Oral History
Norman Davidson

Norman Davidson, 1966

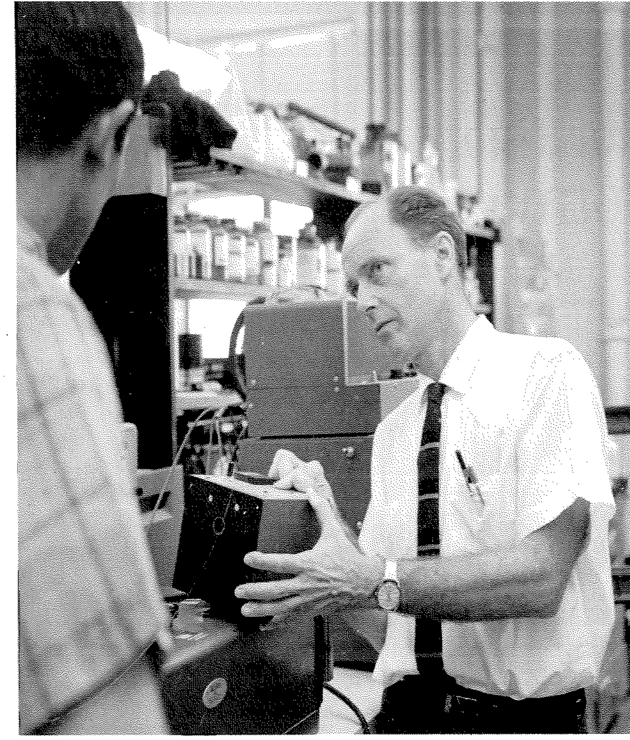
Norman Davidson, the Norman Chandler Professor of Chemical Biology, who was recently awarded the National Medal of Science (see page 43), began his career as a physical chemist. He was recruited to the Institute in 1946 by Linus Pauling and later joined in an honorable Caltech tradition of switching from the physical sciences to biology. In this brief excerpt from his 1987 oral history (he was interviewed by Heidi Aspaturian for the Caltech Archives), he describes his conversion.

HA: Who was your main conduit?
ND: Linus was one. Linus was *the* example of a person who had the 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 John 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. . . .

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, 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. . . One day, I remember, a guy named Frank Schmidt came to visit. Schmidt 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. . . . 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



Former chemist Davidson uses a spectrophotometer to measure DNA concentrations (1966).

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 57 minutes were as bad as the first 3. And I never had the guts to walk out when he did.

I forget who all was there among the physical scientists. Charlie Townes, who invented the laser, was there. He toyed with the idea of becoming a serious biophysicist, but never did make the switch. Bruno Zinn 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, 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. . . .

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 that 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. . . .

HA: 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.

ND: 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. 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 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. □