evening. There were two of my friends on a date with two nice young ladies, one of whom ended up as Annemarie Davidson. I invited myself to sit with them. She was a graduate student in economics at the time at Columbia. Her family is from Berlin; they got out in time. They lived in Long Island. Then, around June of '42, Urey said they didn't need me anymore, so I got a teaching job at a joint institution called Illinois Institute of Technology and Lewis Institute, on the far west side of Chicago.

That was a pretty miserable experience. My teaching labs were at the Lewis Institute, on the medium West Side. My wife and I got married shortly after I moved back to Chicago, and we lived on the South Side, in the Hyde Park neighborhood. So it was a long public-transportation ride. As part of my teaching, I tried to set up a lab to do some research I was interested in. But I'd come back two days later and everything would be covered with dust, because of the way things are in Chicago. Generally speaking, I wasn't very happy, and the draft situation was getting more serious. And then, here were all my friends from graduate school telling me they were working on the thing called the Metallurgical Project. All they said was, "If we're successful, it'll end the war." That's all I knew; really, the security was excellent. So eventually Annemarie got worried, and I got worried, and I went and asked for a job there. I had recommendations from my friends. So I walked in, and I'm told that I'm going to be in the Glenn T. Seaborg group.

The first day, Glenn Seaborg and some of his lieutenants explained the whole project. The chain-reacting pile made uranium into plutonium. Plutonium would be purified. It was a fissionable material. Fissionable material, if you slammed it together from a subcritical mass to a critical mass, would become a bomb. And it was extremely exciting. We could go to meetings where [Enrico] Fermi talked about the physics of the pile. We had access to all the reports. It was just a totally different environment from Columbia, and morale was very, very high.

The history of our particular project has been written about so often by so many people that I want to be pretty brief and just mention a few personal items. The Seaborg group was responsible for developing procedures for extracting plutonium from the spent uranium out of the pile, which would be loaded with fission products and all sorts of radioactivity. It had to become manageable from the point of view of radiation damage. It had to become ultra-pure in order to work as a bomb material. In fact, nobody realized then just how pure it had to be. We had the whole responsibility for devising procedures, which would be scaled up by other people.

You might say I became very impressed with the quality of the DuPont engineers who had the responsibility of scaling up things like this, doing control procedures which they'd never had to do before. And I may say, parenthetically, that based on that, I thought the Russians would never be able to do it, because it seemed to me that just the engineering, even if you stole the scientific secrets, was so sophisticated. I still don't quite understand how they did it so well. But apparently, when they really care, they can do high-tech stuff OK.

I remember this period as a time of tremendous intellectual excitement for me. I first had to do some wet chemistry—that's solution chemistry involved in process development. That turned out, and I worked under the guidance of a guy named Stan [Stanley G.] Thompson, who invented what was called the bismuth-phosphate procedure, which eventually became, at least for a while, the procedure used for production. When that was done, we started working on the chemistry of solid-phase plutonium compounds. Some people were learning how to make the metal, which was what was needed for the bomb, and the rest of us were just learning as much as we could about the chemistry of plutonium and the chemistry of element 93, neptunium—uranium is 92, neptunium is 93, plutonium is 94. The later elements came along basically after my time. But I teamed up with a guy named Sherman Fried, and we were making compounds of neptunium and plutonium—especially neptunium, when that became available in more than radioactive quantities—at the rate of one a week. There was a marvelous crystal-structure-determining guy named W. H. Zachariasen there, and for these simple compounds he could determine structures as fast as we could make compounds. We were discovering new things every day.

ASPATURIAN: When you say "determine structures," could you elaborate on that a little?

DAVIDSON: We'd make something, and we didn't know if it was neptunium with three fluorines or neptunium with four fluorines, or neptunium with an oxygen and a few fluorines. It would have been very, very difficult to find out chemically, but Zachariasen found out. We would do something, for example, that we thought would make either neptunium with three fluorines, or with four, and he would tell us which it would be. We did the same with more complicated compounds. It was just a blast, and I learned a lot of interesting chemistry at the time.

There are a couple of other points that are somewhat more sensitive. Seaborg was an aggressive manager who was concerned with power and with receiving credit. There was a certain amount of infighting, especially between his group and a group led by Charles Coryell, which had a different responsibility. Charles was a much more naïve, simplistic guy, so he got outmaneuvered by Seaborg. Glenn Seaborg was one of the co-discoverers of plutonium and neptunium, along with a guy named Edwin McMillan, who stayed at Berkeley.

One of my closest friends at the time was a guy named Joe [Joseph J.] Katz, who stayed at the Argonne [Argonne National Laboratory] as a senior scientist and became quite a distinguished guy in photosynthesis but also was a great writer. So Glenn Seaborg used to ask him to be his coauthor on innumerable books, including histories of the plutonium project and the Manhattan Project.

Toward the end, when they began to have milligram quantities of plutonium, somebody finally realized that it was like radium, potentially extremely dangerous, because it was an alpha emitter, and the chemistry was such that it would concentrate in the bones just like radium.

ASPATURIAN: They did not realize until then that the radioactivity posed a danger?

DAVIDSON: Nobody really thought about it, because they thought only of radiation and not about the fact that if an alpha-particle emitter gets in your bone marrow—and this stuff had the chemistry such that it would get there—it would be real, real bad. And alpha emitters are much more difficult to detect than things like phosphorus or iodine that we work with all the time now, where you can wave a counter six feet away and detect it. I just didn't like that atmosphere of working with something I really couldn't see and couldn't detect very well, and with people whose technique you didn't always respect working in the same lab.

So, toward the maturity of the Chicago project a lot of the other people went off to Los Alamos or Oak Ridge to continue their work. As you know, the most exciting, terminal stages of everything were at Los Alamos. But I wasn't needed anymore, and a guy named John Turkevich, brother of Tony, talked me into taking a job as an electron microscopist at RCA Labs. I don't know why, but I decided it would be interesting. He was a very articulate salesman. People who know him will appreciate that as an understatement. So we went to Princeton for a year. Actually, I left the day that our first child, Terry Davidson, was born. I saw him born, and then I got on a train. Annemarie came with Terry a month or so later. I worked with a guy named Jim Hillier, who basically was the first guy on the American continent to make an electron microscope. And it was semi-interesting but not real interesting.

ASPATURIAN: What were you doing there exactly?

DAVIDSON: Just developing applications of electron microscopy to structure. Specifically, I was trying to use it for doing diffraction work, which is a kind of structural work for learning. It took many, many more years before that technique got anywhere, and I realized it wasn't highly satisfactory. And I kept having ideas, based on my experience in inorganic chemistry, of projects I thought were interesting. So I wanted to get back to doing my own research.

After a year, we looked for a job. Actually, I wrote my friends here, and it was very, very funny. I had an offer from some more senior guy, George Watt, who was at the University of Texas, who offered me a job as an assistant professor. But there's in the Archives this original letter from Pauling, written by hand with his excellent writing, from a hotel in St. Louis, where he was on a trip, offering me a job as an instructor at $3,600 a year.

ASPATURIAN: Was that a generous offer in those days?

DAVIDSON: It wasn't ungenerous; it was the going rate for instructors. And I had this offer to be an assistant professor in Texas. I knew I could make it in Texas, but I didn't know if I could make it here. But Annemarie said she wanted to live in the sun. I think I sensed that I wanted to be in a place that was intellectually exciting, even though I tend to be a cautious person. So I made the very, very wise decision to come here.

We came across the country in a house trailer, pulled by a Jeep, which was the only car you could buy, with our son and a long-haired dachshund. We moved into a trailer park on Rosemead, just where the DMV [Department of Motor Vehicles] is now. We lived in it for three days. It was very close contact with a lot of neighbors, and it's life in the raw. And Annemarie said, "I can't stand this, all these people yelling at each other all the time." Fortunately, we had a little money, because her real father—there was also a stepfather—had died and we had enough money for a down payment on a house. So we bought a small house in Sierra Madre.

Now, Ray Owen [professor of biology, emeritus], who's a much more cautious person than we are in terms of managing his affairs, also came with June Owen around that time. They lived in a trailer for a year, then they bought a house. Well, housing prices actually went down, because when we came, it was just at the height of the postwar shortage. But we sold our trailer at a better price than the Owens did, so it was a wash.

NORMAN R. DAVIDSON

SESSION 2

August 19, 1987

Begin Tape 2, Side 1

DAVIDSON: I wanted to open this session with a reminiscence of Linus Pauling that I actually told at the birthday party that the chemistry division had for him in February 1986. I have this very clear remembrance of walking into Caltech for the first time in 1946 and going into the library in Gates [Laboratory of Chemistry]. That library has very high book stacks, and there were mobile ladders hooked to the stacks at various places so you could climb up and get various books. There was Linus Pauling, at the very top of a ladder at the east end of this little library, reading the *Journal of Physical Chemistry*. He was perched on this ladder ten feet from the ground, and I thought, “Oh my god, there’s the world’s greatest brain on some very fragile legs.”

I came to Caltech in 1946 with the assignment of assisting Linus Pauling in teaching freshman chemistry. This involved organizing the laboratory course, giving lectures, attending lectures, making up problems, supervising the teaching assistants, making up homework assignments, et cetera, and occasionally giving lectures when Linus was out of town.

ASPATURIAN: What were your impressions of Pauling?

