Photograph by H. Shoebridge, courtesy R. Owen

RAY DAVID OWEN
(1915 – 2014)

INTERVIEWED BY
RACHEL PRUD'HOMME

October – November 1983

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area

Biology, genetics

Abstract

Interview with Ray Owen, Professor of Biology at Caltech, conducted by Rachel Prud'homme in 1983. The interview covers Owen's early life growing up in Wisconsin, where he attended the University of Wisconsin, studying cattle genetics with C. Stormont; his early research on chimerism in twin calves and on immunological tolerance. In 1946 he joins the biology division faculty at Caltech in genetics as a Gosney Fellow. Recollections of genetics at Caltech following World War II: concurrent arrivals include G. W. Beadle, N. Horowitz, H. Mitchell, and L. DuBridge. Recalls T. H. Morgan's reputation and his colleagues A. H. Sturtevant and S. Emerson. Other members of the biology division at this time include C. Bridges, H. J. Muller, H. Borsook, A. Haagen-Smit, C. Wiersma, A. van Harreveld, and F. Went. Recollections of L. Pauling. His book, *General Genetics*, with A. Srb published in 1952. His work with D. Lindsley on bone marrow transplantation. At Caltech, involvement with freshman admissions. In 1961 becomes biology division chair. Discusses teaching and further work in student affairs, including admission and recruitment of women, the Committee on the Freshman Year, and pass/fail grading. Appointment in 1975 to dean of

students and vice-president for student affairs. Involvement with National Cancer Program (1972-1975) and continuing research on immunological tolerance. Concludes with observations on genetic engineering and safety of genetics research.

Administrative information

Access

The interview is unrestricted.

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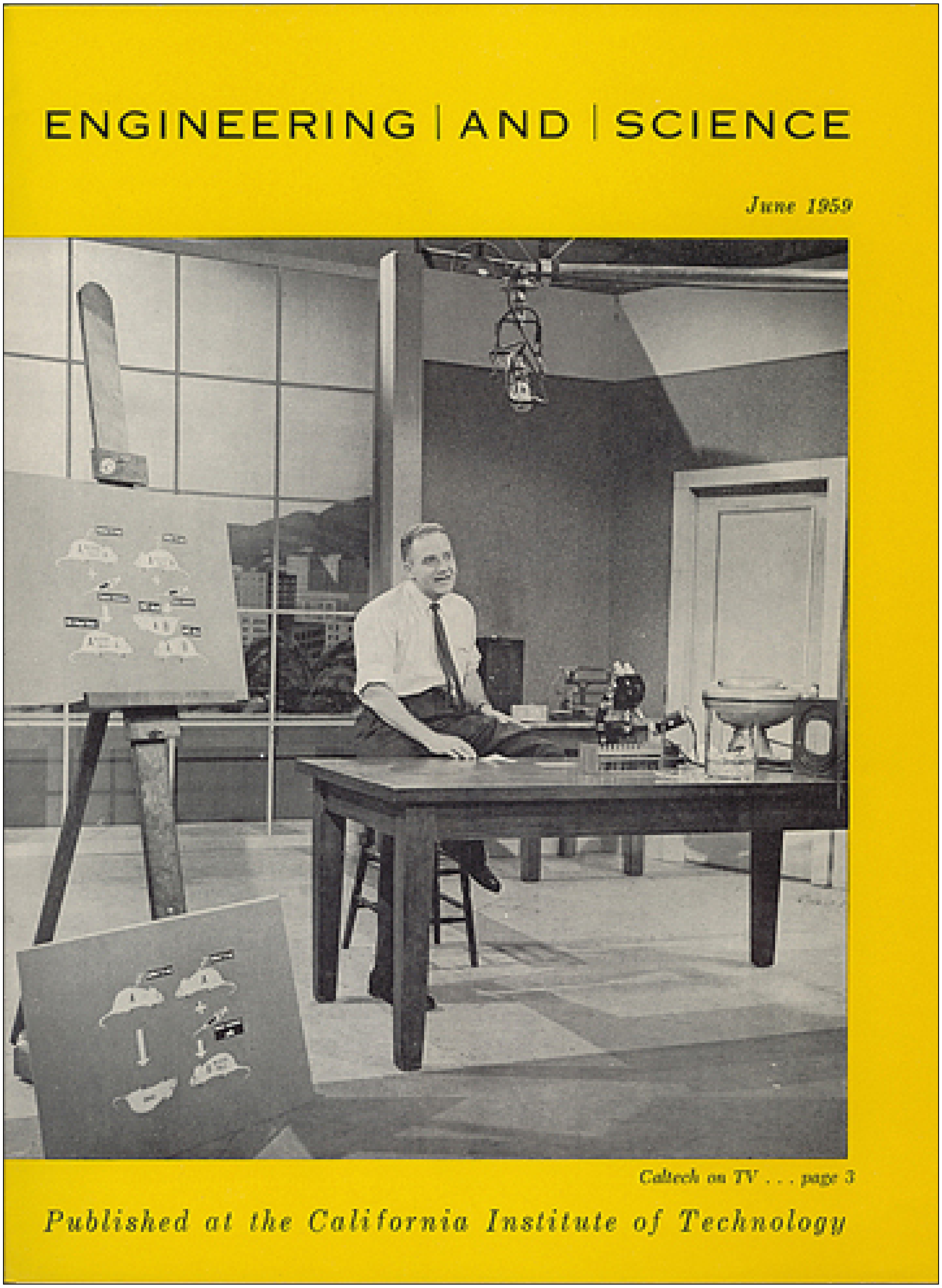

Ray Owen was a pioneer in Caltech's television history. He spoke to the cameras on the institute's first TV series, "The Next Hundred Years," aired between November 1958 and May 1959. In "Facts for a Friendly Frankenstein," Owen talked about experiments in tissue transplantation aimed at understanding the immunological barriers to foreign tissue acceptance. Though still possibly perceived as Frankensteinian at the time, in fact the first successful renal transplant between identical human twins had taken place in Boston in 1954. Image courtesy of *Engineering and Science*.

California Institute of Technology Archives

Oral History Project

Interview with Ray David Owen

by Rachel Prud'homme

Pasadena, California

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in biology.

Discusses his Caltech teaching experiences: courses taught; interaction with students; increased interest and funding in biology research. Debates the practice of professors' names appearing as research contributors on their students' publications. Recollections of former graduate student W. Hildemann and his research on scale rejection in goldfish. Discusses the influence of the Honker group, C. Rogers, E. Swift, and R. Bacher on ways to improve Caltech student life, both academically and socially. Describes the concerns of the Ad Hoc Faculty Committee on the Freshman Year: the debate and implementation of the pass/fail grading system for the freshman year; the reduction of the freshman course load to encourage more options; the debate and eventual decision to admit women undergraduates.

Session 4

Further recollections regarding the admission of women to Caltech and their first years here; women's preferences in the sciences. Discusses work on the admissions committee: the recruitment of women; the admissions standards; the interview procedure; the high school visitations; the recruitment of minorities. Compares the Caltech students by decades; discusses the creation of an independent studies program.

Discusses the 1960 Nobel Prize in medicine given to P. Medawar and F. Burnet for work on immunological tolerance. Recollections of L. Pauling, and the presidencies of L. DuBridge, H. Brown, and M. Goldberger. Discusses his retirement as biology chairman in 1968 and the circumstances leading to R. Sinsheimer becoming chairman; his desire to return to teaching and to reestablish his research program. Briefly describes the research done by his graduate students J. Frelinger, L. Blankenhorn, J. Klotz, S. Melvin, and S. Ostrand-Rosenberg. Discusses his participation on the National Cancer Program from 1972-1975 and the debate surrounding its funding and research; recollections of the program chairman B. Schmidt.