DAVIDSON: He was very supportive and helpful to any young person, including being worried about whether we had enough money and things like that. Jack [John H.] Richards [professor of organic chemistry and biochemistry] has told me that when he first came to Caltech, Pauling asked him what his money situation was and put him on the payroll three months before his appointment was supposed to start and raised his pay early. My specifics are somewhat different, but Pauling was really concerned about this, as well as with the intellectual welfare of the young people. He was, of course, a marvelous lecturer. I usually managed to see him a day before his class and remind him what he was supposed to lecture about. I would tell him some points that I felt had to be emphasized on the basis of our experience in the discussion sections and try to keep him on track, because he loved to talk about what he felt was interesting. There are all of these anecdotes about how he would walk in and ask somebody, “What am I supposed to lecture about today?" and then give an absolutely marvelous lecture. He's one of these people who can think very, very effectively on his feet. I don't know what the intellectual faculty is; I think there are people who have high intellects who can't do it. I don't know if I have a high intellect, but I know I can't do it. But Pauling had everything organized; he could see everything in his mind in a logical order, and it was presented beautifully as well as being beautifully stated. Let's continue with Pauling later. I'm sure many other things will occur to me. If not, I'll make some notes and be a little more systematic about it next time. [See Appendix]

I gave a moderate number of lectures in Pauling's place when he was on trips, and I always looked forward to it, because I thought I could explain things as interestingly as he did, from a somewhat different point of view. Finally, when there was some kind of teaching evaluation, it turned out that the students thought I was terrible. The fact of the matter is that I was a very slow, hesitant speaker. I still am, except when I get psyched up and prepare myself—then I can give a reasonably fluent lecture. And at the time, I didn't realize it; this was sort of a real shock to me. Linus was very, very helpful about that. The situation came up a few years later, at the time of my reappointment, when there was a distinct possibility that I wouldn't be continued on to a tenured appointment at Caltech. Linus directed me to a speech teacher, Lester McCray, who was on the faculty of USC's speech department. I took private lessons from this guy, and it turned out to be marvelous for me, because he was somebody who wasn't interested in the kinds of things you think of speech teachers working on, like proper elocution and proper presentation. He was interested in effective communication, which meant making contact with your audience. It meant trying to come through in an interpersonal or human relation with the people you were talking to, and trying to relate to them as you talk and to sense their feelings. He engaged in a certain amount of amateur psychoanalysis about me and decided to try to convince me that I was afraid of people, or inhibited, or something, part of which is at least partly true. The net result of it was that I did become a much better and a much more informal lecturer. And of course Linus just did all that stuff naturally. In that connection, I can remember giving to all the faculty the lecture that was the important one either for the continuation of my appointment or for my final tenure appointment review. It was a major, one-hour lecture, and I remember I was quite fluent. So the view that I should not continue at Caltech, which some of the faculty held for reasonable reasons—they were convinced otherwise, and here I am. I think it was good for Caltech as well as myself, I may say.

I would like to say a little bit about my own research work. One of the reasons I wanted to leave RCA was because I was getting ideas about making a certain class of organometallic compounds: actually, a trifluoromethyl group instead of a plain ordinary methyl group as the entity joined to the metal atom—for example, zinc bis-trifluoromethyl. I just thought those were revolutionary ideas and that these compounds might have interesting properties. When I came to Caltech, I set up a vacuum line to do this kind of chemistry in 101 Gates, which was my laboratory at the time. I tried to do that work, but between various committee and teaching responsibilities—the freshman laboratory was right next to me, and I had to go in and see that it worked—and various other responsibilities, I could only work on my project two hours here and two hours there. I eventually had to give it up, because it just wasn't working. Graduate students came to talk about research projects, and there were basically three things I was interested in for research. I was interested in gas-phase reaction mechanisms. I forget where that interest arose in terms of my own education, but there it was. There were some students who thought that was interesting and chose to work with me on that. I had a less intense interest in the formation of complex ions in solution, but I liked to carry on a minor program in the formation of copper complexes and iron complexes, and the formation of mercury complexes especially. In fact, I got a rather bright idea from some work that Pauling's people had done and that Linus was interested in. This was about the color that developed when one has inorganic ions of two different oxidation states—like plus-2 and plus-3 iron, or like plus-1 and plus-2 copper—in the same solution. When they were in the same solution with appropriate anions—ions or ligands that could form a bridge between them—one frequently then saw a strong color, due to the electron transfer, in a transient molecule, between the two oxidation states.

There were students who were interested in the gas-phase reaction kinetics and mechanism work and students who were interested in this solution work, but there were no students interested in what I thought were my very exciting and revolutionary ideas about trifluoromethyl. It later was done by other people, and I don't think it ever became a major thing in the really revolutionary expansion of organometallic chemistry, which is still going on. But it was a significant minor chapter.

The solution-chemistry part is noteworthy. It was very good for undergraduate research, because the students worked on problems they had learned about from their undergraduate studies. You could give them a small undergraduate research problem. It was fun for them and fun for me. It's noteworthy otherwise only because Harden McConnell, who's now a very famous Stanford professor—he may get the Nobel Prize sometime, or may not, but is in that class—came to Caltech [1947] as a very young, very bright, ambitious, starry-eyed young man who wanted to do theoretical work. He walked in to see Jack [John G.] Kirkwood, who was a new member of our faculty then. Jack said, "Go away. You're too young, too inexperienced. Come back after a couple of years." So McConnell looked around and talked to various people, and I talked to him about this project, which had really interesting electronic consequences. That's what McConnell chose to do his PhD thesis on.[2] It resulted in a couple of nice papers—not earthshaking ones, but good ones. Because one didn't know the precise structure of the colored molecules, and because molecularly they were too complex for real theoretical analysis, it turned out to be a subject that never became an important part of the field of what you might call semiconductors. It's really closely related to electron-transfer reactions and semiconductor phenomena. It's sort of an amusing appendix to the major subject. McConnell then went on to do other things as a postdoc and do marvelous things in his independent work, in which he used a beautiful combination of theory with experiment. He has always said—and I believe it—that from me he learned how to get a very good feeling of the close connection between theory and experiment, and of the meaning of experiments. And I'm proud of that. If you look at most of Jack Kirkwood's students who have continued in the field of statistical-mechanics theory, I think most people would agree that their work has been relatively sterile since that time, up through now—not totally, but relatively. I think it may have been very good for McConnell to have worked with me rather than with Jack Kirkwood.

The other important thing in terms of my intellectual history of this inorganic solution chemistry theme was that I did always have an interest in mercury complexes. When we did shift directions and start working on DNA, one of the first and obvious things was for somebody who understood the inorganic chemistry of mercury to apply that to the study of mercury complexes of DNA. That, in fact, turned out to be a significant—not a major—contribution to the development of DNA methodology for five or six years. Like many methods, it was used for

[2] McConnell, Harden M. (1951) *I. Investigation of possible interactions between thallium (I) and thallium (III)in solution and in the crystalline thallium sesqui-halides. II. Spectrophotometric investigations of the copper (II) chloro-complexes in aqueous solutions of unit ionic strength. III. Optical interaction between the chloro-complexes of copper (I) and copper (II) in solutions of unit ionic strength. Interpretations of the spectral absorption of a copper (I) - copper (II) dichloro-complex. IV. Spectrophotometric investigation of the interaction between iron (II) and iron (III) in hydrochloric acid solutions.* Dissertation (Ph.D.), California Institute of Technology.

a while and then supplanted by other methods. But the things we introduced were both interesting and useful to the community for a while. But that came later.

I think my main contribution in physical chemistry is in the field of fast reactions. The way that happened was that I was just generally interested in free radicals—which are unstable intermediates in chemical reactions—and was always thinking about ways in which you could isolate and study them rather than inferring their properties indirectly from microscopic observations of the phenomenological observations of the rate of the reaction. At the time, I had a young graduate student, John [H.] Sullivan, who did a very nice phenomenological study of the reaction of chloroform with bromine, from which we both learned a lot about free-radical reactions.

As a result of this, I was thinking more and more about how to create conditions under which one could directly have a sufficiently high concentration of atoms or free radicals—which are normally present at very low concentrations in a so-called steady state as a chemical reaction is taking place—so that one could directly observe them.

At this time, let's have a major diversion and talk about the environment at Caltech. Caltech was a particularly good place for getting ideas, inputs, and knowledge of methodologies from your colleagues in other fields, so that if you were imaginative and adventuresome, you could apply their methods to the problems of interest to you. I remember that back then I used to go to the Athenaeum for lunch almost every day—which I don't do now. The faculty tables were in about the same places—the one in the center, east side, was exactly the same as the one commonly used now for the same purpose. There were very lively, interdisciplinary conversations at these tables, and you met a lot of people. The Caltech faculty then was so small that I think I knew a very large percentage of the faculty, partly by virtue of people I met at the Ath, partly by being interested in other people's work and going and asking them about it. That is not the case now. The only way young faculty meet faculty in other divisions now—and no doubt in a large division like engineering, faculty in other departments of the same division—is through service on faculty committees. Since young people now are very wrapped up in their research, and have to be, the amount of service on faculty committees that they engage in is a lot less than we used to. There's a lot less tendency today to meet and be interested in your colleagues and in other disciplines. But at the time, it was very exciting.

One of the topics you jotted down on this list to talk about is "relations between chemistry and other divisions, especially biology." My interest in biology at the time was purely intellectual, not active. Linus Pauling himself was the one who created ties with the biology division. Max Delbrück [professor of biology, d. 1981] was a person with whom it was very easy to relate intellectually. So was Norman Horowitz [professor of biology, d. 2005]. You could just go and talk to anybody about what they were doing then, and I remember it was very exciting and very nice.

When I didn't have time to go to the Athenaeum, I would go over to what we then called the Greasy Spoon [Chandler Dining Hall]—people still do, to some extent—which was then this little wood-frame building, a temporary World War II building, I believe. Frequently, there was Robert A. Millikan [Caltech head 1921-1945], sitting by himself, having lunch. This was after [Lee A.] DuBridge came [Caltech president 1946-1969]. Millikan was retired as president [Millikan's title was not president but chairman of the Executive Council.—ed.] but he still had an office. Sometimes he was at the Athenaeum for lunch. Every time I sat down with him—he was eighty plus or minus a few years at the time—he was very cordial. If I saw him once every week, he would ask my name again; he didn't remember a thing about me. But I'd ask him what he thought about this or that scientific issue, and he'd ask me what I thought about various things. He was just really a nice, interesting person. This great institution was made by three people, and in my picture of things, Millikan was really the driving force of the three. Of course I never met [Arthur Amos] Noyes [professor of chemistry 1919-1936]. I know what people like Ernest Swift [professor of analytical chemistry, 1928-67] said about his major intellectual impact on the directions the institute took, but I had no personal appreciation of that. Millikan's impact, though, I could hear about in anecdotes and by talking to him. It was very exciting, as an instructor, to meet a man who had really made history.

I remember some other things. Before I knew it, I think the first year I came here I was put on the Freshman Admissions Committee. Nobody asked me if I wanted to be a member—or if they did, I didn't say no. I don't think I could have.

ASPATURIAN: Did you enjoy that?

DAVIDSON: Yes. Of course, it's demanding. Fortunately, I had the Glendale-Burbank area, maybe going out to San Fernando. I spent maybe a week or two driving to the various high schools and having interviews and then a couple of weeks at the committee meetings, where we made our recommendations. It was rewarding. There'll be a lot of other people who have had experience on the admissions committee who can comment more about the trade-off between the value of our admissions procedure with faculty interviews versus the drain on faculty time and the distraction from other possible contributions, either scientific or to the institute. But it clearly was a very good and unique feature of the way Caltech has done things. Winch [Louis Winchester] Jones was the head of the Admissions Office [1937-1968]. I guess he was also still a professor of English [associate professor of English, 1943-1960—ed.]. He was in the old tradition of the humanities division, where the humanities faculty were more interested in student development than in their own intellectual contributions. He was a literate person who could write well and talk well, and I'm sure he could teach English well to Caltech undergraduates. But he was now full-time with the Admissions Office and maybe taught one course. He was one of the lively members of the faculty tables at lunch.

Overall, the admissions process was a good one, except for one point that I would like to put into the record, because when I tell young people about it, they all raise their eyebrows. The first few years I was there, we'd be talking about various people, and then one of the members of the committee would quietly say, "Well, I've got this guy; he's pretty good. But we've filled our quota for Jews for this year." And these were very nice people. One of them was Fred [Frederick C.] Lindvall, who was chairman of the engineering division [1945-1969] and was overall a marvelous, fair-minded person whom I often went to for help or advice about various problems. But I somehow have this picture of him being both a member of the committee and making that remark, as though it was just understood. They didn't know I was Jewish, and I felt it was better not to say anything. The thing just died a natural death in about two or three years.

ASPATURIAN: Different committee members?

DAVIDSON: Somebody just said, "Look, this is old hat. This was supposed to have gone out in 1942, and here it is in 1948. What are we doing?" Everybody said yes. And that was that. If you look at the Caltech faculty prior to the great expansion and intense competition for outstanding people, starting, let's say, in 1950, and compare that to the faculty at other leading institutions, there were many, many fewer Jews here. That's partly cultural; this went on in Southern California. I have no idea whether it was any more than that, but it is a fact.

ASPATURIAN: There are old stories to the effect that Millikan and [Thomas Hunt] Morgan had doubts about admitting too many Jews, or that Millikan was somewhat anti-Semitic. Did you ever run into any of that while you were here?

DAVIDSON: I've never heard that about Millikan. As for T. H. Morgan, Norman Horowitz was a protégé of Morgan—he was more a protégé of [George W.] Beadle, but also to some extent of Morgan. Norman Horowitz is a great admirer of his. If he had ever heard anything like that, I've never heard him say it, and I doubt it. It must have been very restrained in Morgan's case, or just not true. I do recall hearing that some history-of-science student at Johns Hopkins did a PhD thesis on the history of biology at Caltech.[3] I am told that she had some stories about a tendency of T. H. Morgan and the rest of the early—well before George Beadle's chairmanship [of the Division of Biology, 1946-1960]—biology leadership to prefer non-Jews, but I myself never heard anyone at Caltech talk about this. Parenthetically, I'd like to say that the two people I think of who would never have condoned any kind of discrimination are Linus Pauling and Max Delbrück.

Stories I have heard about Millikan are that he ran the institute with his limited budget out of his own pocket, paying people only what he knew was necessary to keep them and not trying to reward equal performance with equal pay and highly meritorious performance with especially high pay, if he didn't have to. I particularly remember Jesse DuMond, who I knew well—a great X-ray physicist and a very nice person to discuss topics in physics with, and who apparently was a person of independent means—saying that Millikan knew he loved Caltech. Whenever Jesse would come around with an offer in his pocket, Millikan knew that he wasn't about to leave Caltech and that he didn't need much money. So Millikan didn't pay him much money. And, of course, that hurts. But DuMond loved Caltech; he loved the whole spirit of

[3] Lily E. Kay (1987), "Cooperative Individualism and the Growth of Molecular Biology at the Californ ia Institute of Technology, 1928-1953," Ph.D. dissertation, The Johns Hopkins University.

Caltech. And he was a great scientist. After DuBridge came, our salaries went up quite a bit, because he realized that we were, by competitive standards, woefully underpaid. And that helped.

Pursuing the Millikan business a little more, I remember he came regularly to physics seminars. I also used to go then. Millikan would ask questions which clearly indicated that he didn't know what was going on anymore in terms of modern physics. In a sense it's sad, but I don't think it's unexpected or reflects in any way on his intellectual contributions. Somewhat later, when he was really getting old, I can remember that he would not only be sleeping in seminars—that's easy to do; I could do that at age thirty, not to mention twenty—but even during lunch at the Ath. I have this picture of him every so often, sort of closing his eyes in the middle of what I thought was a vivacious conversation with everybody else at the table. But my main picture of Millikan is of this great man who had played either *a* or *the* major role in making this great institution, and that he was easy to approach, easy to talk to, and was a man of high principle.

To return to my strong desire to be able to directly study, rather than inferentially study, unstable intermediates like free radicals in chemical reactions, I was probing for methods, which intrinsically had to be pulse methods, for creating high concentrations of unstable intermediates—radicals or atoms.

I also had good relations with other divisions, especially physics. I of course, had learned a certain amount of nuclear physics during the bomb project. I remember especially the senior Lauritsen, Charles Lauritsen. He was wise, temperate, supportive, and smart. Charles was the head of the low-energy physics in Kellogg [Radiation Laboratory] at the time, and there was Willy [William A.] Fowler, Tom Lauritsen, and Charlie [Charles A.] Barnes. In connection with my ideas about new instrumentation, they were very, very helpful and supportive, and very interesting. So with help and advice from the physics group in Kellogg, an undergraduate, Bud Larsh [class of '50], and I did a small but successful and interesting project on electronic motion in liquid argon, using electrons liberated by alpha particles and pulse-detection methods. In all my thinking in this field, the Kellogg people were generous with time and advice.

Then I heard about this group in hydrodynamics that was using flash lamps to take high-speed photography of fluid flows. I realized that by using the flash lamp to photodissociate a molecule that absorbed light but was itself stable, one could create an instantaneous high concentration of the radicals. Then, if you added an appropriate spectroscopic method of detecting them—basically high-speed absorption spectroscopy with photomultipliers—you could learn something about the properties of the radicals. So we started doing this. The active young faculty member—I don't think he was on the tenure track—was Haskell Shapiro. [Haskell Shapiro was not a member of the faculty; 1941-1957, he was section chief in Caltech's Hydrodynamics Laboratory—ed.] And with his help I designed an apparatus. Haskell was very generous with advice and with lending the equipment, and then I raised enough money from the ONR [Office of Naval Research] to do stuff myself. At that time, the ONR was the principal federal institution funding basic research in physics and chemistry. It was very good for me, and it was very good for everybody in physics and at least in physical chemistry.

ASPATURIAN: Was their involvement in that basically an outgrowth of the Second World War?

DAVIDSON: Yes. This was as good as working for the NSF [National Science Foundation] now. There was no security, no mission orientation in the specifics of your work. The ONR realized that basic research in very, very general areas of interest was bound to be valuable for their more practical missions.

Between the ONR and Haskell Shapiro, I put together an apparatus. We made some measurements on it. Actually, the first thing we did was measure rates of photodissociation of iodine molecules into atoms and then measure the rates at which the iodine atoms came together. Interestingly enough, two other groups, one with George Porter and his professor at Cambridge [Ronald G. W. Norrish], did the same thing about the same time, completely independently. They did more, and they did better than we did. I'm sorry that we didn't do it better. But I'm glad, of course, that George did it well.

Simultaneously, [German biophysical chemist] Manfred Eigen was developing the temperature-jump methods for measuring fast reactions in solution. It's interesting that I had the same idea—this T-jump idea—of using a condenser discharge to heat up a solution rapidly and measure chemical reactions that you could induce that way. But I had only one student, who was not competent in instrumentation, trying this, where Manfred had a skilled engineer, Leo De Maeyer, as his collaborator and just did it a lot better.