Session 5

Continues recollections of the National Cancer Program and of meetings in Washington with Presidents R. Nixon and G. Ford. Discusses his appointment in 1975 by H. Brown to become dean of students and the vice president for student affairs; describes the responsibilities of the positions and the need to improve the overall well-being of the undergraduates and their life on campus. In the early 1980s, becomes the biology undergraduate advisor; describes the duties of the Biology Undergraduate Student Advisory Council (BUSAC). Discusses the qualities considered when evaluating the performance of professors as teachers and the need for student feedback for such evaluations. Briefly describes his life as professor emeritus.

Discusses current research in genetic engineering and the safety of such research. Describes his most rewarding work: in research, his contributions in immunological tolerance; in administrative capacity, his work as dean of students and biology undergraduate advisor. Lists the positive influences and aspects of having been associated with Caltech.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Ray David Owen **by Rachel Prud'homme**

Pasadena, California

Session 1	October 20, 1983
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PRUD'HOMME: Mr. Owen, tell me about your background.

OWEN: I was born on a dairy farm near the little town of Genesee, Wisconsin, which had a population of 180. I grew up on the farm. My father was an immigrant from Wales. When he was about sixteen or so, his father had given him a watch and sent him out in the world, as he had with his other sons. My father came over here on a cattle boat carrying purebred dairy cattle from the island of Guernsey, and he took a job as a herdsman, in northern Illinois. But his ambition, as with other young people in those days, was to get a farm of his own. So he went up to Wisconsin, married a local girl, and started farming. I was born in 1915, during the First World War.

PRUD'HOMME: So you were a farmer's boy. Were you interested in science from the outset?

OWEN: No. I went to school in Genesee State Graded School, which had two teachers for eight grades. Grades one through four were all in one room; so the first graders listened to the second and third and fourth graders as they went up to the recitation bench.

PRUD'HOMME: A one-room schoolhouse.

OWEN: Two rooms. There were three or four other farm children in my class. At first I wasn't too keen about school. The teacher would give us first graders things like sewing cards to keep us occupied while the other students were reciting. I had a terrible time with those sewing cards. As far as I'm aware, I really got interested in what would now be called a scientific direction in about fifth grade, when we studied physiology and talked about the blood circulation and things like that. I found myself both interested and sort of good at it.

PRUD'HOMME: Growing up on a farm would probably help.

OWEN: Yes, I think growing up on a farm was good in many ways. You were part of a family enterprise, and you witnessed things like birth and death and husbandry as part of your daily life, and you were interested in plants and animals. By the time I got to high school, biology had become a most lively interest of mine—although I majored in vocational agriculture in high school, and the idea was, naturally, that I'd be going back to the farm after high school. I think I was the first person in that community to go to college.

PRUD'HOMME: I was going to ask you if it was common to go to college.

OWEN: Not there, in those days. In fact, for quite a while, I was probably regarded as an unusual person to come out of a farm community.

PRUD'HOMME: Did your family support your decision?

OWEN: Oh, yes. My father and my mother both took pride in the fact that I seemed to be doing very well in school. And they were interested in my interests, although neither of them had had more than an elementary school education. Then in high school, I was most influenced by an English teacher named Miss Grubb. She looked upon me as college material and tried to see that this shy boy off the farm who was going back and forth doing farm chores all the time, not much involved in high school work, took up the job of being the yearbook editor and things like that. She consciously steered me toward college. I remember a conversation between her and my teacher of vocational agriculture, a guy named Jones, in which Miss Grubb said, "He should take a foreign language." And Jones said, "What the hell do you expect him to do, swear at the cows in French?" [Laughter] This was a small city high school, in Waukesha. We lived eight miles from it and I commuted every day, of course. The other teacher who influenced me the most was the biology teacher, Miss Nehls. In fact, after I went to college I did some practice-teaching in the high school toward a teacher's credential, with Miss Nehls as my supervising teacher.

PRUD'HOMME: Where did you go to college?

OWEN: I went to Carroll College, a small Presbyterian church-connected, liberal arts college, in that same community, Waukesha, Wisconsin. And I continued living on the farm during my first three years of college, commuting back and forth and doing farm chores morning and night.

PRUD'HOMME: How did you pay for college?

OWEN: I was on scholarship. The total cost was about $200 a year. I remember quite keenly that the summer after I graduated from high school, I was back on the farm. Miss Grubb had urged me to go to the University of Wisconsin, but those were Depression times, and we didn't see that we could afford that. I was just going to stay out of school, at least for a while. And then a field person for this little college, who happened to be the speech teacher, came out and urged me to come to Carroll College. He said they'd give me a scholarship, so I went. It turned out that one of the most important things that happened to me in college was meeting June, now my wife, who also came from farm stock in Wisconsin. We were associated especially with speech activities—debating, and things like that. She was there on the same kind of scholarship. This college had recruited some pretty good people during those Depression times, and made it possible for them to go to college when otherwise they would have found it difficult. But at the same time, the college kind of trapped you. After a year or two, some of us thought we might try to transfer up to the university for a more diverse and intellectual program. But we found out then that the college defined our scholarships as loans, and if we transferred out, we would owe them some money. [Laughter] So we stayed and graduated. I've never been sorry that it worked out that way.

PRUD'HOMME: Is your wife a scientist, too?

OWEN: No. As an undergraduate she majored in history. Then she did graduate work in economics at the university. So she represents a different kind of intellectual interest.

PRUD'HOMME: Who were your mentors in college? Did you have any teachers whom you remember well?

OWEN: Yes. At Carroll, to begin with, an English teacher named Miss Smith. I remember doing an essay on *The Pickwick Papers* that first Christmas back at home on the farm, with the big pot-bellied isinglass-windowed stove, smoking my first cigars and writing about Dickens. The chemistry professor, Ward Ray, was a very intellectually active person; I took quite a bit of chemistry. The other day, I was looking at my old college texts. One was an introductory college chemistry text by Harry N. Holmes, a revised edition 1931, with 1932 printing. I found I'd written in the front, in April and May 1935, some things that Professor Ray had said that I thought were interesting. They didn't have much to do with chemistry. For instance, I'd written, "Some of these days, when the coal and oil are gone and when our wood supply becomes insufficient, we're going to get our energy from the sun, using it to synthesize fuel in the same way that nature does it now, and more efficiently."

PRUD'HOMME: He was a futurist.

OWEN: That's right. Some of his comments look now very prophetic.

I had decided to major in biology. It was a one-person biology department—Professor Ralph Nanz. He taught everything—botany, zoology, microbiology, evolution, comparative anatomy.

PRUD'HOMME: How many students were there in the department?

OWEN: I would guess, maybe, ten or fifteen of us majoring in biology.

PRUD'HOMME: And were you still living on the farm?

OWEN: Yes, that did limit some of my involvement in college, but at the same time, it was a good way for a shy young farm boy to develop. Carroll College was a relatively small environment where it was easy to get a feeling of success and reward, and to undertake positions of some leadership. I even learned to dance when I was a freshman. It was a school in which the social life was divided between the Greeks and the non-Greeks—that is, fraternities and sororities played an important part. And I was, of course, a non-Greek. But I belonged to clubs, and so did June. She worked her way through college as domestic help for a professor's family; she cooked and did housework. I belonged to a club called the Pioneer Club, which took pride in its social program and its excellence in academic things. Then, during my senior year, when I was president of that club, I lived in the house off campus and didn't get back to the farm so much. In fact, from then on, I got separated from farm life.