Well, both George Porter and Manfred Eigen got a Nobel Prize [1967] [shared with R. G. W. Norrish—ed.], and I didn't. I must have been moderately close. But we did some nice work. I had a graduate student working with this named Tucker Carrington, who had been imbued by me with this philosophy—that we want to take a stable molecule and apply a pulse of energy to specifically dissociate it. This would then produce unstable radicals or other intermediates, which would have been created in such a short time that there would be a short interval of time in which you could observe them before they disappeared.

Begin Tape 2, Side 2

DAVIDSON: While we were working on flash lamps, Carrington began saying to me, "There's also another way of doing this" —something about a shock wave. I'm very fuzzy about it, but I know he had the idea long before I did and finally convinced me that I really ought to understand it. So he and I together learned this new field of gas dynamics—new to us, that is—and what is a shock wave, and learned how to formulate it. I played a major role in this; it was by no means just Tucker Carrington working on it with me as a supporting faculty member. But he was important in initiating the idea and, of course, in executing it. We worked together marvelously, and we decided to make a shock tube. We planned and designed the shock tube, the electronics and the photo tubes and all that very, very carefully, so that it worked the first time we used it.

I remember one of my sources of advice about this was Jack Kirkwood, who'd done a lot of work on the propagation of shock waves through explosives during the war and understood the subject theoretically. I told him my ideas, and he said, "Yes, those are good ideas, Norman. Can I tell people about them?" I said, "Maybe you better not for a while." And then I remember telling him it worked, and he said, "Can I tell people about it?" and I said yes.

Tucker and I designed this apparatus and built the shock tube. We measured for the first time the rate of dissociation of N_2O_4 into NO_2 and got good numbers.[4] That really was a unique first. And as with everything, there was somebody else trying to do it; specifically, Don [Donald F.] Hornig [at Brown University] was trying to do it. He was behind us and really hadn't planned things as carefully as we did. That was the basis of the major part of my research for

[4] Tucker Carrington & Norman Davidson, "Shock Waves in Chemical Kinetics:The Rate of Dissociation of N_20_4," *J. Phys. Chem.* 57 (4): 418–27 (1953).

four to six years—applying this to various other reactions. We did good stuff. We never did get to the stage where we could look at what was supposed to be the plum in the field—namely, methyl radicals, whose spectroscopy is quite difficult.

ASPATURIAN: What was the advantage of this technique over the flash lamp?

DAVIDSON: You could study molecules that didn't absorb light but did decompose on heating. Modern lasers will do it better. In fact, it was a factor of 3, or something like that, faster than the flash lamp. But the main advantage was for molecules that you couldn't do anything very effectively with by photochemical dissociation. You could, in some cases, study them by thermal dissociation. That was, I think, our biggest and most interesting thing for quite some time.

After a period of five or six years, we had done all the things you could do with rapid thermal excitation by shock waves, followed by rapid photoelectrical observations by absorption spectroscopy. We were utilizing light in the visible and near-ultraviolet region, basically the quartz ultraviolet region. There were a number of additional, important problems in atmospheric chemistry involving oxygen molecules and other very simple molecules that could have been investigated but required building new shock tubes, new kinds of lamps, and a major commitment to new instrumentation.

At that time, the plum, the great mystery, that people really wanted to get physical studies about, was the methyl radical. It was the simplest prototype of a free radical, which had been studied a great deal in chemistry. It would have been semi-approachable theoretically if you could observe its spectroscopic properties. We didn't succeed in doing that. George Porter didn't succeed. But a very famous spectroscopist in Canada, [Gerhard] Herzberg, did. He had a young collaborator, [Donald Allan] Ramsay, whom I remember because he and I were both interested in flash lamps. We really felt very supportive of each other and not competitive but helped each other as much as possible. Ramsay and Herzberg were the people who managed to do everything right, if it involved high-powered spectroscopy, as well as high-powered flash technology. And they did it when we didn't.

To come back to shock tubes, which was our big thing, here I was at a stage where, if I was going to continue to do anything new, it meant a major commitment to building a new instrument that would work under conditions in the very far ultraviolet, where you could observe oxygen and nitrogen molecules and atoms, for example. I was thinking about doing that. But all this time, I was learning more and more about biology. At this point, I knew I had to make a decision. I was well established. I had tenure. In fact, a year or two later [1960] I got elected to the National Academy of Sciences—which I hadn't anticipated—because of this work, not because of anything about my biochemical work.

ASPATURIAN: You were elected at a relatively young age, weren't you?

DAVIDSON: Relative to some guys, yes; relative to others, no. [Richard P.] Feynman was younger. The other thing is that as far as I know, I was elected with no political work on my part.

ASPATURIAN: Is that rare?

DAVIDSON: I believe so, yes. I think Jack Roberts, who was also elected at a very young age and was already here, was the guy who managed all this for me. Linus Pauling wasn't really interested in chemical kinetics and chemical reaction mechanisms; he was interested in structure. He knew about my work, but he didn't really find it exciting. That was generally true of all the people in reaction kinetics here. If you could think about structures from your work, why, then Linus would like to think about it with you. But in terms of the methodology of doing this, which was itself a challenging intellectual problem, Linus wasn't terribly interested in your contributions in that direction. So I think it was Jack Roberts. All I remember is George Beadle saying, "Gee, I went to the academy meeting and I didn't even get to vote for you, because the mail vote was so decisive that the issue didn't come up."

ASPATURIAN: Had you realized that your name was under consideration?

DAVIDSON: No. All of a sudden my students put a sign up on the board, "Congratulations, Academician Davidson," or something like that. That was very nice. I wasn't very interested in it [my election] anyway. I didn't realize until a few years later that it had a tremendous advantage, both in terms of my salary and in terms of being asked to give seminars and invited to be a visiting professor, all sorts of junk like that. I still don't think it's a big deal, but it did happen.

But the point I want to make here is that my election, so far as I know, was for our work in fast reactions. But I kept in touch in a general way, with what was going on in molecular biology and in modern biology.

ASPATURIAN: Who was your main conduit?

DAVIDSON: Linus was one. Linus was *the* example of a person who had intellectual courage—you could even say the chutzpah—to think, "Well, if I know basic chemistry I can apply it to biology." Of course, since he was a genius, where some other people might not have done it so well, he did do it with extraordinary skill and made extraordinary contributions, as the record shows.

Delbrück was easy to talk to. Delbrück had been a physicist, and at this time was not interested in biochemistry—or in molecules. In fact, it's said that he vetoed the suggestion that Jon [S. Jonathan] Singer, then a senior research fellow here and a very good protein physical chemist, should be on our faculty, because he didn't think that the field had any future. He thought genetics and virus phenomenology was the way to go. Later, he realized he was wrong and he changed his mind. But Caltech was not strong in the biochemistry of DNA at the time. I think that all changed at the time of the Matt [Matthew S.] Meselson-Frank [Franklin] Stahl experiment [1958], which we'll talk about a little later.

To some extent, I knew that very exciting things were going on in biology, but at least right now I can't remember specifically what I knew in detail. The Watson-Crick structure had been discovered [1953], and it was realized that this was going to be central to the understanding of genetics and would found the subject of molecular genetics. But, to my recollection, there wasn't an awful lot of that nature going on at Caltech at that time. [Renato] Dulbecco was here, and he was interested in small animal viruses and knew that their genetic material was DNA. The group who were protégés of Delbrück—especially Bob [Robert S.] Edgar, and then through him, Bill [William B.] Wood—were working on the genetics of bacteriophage. But they were primarily geneticists at the time. Dulbecco was a cell biologist.

Then I remember that a guy named Frank [Francis Otto] Schmitt came to visit. Schmitt was a professor at MIT and a great organizer and promoter—in the good sense of the word—of what was then called biophysics. He was a crusader for converting physical scientists into biophysical scientists. He'd heard that I was interested in this, and I remember having lunch with him at the Athenaeum. He said, "We're going to have this big, four-week conference at Boulder, sponsored by the Biophysics Study Section of the NIH. The idea is to educate bright young physical scientists about what's going on in the new biology and what contributions they can make."

I went to the Boulder conference. It was the summer of 1958, I think. It was marvelous. It was a typical kind of a meeting of that type. In addition to the people who were supposed to be the educatees—the students—a tremendous number of leaders in the fields were there. Basically, they gave lectures, and then there were workshops in which they really talked to one another more than to us and we were supposed to try to find out what was going on. But I have this mental picture of Leo Szilard, who after World War II, with his student Aaron Novick, had gone into one area of biology from physics. He was kind of a senior statesman. I remember Meselson and a bunch of other young people used to sit around on the lawn with him every day at Boulder and talk about things. Max Delbrück wasn't there. I think Szilard knew that he had cancer, and he had no more than a few years to live—I forget just how many it actually turned out to be. [Szilard's cancer was diagnosed in 1960 but went into remission; he died of a heart attack in 1964.—ed.] He was a person with a lot of intellectual courage who didn't worry about conventions, and he knew he didn't have time to waste. At every lecture, Szilard would sit in the front row and listen to the first three or four minutes of the lecture. The titles all seemed fascinating, and I was sitting with anticipation in the back. Sometimes, though, the first three or four minutes were just dull, and I kept thinking, "Gee, this is supposed to be an exciting topic. When's it going to get exciting?" But after three or four minutes, if it wasn't exciting, Szilard would get up and walk out. He didn't leave like some people do—wait till the room is dark, then hunker down and sort of sneak out. He just stood up and slowly walked out. And by god, he was never wrong. Every time he stayed, the lecture was good; every time he left, the next fifty-seven minutes were as bad as the first three. And I never had the guts to walk out when he did.

I forget who all was there among the physical scientists. Who's the guy who invented the maser? Charlie [Charles H.] Townes. Townes was there. He toyed with the idea of becoming a serious biophysicist but never did make the switch. Bruno Zimm was there, along with a lot of other people who had basically made the conversion, although they weren't fully established yet. But the point I want to make is that, so far as I know, the only real hard-core physical scientist who became a hard-core exclusively biological scientist as a result of that conference was Norman Davidson. In a certain sense, they spent $500,000 or whatever to convert me.

I went to Boulder with an interest that had always fascinated me—whether electric fields could cause chemical reactions. Earlier, at Caltech, Alex [Alexander] Rich [research fellow, chemistry division], who was both an intellectual and personal friend of mine, a few other people, and I organized an evening study section on how nerves work. I'd always been vague about it before, but I came to understand the Hodgkin-Huxley discoveries about propagation of the action potential along an axon. I learned that here was the most marked example of where a medium-strong electric field caused a large protein, embedded in the membrane, to undergo a structural rearrangement so that a pore opened and became permeable to ions. I thought that it was fascinating and that I'd like to do something about that at the physical-chemical level.

At Boulder, there was a guy named Bernard Katz, a distinguished electrophysiologist from London, and I consulted him. He said, "Look, you can't do it. You can't do anything that we're not doing, which is stick electrodes in and measure potential changes." The concentration of these molecules on the surface of the available preparations, like the squid axon, is so low that there just aren't enough of them to make even microscopic measurements or any other measurement besides the electrical measurement. So I had to give that up. The thing that was clearly central, active, and amenable to approach by chemical and physical-chemical methods was how nucleic acids worked in general, but perhaps more so how DNA worked. This was four or five years after the Watson-Crick structure. So I realized that that was a good thing for me to do.

After this conference, I came back to Caltech determined not to build a new shock tube but to change fields. There are several ways you can make a major change, especially from physical science to biology. The most courageous way is the way Max Delbrück and Seymour Benzer did it, in which they said, "We are not going to use any of the specific techniques and approaches we have learned in our work as physicists" —Max in theoretical and nuclear physics, Seymour in semiconductor physics. "The only thing we're going to bring from physics to biology" —and this was Delbrück's *real* contribution— "is the habit of looking for systems where you can ask specific questions, preferably with quantitative evaluations of the answers." Clear-cut qualitative answers are really just as good, but Delbrück's major contribution by consensus agreement—this is not an original idea of mine—was to select bacteriophage for that purpose. Benzer picked a specific genetic locus in T4 bacteriophage and made major contributions to the nature of mutations, but he didn't use any solid-state physics.

I was smart enough to realize that for the major questions much of the fast-reaction technology and intellectual approach I had developed wasn't really very useful. You could do good experiments and publish papers, but they really weren't central. On the other hand, I did decide to continue to use physical chemistry and inorganic chemistry to try to study DNA. I realized early that there were several important questions you might be able to study. I learned that somebody had done some simple initial experiments on mercury and DNA. I knew enough about mercury ions and their complex chemistry to realize that these had the potential of being very clean complexes, which might be useful. I thought they might be useful for X-ray diffraction in order to do structural work using the principle of heavy-metal substitution. That turned out to be completely wrong, because the structures became completely disorganized on binding mercury, and it's never been used usefully for that. But it did turn out to be useful for other purposes, because of its unique and simple chemistry, and I recognized that.

I had at the time a very devoted, hardworking graduate student, a Brazilian-Japanese, Tetsuo Yamane [PhD 1960]. It was hard to get him to express opinions about many things at the time. He was working on a fairly straightforward, interesting problem on mercury complex ions in inorganic chemistry. I asked him, "Tetsuo, how would you like to see what mercury ions do with DNA?" He said, "You know, I've been wondering if we could do that. It's the most wonderful idea in the world." So it became real easy for me to get started. I did a certain amount of reading. The big problem then was, What's involved in going from duplex DNA to single strands?

ASPATURIAN: How does the helix unzip?

DAVIDSON: How does the helix unzip? There was a certain amount of work on the effect of acid on that, and I realized we could do still better work on that. I also had various other ideas—not earthshaking but good enough to get started. That year, when I decided to do this and had Tetsuo already working on mercury, four graduate students showed up who were enchanted with my ideas. They'd come to be chemists, but they knew that the real action was in DNA biochemistry. There was Danny [Daniel L.] Wulff; there was Bill [William F.] Dove; there was Bob [Robert F.] Stewart. All of a sudden, I had a DNA group. We were really working on various problems in the solution chemistry of the reaction of DNA with mercury ions—and a few years later, with silver ions, and actually with protons. I was still down in 101 Gates at the time, and I now had an office as well as labs in the basement.

That, incidentally, was the time that my physician fortunately saw an X-ray shadow in my lungs. I did have cancer, but it turned out not to be Hodgkin's disease, which would have been very serious, but a tumor of the thymus, a seminoma, which was very radio-sensitive. Linus Pauling was, incidentally, very supportive, very helpful, and sent me off to Chicago to his favorite oncologist, Charlie [Charles Brenton] Huggins, for a consult. You usually get a seminoma in the testicles, but seminomas of the thymus are not that unusual, and it's a very radio-sensitive malignancy, so it was fortunate. I had this period of six months when I was quite sick and went through several stages of radiation therapy. I remember every day, when I was back at the lab, taking a half-hour nap on a sleeping bag on the floor in my office and things like that. I remember walking around a lot and just looking at the beautiful sky and thinking how beautiful the world was—which, of course, you don't appreciate if you think you've got a long, long time to look at it. Of course, I was very worried for many, many years about a recurrence; but that gradually goes away, and there have been no recurrences.

Shortly thereafter, I moved to an office in 021 Church [Norman W. Church Laboratory of Chemical Biology], with my lab just across the hall. I moved there because Jerry [Jerome] Vinograd was the other biophysical chemist, and because he worked with ultracentrifuges he needed to be in the basement. There was also Matt Meselson. He started out as an undergraduate at Caltech, and I was his advisor when he was a freshman, but he left to go to the University of Chicago, which had broader intellectual interests. I think he also said, and I'm sure he meant it, that he was also leaving because Chicago was coeducational. He went to Chicago as an undergraduate and came back to Caltech—without a wife—as a graduate student. He wanted to apply chemistry to biology, and in that field Pauling was the shining light. He did his graduate work under Pauling. But Pauling's ideas on what students should do were not really good. As far as Linus was concerned, the secret of everything was structure, and that's solid, that's good.

Actually, it was before one could do protein structure, that's fair to be said. But instead of suggesting other kinds of more interesting experiments, Pauling had people determining the structures of dipeptides and tripeptides, and things like that. With the technology available at the time, Meselson spent at least three or four years as a graduate student determining the structure of one tripeptide. It wasn't intellectually exciting work. I think he would say now that he probably should have figured out something better to have done. But by the time he'd finished [PhD 1958], exciting things were happening. He teamed up with Jean Wiegle [research associate in biology, d.1968] and started to work on phage genetics. He and Frank Stahl got this idea of doing experiments to test the semiconservative replication model for DNA—that DNA is a duplex and then, during one cycle of bacterial replication, the DNA plus strand is copied to make a new minus strand, and the minus strand is copied to make a new plus strand. So that of the two cells inheriting the progeny DNA molecules, each has one parental strand and one newly synthesized strand. They realized that by a heavy-isotope labeling procedure they could distinguish between the newly replicated DNA strand and the old one. By this model, each progeny DNA molecule would have one newly replicated strand and one parental strand.

They went to Jerry Vinograd. The stories differ somewhat over here, and I'm not sure just what happened. My picture of the situation is that they had a vague idea about using the centrifuge for this. Vinograd, who understood the physical chemistry of the centrifuge very well, immediately recognized that it was a superb idea. Together they did what is called the Meselson-Stahl experiment. There were two papers, one by Meselson, Stahl, and Vinograd on the technology,[5] and one by just Meselson and Stahl in which they actually demonstrated semiconservative replication.[6] It was marvelous. There was, incidentally, a certain amount of hard feelings generated about recognition and credit. Matt and Frank felt that Jerry had to some extent hijacked their ideas. But in the real world, it really took all three to do it, and Jerry continued to use the centrifuge as a major technique for the study of what happens to DNA in biological systems. He made a large number of highly original contributions and was recognized after as *the* scholar in the field of circular DNA. He was elected to the National Academy [1968]; he was pretty close to a Nobel Prize. It was all totally deserved. Matt and Frank have

[5] Matthew Meselson, Franklin W. Stahl, & Jerome Vinograd, "Equilibrium Sedimentation of Macromolecules in Density Gradients," *Proc. Nat. Acad. Sci.* 43: 581-88 (1957).
[6] Matthew Meselson & Franklin W. Stahl, "The replication of DNA in *Escherichia coli*," *Proc. Nat. Acad. Sci.* 44: 671-82 (1958).

had their own independent, excellent careers on more biological approaches to fundamental problems.

I'd like to say a little more about Jerry Vinograd, who was one of Pauling's better appointments. There was a fair amount of resentment among the rest of the chemistry faculty with respect to Linus Pauling. When he was chairman [1937-1957], he had a lot of research money for his work, because it was rightly recognized, especially by the Rockefeller people, that this was a major frontier in biomedicine. As a result, he was able to make faculty-level appointments without consultation and for the convenience of his program. Some of those appointments weren't so good; some were very, very good. One of them was Vinograd.