PRUD'HOMME: It was a nice, easy transition, then.

OWEN: Yes.

PRUD'HOMME: Did you work to earn money?

OWEN: I did, of course, during the summers, but during the school year, for those first three years, I just worked on the farm. I didn't get paid by my father or anything, but it did help to get the farm work done. During the summer after my junior year, when I wanted to move into the Pioneer House, I sold Fuller Brushes. And that was a good experience in many ways, too. The Fuller Brush Company developed good salesmen, rewarded them if they did well. I got a little old Model T-Ford and went around the countryside selling brushes.

PRUD'HOMME: If you were shy, it must have been difficult.

OWEN: Well, by then it was easy. It was an interesting time, because it was in the depths of the Depression—1936. As you went through sections of the town of Waukesha, you'd meet truckers' families who were unable to operate their trucks because they couldn't afford to buy licenses. There was a lot of unrest connected with the economic depression. It was an education.

PRUD'HOMME: Was your family affected by the Depression?

OWEN: Not much. Farm families were able to be reasonably self-sufficient. We grew our own food, and my mother preserved and canned things. We always had lots of milk and dairy products. The thing that we didn't have was money. My mother was a wonderful cook. But I still think that in contrast to homemade things there's something special about having a bought cookie, you know. [Laughter]

PRUD'HOMME: When did you get married?

OWEN: June and I dated during college. After we graduated, in 1937, I went up to the University of Wisconsin, and she took a job as a high school teacher in Wautoma, Wisconsin for a year. Then she came to Madison, too, and took a job in the State Department of Taxation. Then she started her graduate work in economics at the university. We got married the year after that, while I was still a graduate student. In those days, it was frowned on by your major professor if you got married. You were regarded as not a properly dedicated young scientist. But it was really a pretty good life. We were in a close-knit society of other married graduate students who didn't have much money either.

PRUD'HOMME: What made you decide to go to the University of Wisconsin? Did you have any choice, or was that just the obvious place?

OWEN: Well, as I looked forward to graduation from college, I thought I would probably become a high school teacher. I didn't really expect that I'd be going on to graduate work. Again, it was a very uncommon thing at that time and place. But then, I had served as a teaching assistant for Dr. Nanz. And I had become particularly interested in human genetics, in part from the viewpoint of the then current concern with eugenics. There was concern about what was thought to be the deteriorating genetic quality of the human race as a result of differential reproductive rates, in which the less intelligent people were having larger numbers of children. At that time, that was a fairly dominant feeling among socially-oriented biologists. And I got caught up in that to a certain extent. Professor Nanz very firmly held that kind of opinion. Also, during my senior year, I had entered a national essay competition, sponsored by the biological fraternity Beta Beta Beta. My essay won the competition and was published—*Bios* 8:50-56, 1937—so that became my first publication. And, in a way, that oriented me toward going on, and attracted a little attention. I have a reprint of that essay. I was looking at it and thinking of my talk with you. It's the work of a callow young undergraduate in a little, relaxed college. And it was on a pretty dull subject, "The Place of the Study of Biology in the Educational System of the United States." But as I read it now, it's interesting to see what I was thinking then. It's not commonly the case to have a record like that of what you were thinking when you were young. The essay's not too bad, although I wouldn't bring it out to show it to my scientific colleagues. There's a little section in here in which I talk about why biology is unique among the sciences, and it gives you an idea of why a young person in those days would decide that biology would be a subject to which he might want to devote his life.

PRUD'HOMME: At the University of Wisconsin, who were your professors?

OWEN: My major professor was L. J. Cole. He had established the Department of Genetics, had been its first professor back around 1909, at a time when it was called the Department of Experimental Breeding and was, I think, the first department of genetics in the United States. Cole worked on birds—pigeons and doves primarily—and was interested in what we would now consider to be rather straightforward Mendelian genetics. He also invented the technique of bird banding to follow bird migrations. It was in connection with my work in his lab that I got started on several things. For example, Willard Hollander—who was one of the more advanced graduate students—and I did a study of iris pigmentation in chickens and in pigeons. Willard had noticed that if you have a gene that affects melanin formation in pigeon plumage, it also is likely to affect the coloration of the iris. He had also noticed that in the chicken that wasn't true; gene mutations that affected melanin in the plumage left the iris its regular orange color. I remember we arranged with a local place that prepared poultry for the market to have them save the heads of the chickens for us. We got bushel baskets full of chicken heads and fished out the irises and did some simple chemical things to them—and also pigeon irises; and we found that sure enough, although the irises of the pigeon and the chicken looked alike, their chemistry was essentially different. The pigment of the chicken iris was a carotenoid, fat-solvent extractable substance, whereas the pigment of the pigeon iris was melanoid. This was before [George W.] Beadle's work on biochemical genetics in *Neurospora*. As I look back on it, I think what we did reflected an emerging perception that genes act in biochemical pathways. That was very satisfying.

I also collaborated with another graduate student, on a project involving a male rat that was semi-sterile. The females with which this rat was bred had small litters. We found that almost half the fetuses were being resorbed very early in development. And we figured out that a translocation involving two of the chromosomes of this male rat led to unbalanced gametes and, therefore, unbalanced zygotes. This work didn't attract a lot of attention, but it was, I believe, the first encounter with a spontaneous chromosomal aberration in mammals.

PRUD'HOMME: Did you devise these projects, or did Professor Cole?

OWEN: Cole had nothing to do with it. In both of these cases, the leader was an advanced graduate student, and I was a newcomer, and we got together and talked about these things and worked together.

PRUD'HOMME: It seems to me that you had great freedom to do what you want.

OWEN: Oh, yes. Cole was very permissive about what you might choose to do and how you chose to do it, even to the point of how you got together the things for doing it. I remember he suggested early that I set up a mouse colony illustrating the various color genes affecting the mouse coat. I dug up information about what kinds of boxes you bred and kept mice in, and went out to the Forest Products Lab to get some advice about what kind of wood to use. Cole was not a managerial type of professor at all. On the other hand, he called things to our attention, like the famous naked pigeons that my grad students have had so much fun with. I've got these absurd pictures of naked pigeons bowing and cooing. Professor Cole had been sent some naked pigeons, and I developed a technique for artificial insemination of small birds, which became generally useful elsewhere as well, and I studied the inheritance of the naked characteristic. Cole was interested in pigmentation in the evolutionary and natural history of mammals, so we got skins of field mice from various museums to compare with color mutations that were known in domestic mice. But mostly he left me pretty much to do what I wanted to do.

PRUD'HOMME: What did you do your thesis on?

OWEN: I worked on developmental aspects of intergeneric hybrids in birds, particularly the guinea fowl and the domestic fowl. I was interested in the developmental reasons that these hybrids were commonly sterile. The thesis wasn't a big deal; it was published in the *Journal of Experimental Zoology*, and would probably not justify a PhD here at Caltech these days, but it was OK for then. And it had a considerable effect, as I look back on it, on my later career research interests, because it was known then—from work primarily by a zoologist named Emil Witschi at Iowa—that the germ cells in birds originate outside the embryo, in a region called the pre-aural crescent. They migrate from the circulation to the region of the presumptive genital ridges, and then become the primordial germ cells of the gonad. Their behavior is different in the ovary than it is in the testes, in a very interesting way. I had tried to find out which stages in that early process led to the later sterility of the hybrids. And because the problem dealt with these migrating cells, when I later encountered the phenomenon of erythrocyte chimerism in bovine twins it was natural for me to think about how that could have happened, because I was already familiar with cell migration in embryonic development.