Vinograd came to Caltech after the war [1951] because he was unhappy at Shell Development and wanted to do something in biology and physical chemistry of biological macromolecules. Pauling had the perception to grab Vinograd right away and put him to work. I think Pauling wanted him to do particular things, but Jerry was driven, and he started working on the things he wanted to work on. Vinograd marched to his own drum, and it was actually psychologically impossible for him to work on other people's ideas—postwar ideas, anyway. He was a senior research fellow, with a non-tenured appointment, writing independent research grants. Vinograd's status at Caltech depended on renewing his grants, and he just couldn't psych himself up to write a grant. He kept promising he would write one, and he just wouldn't. I'm sure he was psychologically disabled because of his dependent, non-tenure status.

Ernest Swift [chairman of the Division of Chemistry and Chemical Engineering, 1958-1963] was a gentleman of the old school and a gentleman in the real sense of the word, a person of the highest integrity. As a human person, he felt you do things with responsibility. His view was that if Vinograd wasn't responsible, his appointment would have to be terminated. Finally, I organized a movement and wrote a document and got considerable support from people in biology that summarized Vinograd's outstanding achievements and urged his appointment to a tenured faculty position. I think it was in chemistry and biology, but it might have been just in chemistry, and then the biologists invited him to become a faculty member there. This was a really convincing case that I put together. The chemistry faculty approved, and Jerry got this joint professorship [1966] and did marvelous work for a long, long time until a very bad heart condition first slowed him down and then led to his death [1976].

NORMAN R. DAVIDSON

SESSION 3

September 3, 1987

Begin Tape 3, Side 1

DAVIDSON: Today I'd like to talk about a period of time beginning in 1968, when I became chairman of the faculty. I had the impression—it's all rather hazy—that I was the first of the activist chairmen of the faculty and not just a passive figurehead who presided over Faculty Board meetings. I started the practice that the chairman of the faculty got involved in negotiating with committees concerned about student welfare and tried to encourage the practice of listening to students. The student protest movement had started, but things were relatively tame at Caltech compared to other places. There were no sit-ins about the Vietnam War or anything like that. But I think I contributed a lot to encouraging student input into faculty affairs and participation in faculty committees concerned with student affairs.

ASPATURIAN: Was there a lot of opposition among the faculty or administration to the idea of putting students on the committees?

DAVIDSON: My recollection isn't very precise, but I think no. There was a certain amount of foot-dragging, along the lines of, "Well, if these guys are on the committee, how can we discuss such-and-such? Won't they be irresponsible?" or "They won't come." But I think in general, there wasn't very much opposition. When I try to recall this, I can't remember that a lot was specifically accomplished on any of these perennial issues, but I think the style of the office of chairman of the faculty, which has been carried on by most of the individuals who served in that capacity, was significantly influenced by what I did there.

Now, I suppose as a result of that, I was appointed to the next presidential selection committee, when Lee DuBridge was going to resign. Bob [Robert P.] Sharp [Sharp Professor of Geology, d. 2004] was the chairman. The two leading figures in the final decision, insofar as I can judge, were Bob Sharp and Arnold Beckman [chair of the Board of Trustees, 1964-1974], who got along well. There were lots of people on the committee, but when we got down to a couple of [candidates] at the end, Bob was the one who skillfully led us in the direction that he thought we should go, without forcing us in that direction, and skillfully kept Harold Brown interested. The other strong candidate was [James] Fletcher, who's now the head of NASA. He looked pretty good, but he came and somehow he just seemed a little less open and a little more resistant to change. If anything, he was maybe a little more capable of being criticized with regard to a stand on the Vietnam situation than Harold Brown, who came from his position in the Defense Department. I can't really remember all the reasons—it was 6.5 for one and 5.5 for the other, not 10 to 1—although I think history has supported the wisdom of our decision.

ASPATURIAN: How did those two men get to the top of the list?

DAVIDSON: A certain number of people were eliminated because inquiries indicated that they simply weren't interested. One was Donald Kennedy, who's now the president of Stanford, even though he was quite young then. He was chairman of the Biology Department at Stanford then, and he wasn't interested. We even talked about John [W.] Gardner [secretary of health, education, and welfare, 1965-68], but it was clear that he wasn't interested. There were a fair number of people who didn't seem to have the breadth and the stature. So by this process of elimination we had Brown and Fletcher. We did the same thing that was later done with Murph [Marvin L.] Goldberger [Caltech president 1978-1987] but was not done with the new president [Thomas E. Everhart]—namely, at the very end, when we were down to about two people, they both came back openly as people who had been interviewed for the presidency. They hadn't said they wanted the job, but they hadn't said they didn't want the job. They did come openly, and they interviewed with a large number of groups, including the Faculty Board, various subsets of the faculty, and students, and they were asked a lot of questions. The impressions that they made then, in terms of openness to other points of view and what their own views were on various matters, did have an impact. I think the recent presidential search strictly avoided that, which I think reflects a feeling that this approach wasn't optimum in the case of the search that led to President Goldberger's appointment. I have my own doubts about its usefulness the second time. But in the case of Harold Brown it was absolutely essential, because if we were going to bring in somebody from the Defense Department at that time, people had to realize that he wasn't your typical general, or like the secretary of defense now, Cap [Caspar] Weinberger.

One of the issues that was commonly discussed in this committee—and I suspect has been commonly discussed perpetually—was how far Caltech should go in the direction of becoming a broader-based institution. A large number of activists—Murray Gell-Mann [Millikan Professor of Theoretical Physics, emeritus] is like that—feel very strongly that Caltech would be improved if it went further in the direction of being less of a primarily natural-science university. Even if it doesn't have an interest in all fields as a typical university, it should include fields of human behavior and the subjects that can be studied by empirical observation and analytical, especially mathematical, thought. Experience has shown that this is an issue where the bulk of the faculty, when the chips are down, are very uncomfortable with subject matter that can't be tested experimentally.

ASPATURIAN: Are particular disciplines more troubled by this than others?

DAVIDSON: Yes, probably more engineers. The traditional view is that the engineers are more conservative, and I think that has somewhat held up, although there are some ways—which I'm going to point out—that the biologists were very conservative. I think as a subset of people, the physicists tend to be most imaginative in terms of their thinking and to be on various government committees concerned with analysis of policy problems with a technical component. There's considerable variability and diversity in every division, but the general feeling is that if you counted votes on various issues, you would probably find that engineers were a little conservative.

During the presidential search, that was certainly an issue that was strongly discussed—whether we should try to get a president like John Gardner, with a broader vision of the interrelation of science and society than is traditional among the Caltech leadership. I think Harold Brown came here encouraged by the activists to think that Caltech would move in that direction. I think Brown had the idea, based on his experience and leadership in the various organizations that he'd served, and especially in the Defense Department, that the guy at the top could have a major impact, could do things. And what he discovered is that an academic faculty is a Maoist society, and you can't do anything unless the faculty believes in it. If you can influence a significant group of faculty to move in one direction, you could get new initiatives going, but the issue usually has to be faculty-initiated and always faculty-supported. Harold was a very flexible, realistic person, and he learned that lesson fast, insofar as I could tell.

ASPATURIAN: Is that more true here than at other major institutions?

DAVIDSON: I don't think so. From what I hear about MIT and other places, no. But I haven't been anywhere else; it's all hearsay.

But my particular vision at the time—and I can see both sides of the question—was that Caltech should become an institution that strived for excellence in areas where intellectual activity and research impinged on human welfare. I have always felt that it's more important for us to have very good teaching in the real humanistic subjects, [like] literature—that that's what our students need. History, I think, is on the borderline between an empirical science, where you draw conclusions about human behavior, and humanistic subjects. I don't think we need more research on Shakespeare; I think we need to have faculty who can teach young people how to appreciate it. I can't evaluate the attitude that many in the Humanities and Social Sciences Division have—that the best research people are the best teachers. From what I hear, we certainly have some pretty good teachers in our present Humanities and Social Sciences faculty. But the teachers we had between 1945 to 1955, when I knew them well, were people who were much less interested in scholarship but, insofar as I could tell from the students, were very exciting teachers.

In any case, we have moved in that direction: We have some people in anthropology; we have some people in economics. To some extent, Caltech has moved in those directions—perhaps less so than MIT.

There's one particular, minor event in the history of Caltech that I would like to record my view about, because it's distinctly a minority view. At one stage of the game, Immaculate Heart College was in fairly deep trouble financially. This was already at a stage where women were admitted to Caltech as graduates but we were not coeducational as far as undergraduates were concerned. I've always really deplored the monastic aspects of life for Caltech undergraduates, as well as graduate students. Even now, we're only quasi-coeducational. And I also deplore the situation in which Caltech students don't meet—in the natural course of their day-to-day student life, as distinct from weekend, social life—a diverse group of students of both sexes with primary interests other than natural science or engineering.

Consequently, when the possibility of some sort of federation with Immaculate Heart College arose [December 1969], I was very much in favor of it, and so was Harold Brown. My picture was that Immaculate Heart College should be relocated adjacent to Caltech and that there would be common classes but that they'd be distinct academic institutions. Immaculate Heart College may not be Mount Holyoke or Vassar, in terms of the SATs of its students. But it did have a spirit of innovation and independence, it was interested in nonconventional methods of education, and it certainly was not a typical women's Catholic college.

It was clear that there would be a lot of problems about this—there'd be separate faculties with separate criteria for selection of students. Nevertheless, I felt that it would be, in the long run, very good for Caltech—especially for Caltech undergraduate life and the Caltech educational experience. But it was absolutely torpedoed in a faculty meeting, for basically two reasons. Among the senior biology faculty, especially, who are very liberal in other respects, there was a real conservative streak, a distrust of any affiliation with religious organizations. They felt this way about the Catholic Church in particular, because of the bitter fights a hundred years ago between the Church and scientists dealing with the theory of evolution. People like Norman Horowitz and Ed [Edward B.] Lewis [Morgan Professor of Biology, d. 2004], colleagues with whom I share values on almost everything else, were absolutely adamant about that—we'd be totally tainted by association with any Catholic institution. The fact of the matter is that Immaculate Heart College was run by a very liberal group of nuns. I'm fuzzy about the details, but basically because they were unable to agree with the Catholic Church, I think they severed their formal affiliation with it shortly afterwards. I think that [my colleagues'] concern was just absolutely invalid, but it's still expressed. If you talk to young people now, among my colleagues in biology, and if you talk to senior people in biology, they would say that that was the worst thing Caltech ever tried to do. People including my good friends Eric Davidson [Chandler Professor of Cell Biology] , Norm Horowitz, and Ed Lewis will say this.

The other major concern was the problem of dilution of the excellence of Caltech—a perceived lowering of the standards of the faculty and student body. This point has real merit. However, I still think that Caltech as an undergraduate educational institution—as distinct from an advanced research institute—would have been improved and enriched by such an association.

The only disadvantage I see is that it would have diluted our fund-raising efforts. It's true that there are different sources of funding for development in these different directions, but you can only do so much. Caltech has always been quite successful at digging up resources, and to an extent it might have made us less successful in supporting our mission and research. If that had been hampered, why, it might have been bad, but we'll never know. I think Caltech might have been a different and a better place.

ASPATURIAN: I was interested in hearing more about some of your colleagues' reactions when you decided to move out of physical chemistry and more into the molecular biology area.

DAVIDSON: In general, this is a place that respects independence and initiative. I can't recall anybody making any critical remarks. I can recall a number of questions about how I was going to do it and a lot of expressions of admiration and questions about why I wanted to do it. But the important point is that Caltech is an environment that understands and appreciates interdisciplinary research and science. As I said previously, there were precedents in Delbrück, Pauling, and Vinograd. I think the main thing is that it really was a very supportive environment. Even people who don't know anything about it appreciate people moving into new and exciting areas. There are some instances around here of people who haven't been terribly successful in trying to make comparable switches—so that in a certain sense the proof of the pudding is how the pudding tastes, how things actually work out. In my case, they clearly did work out well, both in the objective scoring of what happens to your research grants under peer review and in the more valid subjective scoring of how your work is perceived by colleagues in your field.

ASPATURIAN: I've heard it said that chemical biologists suffer from a kind of identity crisis. Do you have any feelings on that?

DAVIDSON: I think that's a major problem, which I don't see any satisfactory solution to. In this general time-period of my career switch, when molecular biology was in its infancy, biological education was not molecular. People who came into the field with a good education in physical science and a willingness to learn the new discipline and to ask what they could do about major problems in the new discipline, rather than just a desire to pretend to work in biology while still doing the same darn thing they used to be doing, had a lot to contribute.

There was one additional aspect of the situation. As soon as I made this change, I had enormous numbers of good students who chose to work with me as graduate students—and then a little later, postdocs came to work with me. I had some quite good students in my days as a pure physical chemist, but literally none of them has achieved the academic excellence of my very best students in molecular biology. Three of my former students or postdocs—Ronald Davis, now at Stanford; Phil [Phillip A.] Sharp, at MIT; and Jim [James C.] Wang, at Harvard—are not only professors at major institutions but members of the National Academy of Sciences and regarded as leaders in the field in general. There's also a very considerable number of my former students who haven't achieved quite that standard of recognition. Among the young people going out in mostly the last ten years, there are a number of very, very promising researchers. They're now at the tenured associate professor level and doing very, very good work. The relevant point is that many of these people, especially for starters, had been educated as chemists. Without knowing much about it, they knew that chemistry was a more mature field. While it's clear that innovative chemistry is still being invented and done, it is a more mature field; it's harder to find something brand new. They knew that at that time molecular biology was absolutely wide open, but they were somewhat timid, and their undergraduate education was primarily in chemistry. There was a general phenomenon that these students came to Caltech because they knew there was chemical biology here and that it was in the chemistry division. They felt more comfortable there; they didn't feel ready to apply for admission to a biochemistry department or a biology department until they came here. In some cases, they weren't really decided on what to do until they went around and found out what was going on. Then they ended up with me or with Vinograd, principally.

That isn't true anymore, because a much larger number of entering college students know that molecular biology is a vital, active, still new discipline, although not as new as it used to be. They can get a good undergraduate education in molecular biology, although they know less basic physical chemistry, physics, and mathematics. These are disadvantages, but you can't be effective without specializing. I think it was Robert Oppenheimer I once heard say, "Specialization is a condition for excellence," even though Oppenheimer really appreciated breadth. To become a good molecular biologist now, it's probably not wise to take an undergraduate degree in pure physical science. It might be wise to try to get a little more fundamental physical science into some of our good students. But at that time it was the best way to go, and I was positioned in space and time in this department to really profit from that. And the students did also. As I said, the three students who by the formal criteria, and probably by consensus, have been most successful are Jim Wang, Phil Sharp, and Ronald Davis. Wang was very, very smart, very, very, intelligent, and very, very well trained, and very, very hard-working by virtue of his cultural background. He came from the National Taiwan University with a degree in chemical engineering, I believe. But he didn't want to do that, so he went and got a PhD degree [1964] in inorganic chemistry at the University of Missouri. Then he wrote to me to ask if he could be a postdoc in inorganic chemistry. I wrote back and said I wasn't really doing that anymore, except that we were applying inorganic chemistry to nucleic acids. I don't know whether he knew this was an exciting field or not, but if that's what a bright young inorganic chemist was going to do, that's what he was going to do, so he came. The first things he did with me were a combination of inorganic chemistry applied to DNA, but he rapidly recognized that there were very, very interesting things you could do that went beyond that. In fact, the association with Jim Wang was one where he particularly took advantage of our proximity and collaboration with Jerry Vinograd's graduate students and learned how to use the ultracentrifuge. In the long run, he became excellent in enzymology, molecular biology, and biophysical chemistry.

Phil Sharp was a little different. Phil was very bright and came from some small, undergraduate college [Union College] in Kentucky and was a graduate student in more theoretical biophysical chemistry—biopolymer physical chemistry at the University of Illinois. He realized that research really wasn't central and highly fruitful. So he wrote to me and asked if he could do an experimental postdoc with me, because we were already in our electron-microscope era of studying DNA molecules by those methods and we were clearly dealing with central problems. He came here [1969] and became a marvelously skilled experimentalist. This was entirely dealing with DNA molecules of bacterial origin. Then, at the end, Phil said he really wanted to do something more medical, even though it was a less clearly defined field at the time. His question was, How do you apply what we'd been doing to human biology and to things that were relevant to human biology? We thought about it for a while, and then I realized that Cold Spring Harbor was the right place for him, because that was what they were devoted to; they were taking a fundamental point of view. They also had the resources to take somebody for a second postdoc, which was a very important practical problem—it's hard to get funding to continue. Cold Spring Harbor had always realized, especially under [James D.] Watson, that if you get people who are really, really good, irrespective of their training, they're going to do good work.

ASPATURIAN: It's interesting to hear that philosophy from an administrator.

DAVIDSON: He's a scientific leader first, an administrator second. That was part of his genius, more so than any research after the Watson-Crick discovery.

So Phil went there and basically started a field of research which has developed much more naturally—and you can trace a continuum in subject matter—into his present career as a leader in eukaryotic gene expression.

Ron Davis is even more interesting, one of the most highly original scientists. He came here a graduate student from a well-respected, small college [Eastern Illinois University] in Illinois. He's the prototype of the student who didn't know enough to go from the chemistry department to a graduate program to molecular biology but came to Caltech because he knew he could do biology in a somewhat more protective environment in the chemistry department. This was at a stage in my career when I was still spending my time writing that book on statistical mechanics[7] and not really being as effective as I should have been in selecting and designing problems at the cutting edge on biophysical chemistry and the molecular biology of DNA. So Ron started on something that wasn't really very exciting, for Ron or for me, about dye binding to DNA. But I had recognized early that electron microscopy was a superb way of studying DNA molecules. Ron appreciated that and invented the idea of heteroduplex analysis, which then was the basis of some first-rate graduate work on his part here. He did a postdoc at Harvard, where actually not much very good happened, but somehow the people there realized he was exceptionally good and the world came to know him as exceptionally good, even though none of his papers at that time was of the same high quality as his PhD thesis.[8] He was offered a job at Stanford and went there [1972], and since then there has been a constant stream of original, first-rate work, much of which you couldn't have predicted from what he did a few years ago.

[7] *Statistical Mechanics* (New York: McGraw-Hill, 1962).
[8] Davis, Ronald W. (1970) "A study of the base sequence arrangements in DNA by electron microscopy." Dissertation (Ph.D.), California Institute of Technology.

Let me say parenthetically that one could see and measure the shape and length of individual DNA molecules in the electron microscope. This was infinitely superior to the inferences one made about the sizes and shapes of the molecules in a heterogeneous mixture by classical macroscopic biophysical chemistry.

To come back to your original question, since modern biochemistry and molecular biology departments have a much greater appreciation of the applications of both quantitative thinking and biochemistry in their fields, it's much more difficult for a group of people in a chemistry department to have a separate identity. And if you look at what the people working on central problems do now, the role of chemical biologists in a chemistry department is a very tricky question.