PRUD'HOMME: Did you teach at this point?

OWEN: As a graduate student, I was a teaching assistant. I taught the genetics lab. I was the one and only TA, and the professor just turned it over to me. So I had the responsibility of developing the laboratory and running the experiments and getting ready for them, and all the interactions with students that came about as a result of that.

PRUD'HOMME: Did you like that?

OWEN: I liked it. It was a lot of work, but there are still people who will say that they started genetics with Ray Owen's genetics lab when he was a graduate student back in Wisconsin. I felt good at it and rewarded by doing it. So from the very start, the teaching experience became an important part of my values.

PRUD'HOMME: Yes, you're obviously a very good teacher.

OWEN: Well, I work on it.

PRUD'HOMME: You explain things well to people.

OWEN: Some people would say that's not the best teaching. I remember Lee DuBridge describing his experience as a grad student at Wisconsin. He said he had two professors, one of them terribly confusing; students just couldn't understand what the professor was talking about. So Lee spent hours after this fellow's lectures, trying to figure them out. The other professor was very well organized and clear in his presentation, and the students understood everything. But they didn't remember much, because it all seemed too easy. So I suspect that sometimes these allegations of who's an effective teacher and who isn't may be a bit superficial.

PRUD'HOMME: What did you do during the Second World War?

OWEN: I was still at Wisconsin. I got my PhD in 1941, and I was involved in dairy cattle work and some other things. I had attempted to volunteer as an aviation physiology officer, but because of a childhood history of asthma, which has never bothered me since, I was regarded as being improper officer-candidate material. At the same time, the draft board considered me perfectly fit for general service. But the draft board concluded—I don't know how valid it was—that some of the work we were doing which related to productivity in agriculture was important enough at the time to exempt us from general military service. So I stayed with the program I had switched to after taking my PhD in 1941. I had joined M. R. Irwin's laboratory of immunology and genetics, working with dairy cattle—quite different from my thesis work with Cole. Then I became an assistant professor of zoology and genetics at Wisconsin. The person who had probably influenced me most was Professor R. A. [Royal Alexander] Brink, a corn geneticist, chairman of the genetics department, a productive scholar and just a wonderful person. Brink had spent a little time at Caltech, which was a mecca for geneticists in those days. He thought the best thing for me to do would be to take a leave as soon as I could after the war and come out to Caltech for a year.

PRUD'HOMME: I want to get into your work with dairy cattle. Could you describe that a little bit?

OWEN. Yes. At the time I joined Irwin's lab, in 1941, after getting my PhD, the program itself was already well established and humming along. It was initiated by Irwin, who had been interested in immunology and genetics for a good many years, and Lloyd Ferguson, a veterinarian who worked in Irwin's group. Irwin had worked on blood-group inheritance in pigeons and doves, in association with L. J. Cole. He was interested in diseases like contagious abortion in dairy cattle. Ferguson and Irwin had also developed support from two purebred dairy cattle associations—the American Guernsey Cattle Club, and the Holstein-Fresian Association of America. The breed associations were interested in the work on inherited blood groups, because it was just at that time that artificial insemination in dairy cattle was coming into common use, and there were lots of problems with parentage—ascertaining paternity in calves born from cows that had been inseminated with semen from two different bulls, and things like that. As the program at Wisconsin developed, it was primarily in a kind of veterinary-legal context much like the paternity-exclusion cases in human medicine. The people at the university were interested in doing research in genetics; the many blood-cell characters, for which specific reagents could be developed—you could test and study inheritance very readily in domestic animals with them—looked like they would be a good basis for genetic studies. I often say that it was a kind of a bio-business venture; it was supported by commercial interests, and we were paid by the sample for doing blood tests. But it made our research possible, because we got blood samples from all over the world—they'd bleed whole herds of cattle, and cow and bull families, and send the samples to us for our tests and studies. And they funded our work. That was long before the time of the NIH [National Institutes of Health] and research grants. We were at an agricultural experiment station, where the setup was pretty minimal by today's standards, especially for basic kinds of research, but the service aspect of our program made the whole thing possible.

PRUD'HOMME: Of course, being in Wisconsin and being in the dairy area, you had a natural environment for this kind of study.

OWEN: Yes, that's right—although, both of these breed associations had their central offices back in the Northeast. It was a national enterprise. The patterns of support have changed since then. After an interval of very extensive federal agency support—the ONR [Office of Naval Research] at first, and the NIH and the AEC [Atomic Energy Commission]—some kinds of biological research are now being supported again by the private sector, somewhat like our old arrangements with the breed associations. But in many ways the atmosphere then was quite different. The object wasn't to get profit for individuals or even institutions; nobody could have thought that we did the work with the idea of being well rewarded financially. It fitted well into what a university was all about—learning.

PRUD'HOMME: It was more pure science, then.

OWEN: Not necessarily. I think there are possibilities for commercial development—in areas like genetic engineering—to use commercial involvement as a way of promoting appropriate scientific and institutional interests. But it has to be done rather carefully, and I don't think the procedures are really well worked out yet.

PRUD'HOMME: Can you describe briefly the discoveries you made, and working with the cattle twins?

OWEN: There were lots of interesting things in the cattle work. A primary one dealt with the recognition of complex antigenic specificities controlled by what appeared to be single genetic units. The leader in this work was Clyde Stormont, a fellow graduate student at Wisconsin, who has been for many years now a professor at the University of California at Davis. I was involved with Clyde in this. Essentially, we had reagents that identified a large number of inherited similarities and differences in dairy cattle—we were testing for about sixty of them. The reagents were assigned symbols, as you do with human blood groups—A, B, C, D, E, F, and on through the alphabet, sometimes with subscripts and superscripts. So there was a great diversity, and it was inherited.

At first, everything seemed simple. A calf wouldn't react with any of the reagents unless either its father or its mother or both did, and the genetic pattern for positive versus negative reaction indicated that each specificity was a Mendelian unit. So the temptation at first was to think that there must be a gene for each of these letters, and that would mean that there were a lot of genes scattered all over the chromosome. But what Stormont primarily observed was that if you looked at how these symbols were transmitted to progeny, you found that they often went in blocks. For instance, there was a bull at Blakeford Farms, in Queenstown, Maryland, for whose genotype we would have to write on one chromosome, $BGKYA^1C^1E^1{}_3I^1O_2$. And on the other chromosome—the one paired with that—was G. When we looked at the progeny, as this bull was bred to a number of different cows, his calves either got the whole block on the one chromosome or they got the G on the other, with no evidence of the complex breaking up. So the question was, What's the nature of the genetic control of these complex symbols? The simple thing was to say that there was a gene for each letter, and that if they went together, they must be linked. This became a very lively subject of controversy, because just about that time, the Rh complex had been discovered in human beings. And in the early 1940s, as we coped with this problem in cattle genetics, there was a great deal of attention paid to the argument. On one side were R. A. Fisher and R. R. Race, in England, who tended to take the simple explanation—that is, there must be a gene for each of these letters, and if they went together they must be linked genes. On the other side of the argument was an American named Al Weiner, who was much less adept at controversy. His point of view was that you couldn't say that there was one-to-one-to-one relationship between the gene and the symbol for something in the antigen and the symbol for the antibodies that reacted with it. It was pretty clear that there were uncertainties about the specificities of serological reactions. We had developed this same concept in our interpretations of the cattle complexes. Stormont and I took the position that you couldn't say how many genes were involved in a complex because part of the symbolic complexity was related to the fact that there were cross-reactive antibody fractions in the highly heterogeneous antisera we were using as reagents, and different combining sites on the same antigen controlled by a single gene. And it's been interesting, over the years, to see how that controversy is being resolved. Only now, with the ability to clone genes and to use monoclonal antibodies rather than heterogeneous immune sera, do we find that there was really truth on both sides—as is true in most bitter polemics, I think. The situation didn't justify the heat that incomplete knowledge invested in it. But the heat stimulated a lot of work.

One of the other things that came out of that early work with cattle was really mine; it dealt with a phenomenon that I found by accident in twin cattle. As I mentioned, we had all these reagents that we could use to test. Clyde Stormont, by the way, did go off to war; he joined the navy, and it was in the interval while he was gone that this discovery was made. He and I had been aware that there was something funny about twin calves. When you looked at brothers and sisters in the same family, they were rarely alike. But when we tested twins, we noticed that ninety percent of the time the twins tested as though they were identical. [Tape ends]

Begin Tape 1, Side 2

OWEN: Of course, the first thing that would occur to you is that they must be identical twins, derived from the same fertilized egg, and should therefore test as identical. But I knew that couldn't be the explanation, because half of them were male/female pairs. The thing that led me to figure out what was going on came out of the cattle blood-testing program. I mentioned Blakeford Farms, at Queenstown, Maryland. The man in charge there wrote me one day saying that he had an interesting pair of twins that had been born a year or so before, and he wondered if I'd like to check them out. The situation was that one of his Guernsey cows had been bred one morning to a purebred Guernsey bull. Later that same day, a Hereford—white-face beef bull—had broken through a fence and bred the cow again. She had given birth to twin calves, one of which clearly was a purebred Guernsey and the other a Hereford-Guernsey hybrid with the dominant white-face marking of the Hereford breed. So it was clear, from the way these calves looked, that although they were twins, they were only half sister and brother—they had different fathers. Of course I said, "Sure, we'd like to test the family." He sent us blood samples from the twins, the two bulls, and the dam—the mother. And when I checked them, the twins tested as though they were identical. Furthermore, the Guernsey bull had antigens S and X_2, which neither the mother nor the other bull had; and both twins also had S and X_2, so they must have inherited them from the Guernsey. But the Hereford bull had R and I^1, which neither the mother nor the Guernsey bull had, and both of the twins had R and I^1, too, so *he* must have been the sire of both of these calves. Some kind of cooperation between the two bulls had determined the blood types of the twins. By developing a technique that later came to be known as differential hemolysis, I was able to separate two red-cell populations in each of these twins. One was

GSX_2—it had developed from an egg that had G from the dam and had been fertilized by an SX_2 sperm from the Guernsey bull. The other red-cell population was RI^1; it had developed from an RI^1 sperm from the Hereford bull fertilizing a non-G egg—the dam was heterozygous for G. So, clearly, there'd been two separate fertilized eggs at the start, as there should have been; and in the course of their prenatal development these twins had come to have an intermixture of bloods—which was OK. It had been known for a long time, from the work of F. R. Lillie at Chicago, that in ninety percent of the cases an anastomosis between the placental blood vessels of bovine twins could lead to a reciprocal transfusion during much of their embryonic lives. The strange thing about it was that, as I said, we tested these twins a year after their birth, and I knew that the life span of bovine red cells is only about 120 days. So what I was seeing was more than just a mixture of red cells, a transfusion. Essentially a transplantation had occurred between these twins, so that blood-forming cells deriving from each twin had settled out in the hematopoietic tissues of the other. After the twins were born, all during their lives afterward, these transplanted cells continued to function—to give rise to cells derived from the co-twin. The twins were chimeras.

PRUD'HOMME: You must have been terribly excited.

OWEN: Well, I remember going home and telling June that this was interesting. It *was* very interesting! You can see how it fitted in with my thesis work on primordial germ cells and sex development: I was prepared to think of cells moving from one embryo to another. And as soon as that answer became evident from studying this unusual pair of twins—half-sib twins—I was able to extend it to some hundreds of other twin combinations and to find that this type of lasting chimerism was a very common phenomenon in twin cattle. It happened in about ninety percent of the cases. The most provocative thing about it was that there must be something special about these natural transplants between embryos; because if you try to establish a transplant between genetically dissimilar adult individuals, the transplants are rejected. So it was either the fact that these were embryonic cells and therefore able to escape immune rejection by their new host, or, much more likely—and I remember developing this idea in a seminar I gave here at Caltech in 1946, after I arrived—that the success of the transplant must be connected with the development of the immune system. In other words, the immune system, as it developed—to put it rather anthropomorphically—learned to accept what was there in the course of its development and continued to tolerate these tissues as though they belonged, all through the individual's subsequent life. These twins were always immunologically completely normal. They resisted infections and developed immune antibody. But because they had been exposed to each other's blood-forming cells as transplants early in their development, they never acquired the ability to distinguish those cells as foreign. That was the beginning of the concept of immunological tolerance, which I think helped to contribute to the understanding of the immune system and to the development of tissue-transplantation immunology.

PRUD'HOMME: Did this get great press for you?

OWEN: No. I sent a paper to *Science* and got it back; the editors said it was too long. So I cut it. It attracted some attention, but there was no big publicity about it. Some people—like [Alfred H.] Sturtevant here at Caltech—heard about it and thought it was very interesting, especially after I came here on a year's leave from the Wisconsin faculty and gave that seminar. I'm sure that it led to their deciding they'd like to ask me to stay.

But the immunological tolerance thing did come to a lot of public attention when the Nobel Prize was given for it in 1960; and that was given, very appropriately, to Peter Medawar, who had put this system on an experimental basis in the laboratory, and F. Macfarlane Burnet, who would essentially publicize it. And if you'd like, I'll talk about the Nobel Prize situation sometime.

PRUD'HOMME: We can do that later on.

OWEN: Sure.

PRUD'HOMME: You came to Caltech on a leave of absence from the University of Wisconsin, as a Gosney Fellow. Tell me about the Gosney Foundation.

OWEN: First of all, it was R. A. Brink who suggested that I see if I could arrange to come to Caltech for a year. The Division of Biology here had just recently been given a legacy from Mr. [Ezra] Gosney. Mr. Gosney's professional, commercial life was centered in Arizona. And he had become interested, as a lay person, in human betterment at a time when people were thinking about eugenics and things like that. R. A. Millikan, I guess probably, and Thomas Hunt Morgan were perfectly willing to accept gifts to the institute which were identified with objectives like that, even though they might not particularly agree with the social or ethical aspects of them, and as long as the conditions of the gift were broad enough so that the money could be used to support things they *were* interested in. When I was appointed a Gosney Fellow, that program had just started. Herschel Roman—who recently retired as chairman of the Department of Genetics at the University of Washington in Seattle—and Werner Maas and I were the first three Gosney Fellows, and sometimes we joked about who was really first. I think I was the first Gosney Fellow to be appointed, but Hersch came here the summer before I arrived, as a corn geneticist; he worked in [E. G.] Anderson's cornfield down in Arcadia. So he was the first Gosney Fellow in residence. When I came, the conditions of the Gosney Fund had only very recently been set up. And Lois Gosney Castle [later, Lois Castle Troendle—ed.], who was Mr. Gosney's daughter, was here putting this legacy in order, and she worked very, very hard at making it into a genuine asset, with investments—guiding and setting it up. In fact, Lois was for many years an important figure in biology at Caltech, because she knew prospective donors who enthusiastically promoted our interests.