Begin Tape 3, Side 2

DAVIDSON: Judy [Judith L.] Campbell started in the chemistry division but now has a joint appointment in chemistry and biology. In Carl Parker's case, in terms of his research and intellectual interests, he is closer to people in the Biology Division than he is to his colleagues in pure chemistry. In both of those cases, when they came to Caltech—ten years ago in Judy's case, six years ago in Carl's case—the Biology Division leadership was somehow less appreciative of the biochemical approach of trying to reproduce in a test tube what occurs in an *in vivo* situation. So neither Judy nor Carl would have gotten appointments in biology. The fact that we existed then has been very good for the chemical biology group, good for Caltech, and for the Biology Division.

Jack Richards is another person who has made the transition from an organic chemist to a biochemist. He fulfills in part the role that Vinograd and I provided in the 1960s, of somebody who came as a chemist and became a biochemist. But the person who's most clearly in that mode now is Peter Dervan [Bren Professor of Chemistry]. Sunney Chan [Hoag Professor of Biophysical Chemistry, emeritus] to some extent has played that role also. Harry Gray [Beckman Professor of Chemistry] is doing it to some extent, but Harry's keeping his identity as an inorganic chemist, although they're doing very good work on metalloproteins. But these people—Chan, Richards, and I think especially Dervan—are people where a graduate student can come in thinking of himself or herself as an organic chemist, and Peter can say, "Fine, here's the organic chemistry we're doing," but the problem is also important because you contribute methodology to central, cutting-edge problems in basic molecular biology. And his students are doing that. If you look at what many of them are doing afterwards, they're doing their postdocs in standard molecular biology.

I think the point is that we still have a very, very important function to play, but it's a lot more difficult. People like Campbell and Parker are not particularly interested in what [Robert H.] Grubbs [Atkins Professor of Chemistry] does and what [John] Bercaw [Centennial Professor of Chemistry] does—although they're both excellent scientists. And Bercaw and Grubbs are not especially interested in what *they* do. They're happy to know that it's recognized by their peers as first-rate work. I see this as a difficult situation for a chemical biology sub-department. If you're going to have people who are both at the cutting edge and doing work that the nonbiological chemists can understand, it's going to be more difficult to get people who are really at the cutting edge. I'm somewhat pessimistic about the long-range prospects for the subject. Right now we have the idea that it's time to go back to redevelop strength in structural chemistry of the X-ray type, which for long and complicated reasons died out after Pauling and [Robert B.] Corey [professor of structural chemistry, d. 1971] went. Then [Richard] Dickerson [professor of physical chemistry 1963-1981] came, and Dickerson didn't stay. But I think that field is just about to a stage where it can play a role as something that unifies the interests of chemists and biologists and that both of them can understand. It's a very fitting activity for a chemical biology subject. So I think we'll see more of that.

ASPATURIAN: I was wondering how these developments affected the relationship between chemistry and biology on this campus?

DAVIDSON: To me, there has always been a reasonably good working relationship. We've had common training grants. There are a certain number of perks that go with appointments in division A or division B that result in inequities. But to me, the important thing is the relationship between individuals and the flexibility with which collaboration can be created, and these things are fostered by Caltech. I think that's always been true.

APPENDIX

NORMAN DAVIDSON'S REFLECTIONS ON LINUS PAULING

Received August 31, 1987

Part of your proposed agenda deals with my recollections and views of Linus Pauling and his role in the history of the Division of Chemistry and Chemical Engineering. I would like to respond to that in an organized way now.

I first wish to note that I expressed my appreciation of Linus Pauling as a scientist and, in less detail but equally positively, as a compassionate human being and as a great contributor to the peace movement in my address at our recent eighty-fifth birthday fest for him. I very much hope that that speech was taped and can be put in the Archives and that my remarks below will be considered in context with the opinions expressed there. My enumeration of his contributions and my analysis of his genius can only be summarized here. As a parenthetical matter of interest, I stated that I considered him one of the three greatest scientists of the twentieth century. There were some raised eyebrows at that remark; it was obvious—and many, many people agree—that the two greatest are Einstein and Pauling. There is a cult of people—and I am one of them—that puts Enrico Fermi close to the top. However, he is probably a less clear-cut choice than Pauling and Einstein.

Pauling's genius includes an extraordinary ability to use general theoretical principles and his sense of geometrical shapes and relations to deduce complex molecular structures—mineral, proteins, and alloys—especially from limited X-ray data, with the discovery of the alpha helix being probably the crowning jewel of his achievements. He more than any other person saw that specific non-covalent interaction between a protein and another molecule—for example, the antigen-antibody interaction, the enzyme substrate interaction, and, as we now know, receptor-ligand and receptor nucleic acid interactions—was due to complementarity of shape and to conventional weak interactions, i.e., Van der Waals electrostatic and hydrogen bonding forces. His understanding of the role of resonance of the amide group in restricting the shape of polypeptides and of the role of H-bonds, plus his clarity of thought, which led him to reject the requirement for a simple integral helical periodicity, led him to the discovery of the alpha helix, where physicists like Bragg, Astbury, and Perutz had failed. He, more than any other person, formulated the quantum mechanical approach to chemical bonding and applied it with fantastic intuition to chemical problems, most notably to the magnetic properties of transition metal ions and to the properties of aromatic molecules. By his intellectual audacity and pure thought, he stimulated the research that led to the discovery that sickle-cell hemoglobin was a molecular disease and thus helped to establish the paradigm that protein sequence is encoded in the genome. Unfortunately, his contributions to basic molecular biology end with this and the alpha helix.

As a human being, Linus Pauling was compassionate and courageous. There are instances of his generosity to young underpaid faculty. More important, there are instances of the courage and lengths he went to to protect younger and more vulnerable colleagues from persecution in the heyday of the several un-American activities committees. Incidentally, Linus Pauling did a magnificent job in defending himself from such persecution. He had an extraordinary ability to think on his feet and respond to hostile questioning with answers that destroyed the opposition. He made a monkey out of Senator Dodd.

But there is another side of the coin that I feel I should address here. There was a large group of scientists, who were not familiar with what went on here, who have canonized Linus Pauling and accuse the chemistry division of mistreating him so that he felt obliged to leave. Of course, there is also a large group of uninformed people who canonize him because of the absurd vitamin C and megavitamin business. On the other hand, the issue of the atrocious treatment by the Caltech trustees and top administration of Linus Pauling because of his crusade against nuclear testing is a quite different issue. I don’t have much firsthand knowledge about this, but my impression certainly is that the Caltech leadership behaved badly.

But I do want to place in the record my perceptions of some of the negative aspects of Pauling’s scientific influence at Caltech in the period of time just before his departure. He had been spectacularly successful in stimulating Harvey Itano, Jon Singer, and Bert [Ibert C.] Wells to do the work that led to the discovery that sickle-cell anemia was a hereditary defect in a hemoglobin molecule. Sometime thereafter, he initiated a program on determining whether phenylketonuria and then later other development, mental inherited disorders could be similarly simply explained. This program involved some quite good scientists, including Ken Shaw, Tom Perry, and J. F. Catchpool. It was not successful. It degenerated into a totally empirical chemical analysis of the components in urine, or other available specimens, from patients—

mostly mental patients. This sort of research is possibly appropriate in a clinical institution—although even that is dubious—but it most certainly was not in an institution devoted to basic science. It was not good basic biology and it was not good basic chemistry. At the same time, the chemistry division was eager to expand its activities in basic chemical sciences—a growing and exciting area. We were falling behind. Consequently, Jack Roberts—and to his credit Linus Pauling had a significant role in recruiting John D. Roberts—as division chairman, told Linus Pauling he had to contract his space. I believe Pauling's indignation about this was a significant factor in his decision to leave Caltech. I hope I have made it clear that in spite of the indignation of many Pauling admirers elsewhere, Caltech chemistry did the right thing. We needed the space for basic chemistry, and our position of leadership now required this drastic action.

Incidentally, the same kind of totally empirical search for a biochemical correlate of schizophrenia and other mental disorders has been one of the major activities of the Linus Pauling Institute in more recent years. As far as I can judge, there is no attempt to analyze mechanisms and to guide empirical searches by some rationale based on causes. Insofar as I can tell, the Linus Pauling Institute is not recognized as a leader in biomedical research.

Linus Pauling was fantastically creative, but he was never good at self-criticism. The extreme case of this is the vitamin C business and the later megavitamin therapy. Incidentally, I think the reason that Linus Pauling became so contemptuous about the medical establishment was apparently because of the way that they mistreated his Bright's disease in the late 1930s and early 1940s. Some unorthodox physician in San Francisco—his name is in the record, but I don't know it—instituted a therapeutic regimen that in Pauling's opinion led to his cure. I believe all this has been recorded elsewhere, but it should be mentioned here to understand, in part, the vitamin C fiasco.

Finally, I wish to deplore the tremendous focus and interest on Linus Pauling's role in the history of the chemistry division for the period from the mid-1950s to the present. His last great scientific contribution was the discovery of the alpha helix in 1950. In the meantime, there have been a number of very important contributions to the intellectual history of the chemistry division from many faculty. I am not the person to record them in detail, but I do want these notes to include my opinion that these developments were more interesting and more important for Caltech than Pauling's latter years here and his alienation.

I believe it was J. D. Roberts who recruited George Hammond [professor of chemistry 1958-1972, division chairman 1968-1972]. Hammond did some superb science—especially in discovering reactions of triplet states. He had a quite different vision of chemistry than Pauling's—his emphasis was on "chemical dynamics." This was overdone too, but it contributed to the present balanced intellectual views and strength of our division. Hammond was the lead man in recruiting Harry Gray, another great chemist. Many of our later appointments, especially of young people, have been superb.