PRUD'HOMME: So she broadened the base.

OWEN: That's right. And she worked very hard. Now, as the first Gosney Fellow, I was put by Beadle, who had just arrived too, into rather close interaction with Lois. And together we worked out a little brochure that described Mr. Gosney and the history of this fund, with a picture of him on the cover, to advertise the availability of these Gosney Fellowships. And I came to know her very well. She's still alive, by the way. She's over in the hills above Pasadena. But she doesn't get out much anymore; she had an automobile accident.

PRUD'HOMME: What was your research topic when you came here?

OWEN: At the time, of course, I was interested in the tolerance-related thing. I had thought that it would be nice while I was here for a year as a Gosney Fellow to do some experimental work related to what happened in twin cattle. This was in 1946, well before Medawar and other people had picked that up. So I thought rats would be good material to work with. I got some inbred lines of rats and had them shipped here, developed reagents for identifying intermixtures, and set up parabiosis of the rats so that they exchanged blood, as the twin calves had done. But the earliest point at which I could do parabiosis successfully, surgically, was already too late, because rejection set in. However, this brought about the recognition of a set of blood groups in the rat, and was of value to me later, as I can describe shortly. So that first year, I was down here in the basement [in Kerckhoff], in this very office; there wasn't any partition here then. It had been the stockroom for the department, but when [George W.] Beadle came he worked hard at making a real stockroom upstairs. Across the hall was my animal room, where I kept rabbits and rats and mice. I was the only person in biology interested in these immunologically related matters.

PRUD'HOMME: Did you come to Caltech thinking you might stay?

OWEN: No. I had intended—and there was no other prospect—to return to Wisconsin. Beadle had just arrived and was beginning to build up the faculty. He brought [Norman] Horowitz and [Herschel K.] Mitchell down with him from Stanford. Sturtevant was here, too. You know, after Morgan's death the department had been run by a committee, essentially, and not very effectively. The department was in severe need of focused leadership, and they'd chosen to offer the job to Beadle; he accepted it—he was up at Stanford at the time. And he came here at the same time I did—in September, 1946. Lee DuBridge arrived at that time, too. It was a very good year.

These other people attracted a bit more attention than I did when I arrived. June and I had driven from Wisconsin pulling a house trailer, because we knew that housing was short down here. But we expected to sell the trailer here by the end of the year and go back to Wisconsin. Then in the course of the year they decided they would like me to stay on the faculty here. But Wisconsin wanted me to come back. So we went through a rather difficult period toward the end of the year deciding, because we liked and appreciated both places. Outside of the fact that obviously Caltech was a great place to be in many ways, I guess the compelling reason for staying here was that I was much more comfortable being on my own, as an independent member of the community—whereas back at Wisconsin I was among the people who had been my professors. And Caltech seemed better from every point of view, really—institutionally as well as personally.

PRUD'HOMME: You were also in a department which, as you mentioned, was just beginning to establish itself.

OWEN: It was a very lively time, and the *Neurospora* work was new, biochemical genetics was hot. Caltech already had very lively postdocs in biology. It was a very stimulating environment, changing all the time—good senior people who were a pleasure to know, like [Alfred H.] Sturtevant and Sterling Emerson.

PRUD'HOMME: Can you describe some of them to me?

OWEN: Yes. I guess my first close interactions were with Sterling Emerson. By the way, he and Mary still live in Altadena. Sterling was a very, very quiet person. I would guess that Sterling was particularly attracted by my Gosney application because he and Sturtevant had done some speculating about the possibility of achieving directed gene mutation by means of antibodies. As I look back on it, the notion was pretty naïve. But at that time people talked about genes replicating by a kind of a lock-and-key thing—the same idea that was developed by [Linus] Pauling when he explained antibody specificity. It was thought that the gene probably produced its product by serving as a template for the product. And the idea was that if you produced an antibody that reacted with that product, maybe that antibody would have some kind of effect on the gene, too. So Emerson and Sturtevant were interested in the possibility of using antibodies as agents for inducing directed mutation. And in fact Sterling had done some work in *Neurospora* that suggested such a thing might happen. I don't think the experiment was well controlled, and it turned out, when controls were run, that normal serum rather than immune serum had some substance in it that somehow increased the frequency of mutation in some *Neurospora* cultures. So the whole idea was a speculation that didn't work out. But there wasn't anybody here in biology who was very experienced in the preparation and use of antisera. Sterling taught a course in immunology, which was his way of learning about the subject. And here I was—I'd worked on blood groups and genetics and things like that, and I knew how to inject rabbits, and so on. And they were also interested in the twin cattle story. I think it was all of that that led to their interest in having me here—and, also, that led to my feeling right away that I was occupying a useful and respectable niche in this society. There hadn't been very much good work with mammals here, you see. The neurobiologists, especially [Anthonie] van Harreveld, worked with some neurobiological aspects of mammals, and [Henry] Borsook in biochemistry, but Sturtevant's principle—which he used to give with tongue-in-cheek, I think—was that the mammals were an evolutionary mistake anyway: the interesting animals were flies. So it was good to have somebody here who actually wanted to work with warm-blooded animals, as I did, and still was a congenial human being and interested in antibodies and things like that. And of course Linus Pauling and his people, including Dan Campbell in chemistry, were fellow-spirits.

PRUD'HOMME: Caltech was known as specializing in genetics, wasn't it?

OWEN: Yes, that's right, but not in mammalian genetics. Mammals were regarded—part of the reason for Sturt's attitude—as impossibly clumsy material.

PRUD'HOMME: Not tidy.

OWEN: That's right. You couldn't get out a generation in a couple of weeks, like you could with flies. And of course, the phage work had started here, and you could get a generation of phage in twenty minutes. But think of working with something like a cow!

PRUD'HOMME: How did Caltech rank in the United States? What was the biology division's reputation in the U.S.?

OWEN: It was, as I said earlier, a mecca for geneticists, specifically, and for people who recognized genetics as the most compelling basic discipline in biology. It was world-famous.

PRUD'HOMME: Because of Thomas Hunt Morgan?

OWEN: Yes, and the people that he brought with him when he came. Morgan, of course, was very famous. And during the thirties, after Morgan started the department here, it became a place where people came from all over the world who were interested in genetics. Beadle came here as a postdoctoral fellow after completing his PhD in corn genetics at Cornell. [Boris] Ephrussi came here from Paris and started working on interpreting gene action in *Drosophila* in biochemical terms.

PRUD'HOMME: Was Morgan a difficult man?

OWEN: I didn't know Morgan—he died just before I came. I came in 1946, so all I know of Morgan is secondhand.

PRUD'HOMME: What was his reputation?

OWEN: I saw him in some part through the eyes of the Sturtevants, and they called him "Boss." He was idealized. By the end of his life he had left fly genetics and was working on *Ciona* and things like that, going back to an earlier interest he had had in invertebrate zoology in more general terms. I did know Mrs. Morgan, who was here and active when I arrived, and she was just a wonderfully good person. She was a scholar in her own right; she knew fly genetics and had made important contributions to it. They lived in a Greene and Greene house across the street, which unfortunately was destroyed by campus expansion. The street's no longer even there now. But in those days that was the Morgan house. And I remember, soon after my arrival here, Frits Went got the so-called phytotron built, and it wasn't far from the Morgan house. I remember Mrs. Morgan telling me one day that they had these electrostatic screens on the greenhouse [i.e. the phytotron], so that if an insect alit on them it was incinerated; and she would lie awake at night hearing this crackling and thinking of all these lovely insects being burned next door. [Laughter] She was a wonderfully sweet, able person. But I didn't know Morgan.

PRUD'HOMME: What were the differences you observed between Caltech and the University of Wisconsin? Were the students different?

OWEN: I came to know the students soon after I took up my appointment here, because I started to teach the immunology course with Sterling, and then taught it by myself after that first year; and I taught Biology 1, general biology. The students at Caltech, then as now, were a very select group and very bright. When I taught genetics in one quarter I could teach much more rapidly and get farther than I was able to do at Wisconsin in a semester. The Wisconsin students were often just as bright as the Caltech students, but it was a much more diverse environment, and the Wisconsin students were much more diverse in their interests. At Wisconsin I had had a whole mix of people, from all kinds of different areas of interest, in my classes. So you gained in one way from the very select nature of the Caltech students, but to some degree at the expense of diversity.

PRUD'HOMME: Socially, was there a different feeling? Did you find that you had social obligations here, at a smaller institution?

OWEN: At the start, when we arrived with our house trailer, we thought it was a pretty unfriendly place. Caltech was used to having people coming in and out, as I said, and we weren't any big deal. We pulled this little trailer with us and couldn't find a place to park it. Although the Emersons had offered to let us park it in their backyard, that didn't seem feasible to us. We ended up parking it down Rosemead Boulevard, miles away from the campus. Our little boy David was a year-and-a-half old then; and June stayed there with him, and it would start to rain, and I would have the car since I had to come up here every day. And nobody paid any attention to her, and she wished we were back in Wisconsin. [Laughter] Mrs. Sturtevant was kind in this situation. But, again, it still wasn't quite as thoughtful an environment as we had come to feel the Department of Genetics was at Wisconsin. The biology community here was very close-knit, and traditionally, I think, over the years the biologists as a whole have tended not to participate extensively in the life of the institute.

PRUD'HOMME: I wonder why not?

OWEN: Well, biology is a very consuming interest. People talk to each other, and there's sort of a predominant feeling that they don't have much time for mixing or much interest in mixing. My own inclinations were somewhat more general. For instance, soon after I joined the faculty, I became a member of the Freshman Admissions Committee, which got me interested in the students and where they came from and what they did, and got me mixing with the people from other departments who were on that committee. I got out and represented Caltech at various high schools, and visited schools around the country, and things like that. Except for Charles Hamilton now, who's not really a member of our professorial faculty, I guess I'm the only biology professor who has ever served on the admissions committee. And similarly, biologists would go over to the Athenaeum sometimes and have lunch, but always together. Whereas my inclination was to sit down at a faculty table and not worry too much if I had to sit next to an engineer or a chemist, or something like that. I guess it was just a matter of how you felt about mixing with people, and maybe part of it was that June was interested in economics and we therefore made friends in the humanities and social sciences. So, in a way, I guess I'm still a little bit of a maverick in that regard. But it was getting into things at the institutional level that I found very, very rewarding.

RAY DAVID OWEN

SESSION 2

October 25, 1983

Begin Tape 2, Side 1

PRUD'HOMME: When you came here to Caltech from Wisconsin, who were your colleagues? Which of the people you worked with impressed you? Also, can you tell us something about some of the people you might not have worked so closely with—for example, Fritz Zwicky. Did you work with Max Delbrück, James Bonner?

OWEN: All right. I arrived, of course, as a Gosney Fellow on leave from Wisconsin. And I have an unbroken collection of the institute's bulletins over the years, which are very interesting historical documents. They remind you of things, or correct things you may have misremembered. At the time I arrived, Beadle had just taken up the chairmanship of the department; he arrived that summer, brought down with him Horowitz and Mitchell from Stanford. And there was a postdoc he brought down from Stanford—Adrian Srb—with who I became quite closely associated; I'll refer to that later because we wrote a genetics text together. There were only six professors in the biology division listed in the bulletin for 1946-47, with Beadle as chairman. Perhaps I should first talk about Beadle himself. He, of course, had already achieved a great reputation, as a result mainly of the work that he and [Edward] Tatum had done in identifying the primary functions of genes as controlling the specificity of enzymes that catalyzed biochemical reactions occurring in cells and in the organism. Beets had a history at Caltech, because he had come here as a postdoc after taking his PhD at Cornell; he worked in corn genetics and cytogenetics. Barbara McClintock had been one of his colleagues back at Cornell in the College of Agriculture. And when Beadle came here for postdoctoral work, he took up with [Boris] Ephrussi, who was here from Paris that year, and they worked on transplantation of embryonic organ primordia in *Drosophila* larvae.

PRUD'HOMME: What was Beadle like?

OWEN: Well, Beets was a very lively person. He was here all the time it seemed, day and night.

And he liked to do things directly, himself. For instance, this office, as I mentioned, had been the biology stockroom at the time when the department had only Kerckhoff. Beadle undertook to make a good central stockroom upstairs, and he built the shelves for it and fabricated his own wooden reprint boxes, as you've seen lining the walls of my office. He liked to work in the carpenter shop. He had had a reputation for being a terrible public speaker and teacher. In fact, back at Harvard, I believe, he was voted the worst teacher on campus in a poll conducted by the *Harvard Crimson*.

PRUD'HOMME: Strange that he was the head of the department.

OWEN: Well, he undertook to improve himself in that respect. He'd take any speaking engagement that was offered him. He talked to the Rotary Club and the Kiwanis Club and the Women's Club, and gave a great deal of attention to his preparation, preparing visuals with slides that he watercolored himself. And he made himself into an excellent speaker by working very hard at it because he regarded that as being important.

PRUD'HOMME: Did he like the teaching side of his work?

OWEN: Yes. I would say he was probably never one of the world's most popular teachers, but he did like to teach and thought it was important, so that even in that very busy time, when he was rebuilding the biology division, he participated in teaching a course in advanced genetics, according to the catalogue—and later he taught general biology, Biology 1. He liked to interact with young people, especially at the graduate and postdoctoral levels.

PRUD'HOMME: Was he popular with the postdocs and graduate students?

OWEN: Yes. It was a relatively small department, and we did all kinds of things together. Beadle was in close touch with Linus Pauling, who was chairman of chemistry. And between the two of them, they developed a program in chemical biology—and I'll have a little bit to say later about that. One of the professors here with whom I interacted most was Sterling Emerson, whom I mentioned last time. Sterling was a geneticist, interested in *Neurospora* and all kinds of other things. He went out into the Oregon mountains and studied a species of *Oenothera*. There were only a couple of hundred members of the species, which nevertheless maintained a high degree of heterozygosity for a very complicated series of allelic genes. Population geneticists would say that with populations that size you couldn't maintain that level of heterozygosity, just because of the inbreeding effect alone. Sterling was interested in that problem, and he collaborated some with Sewall Wright, who was then at the University of Chicago, in trying to understand the unusual plant population genetic situation. Although Sterling has by now essentially lost his eyesight, he still knows his backyard very well, and he keeps his collections of *Oenothera*, and an eastern dogwood he and his wife Mary planted there. A wonderful person, Sterling—very shy, very quiet, very unassertive, and not at all a good teacher, but the kind of person everyone would like to go to with a first draft of a manuscript on whatever subject. Give it to Sterling first, to look over. He always took this kind of thing very seriously, always gave the manuscript the most careful and insightful attention. So he was the sort of colleague who is so very, very valuable. Often such people don't achieve status in the outside world—though of course Sterling was well known and was elected to the National Academy of Sciences. Sterling and Sturtevant, as I mentioned last time, had an interest in the possibility that antibody could induce specific gene mutation, so it was through my interest in immunology that I was in close interaction with Sterling.

Another person I worked with was Sturtevant. Sturtevant, of course, had come here with Morgan and was one of the original four horsemen of genetics, a *Drosophila* geneticist. Sturt was always a very modest worker, in the sense that he never built up a big research enterprise. He was classifying flies, and interested in problems of evolution from the viewpoint of the genetics of *Drosophila*. He, too, was an excellent colleague to bat things around with. Sturt followed the literature very closely, and he was always bringing into the conversation an interesting article—on any subject, essentially—that he'd run across in the journals that had come into the library that day. Sturtevant and Beadle had together published the standard textbook of genetics at the time [*An Introduction to Genetics* (Philadelphia: Saunders, 1939)]. And, in fact, they were beginning to talk about a revision, but their revision floundered because of an argument between them—not a personal argument, but nevertheless one in which each held to their own view.

PRUD'HOMME: What was the argument?

OWEN: The argument was whether you referred to the alternative forms of a gene as genes, or alleles. And in retrospect this seems like a terribly unimportant kind of argument, but somehow they never got to agree as to how they would define an allele. And, of course, that was a time when defining alleles was a fashionable thing; there were questions about complex multiple allelic series, where the limits of the gene really were. There was no biochemical evidence, so everything had to be deduced from genetic experiments. And in fact Eddie [Edward B.] Lewis, who was a member of our faculty and was at that time an instructor—he'd gotten his PhD with Sturtevant—Lewis was working on pseudo-allelism and position-effect in *Drosophila*. And I had an interest in the complex phenogroups of cattle and human blood, for example. So there was a common element in our interests at that time in what was a common subject of puzzlement for geneticists: how to explain these genetic associations, and where the limits of the gene were, and how you could define a gene in that sense.

PRUD'HOMME: You've talked about Beadle developing the biology division. What was it composed of when you came? Who were the four horsemen of genetics?

OWEN: Well, that refers to the Columbia group who worked with Morgan in the fly room—Sturtevant, Morgan, [Calvin] Bridges, and [H. J.] Muller—with Morgan as the head of the laboratory and three young people as very enthusiastic coworkers. Sturtevant has described the sociology of that group in a chapter in his book on the history of genetics—a chapter called "The Fly Room"—and I think if you're interested in the history you might enjoy reading it. It was a group of people who were very, very intimately associated with one another, in a field of science that, as a result of the availability of very fortunate material in *Drosophila,* was moving very fast. Every day, essentially, brought a discovery of some kind. And these four people had their own personalities and interacted with one another in different ways. Sturtevant—everyone called him Sturt—always referred to Morgan as "the Boss," and one got the impression that he was closest to Morgan, but Bridges also came when Morgan came here. Muller, who later became a very distinguished geneticist, always felt that he was just a step away from the innermost core of that intimate group.

The other professors on the faculty at the time I came here were Henry Borsook, who taught biochemistry and was interested in the early development of the use of tracer techniques for tagging molecules in biochemical reactions—a widely scholarly person. Arie Haagen-Smit, who was a wonderful associate, one of a group of Dutchmen that Morgan brought to Caltech when he started the department. Outside of the group that Morgan brought with him from Columbia—the fly people—he recruited several people from the Netherlands: two in the field of neurobiology—[Cornelis A. G.] Wiersma and [Anthonie] van Harreveld—and two or three plant physiologists—Frits Went and Haagen-Smit.

We just had Kerckhoff at the time, but there was quite a bit of vacant space around; Beadle hadn't begun to fill it up yet. I was given a choice of rooms, and chose this basement place because I could use the room across the hall for my animals, and Beadle had just moved the stockroom out of here. Down the hall in the cold room was where Haagen-Smit processed the flavoring principles of California wines; he specialized in categories of biorganic substances that gave aroma and taste to things. There was a lot of the smell of wine around the place. It was actually that interest of his, I think, that led to his becoming interested very early in what smog was. Also, he was interested in plants, and in close association particularly with Frits Went. Just about the time that I arrived, smog began to be evident. In 1946, I remember, we used to play tennis at noon—[Herschel] Mitchell and I and a couple of other people. And we began to notice that Pasadena was a hard place to breathe around noon. Although, since we hadn't known Pasadena in its earlier, very clear days, the smog didn't seem to be a great problem. But Frits Went had observed lesions on the leaves of a variety of plants, and he concluded that the smog was damaging plants. So Haagen-Smit and Went worked out an assay for the active principles in the smog by using a rather large number of plant species that suffered different kinds of damage from smog, and then used this spectrum of reactions to check on fractions of air. Later, Haagen-Smit produced synthetic smog. I remember Haagi's lecture in 119 Kerckhoff, on the origins of Southern California smog, when he produced some smog on that demonstration bench in the front of the room and let it float out through the audience, so that even in the back two or three rows, where I was sitting, it got to you. Your eyes smarted and you smelled it. He actually worked out the whole sequence of interactions in those years. These were the beginnings of the origin of smog, with the development of the catalytic cracking procedure and a refinement of petroleum products that led to the formation of hydrocarbon molecules with unsaturated ends where they'd been cracked. The reactions occurred in the atmosphere because of ultraviolet light from our famous sunshine, producing organic peroxides which Haggi was able to show were the active components of smog.

PRUD'HOMME: Did he get much press at that point?

OWEN: Oh, yes. And Haagi had a very strong social concern, so that—even though I think he undertook it with some reluctance—he ended up serving on the governor's air-pollution-control panels, which developed much of the political machinery to try and improve the problem.

Haagen-Smit died a few years back, but his widow still lives in Pasadena, not far from where we live, and we see her frequently. She's become a great bird watcher. When Haagen-Smit died, gifts were made to establish a scholarship for a Caltech undergraduate, and each year we pick an undergraduate major in either biology or chemistry to receive the Haagen-Smit award.

PRUD'HOMME: Was there any jealousy of Beadle when he was made the head of the division? How do you think he was picked?

OWEN: Well, of course I wasn't here when this decision was made, but it seemed to have been a logical one. First of all, he had Caltech connections in his background. As I mentioned, he'd been here as a very effective postdoctoral fellow, working with Boris Ephrussi. The work on *Neurospora*, which he and Tatum had undertaken, had clearly become by then one of the most productive and revealing aspects of genetics, because it brought into the laboratory the possibility of finding out what genes did, and perhaps eventually what they were. And I think, from what I heard indirectly, that the situation in the department was regarded as somewhat desperate. Morgan had died. There was a committee that attempted to govern the department, but no one on the committee was terribly interested in governing and there was no central responsibility. Some people had the idea that things were falling apart. Beadle was clearly a person who could exert leadership in rebuilding it, and that's exactly what he did.

About the professors who were here at the time—I referred briefly to Frits Went, the plant physiologist. Frits was a Darwinian-type of character—wide natural-history inclinations, and a great plant physiologist as well. Among the fondest memories June and I have of that first year was the trip—which I believe was an annual thing—to Death Valley, led by Frits Went, who introduced us to the valley plants. We came to know and love the California deserts in substantial part because of Frits's very enthusiastic tutelage. Frits was a great teacher. He taught botany and plant physiology, and enlisted the enthusiastic interest of his students.

There were six professors in the department then. Before that time—especially under Millikan—there hadn't been a lot of attention paid to promoting people or to improving their salaries or anything like that. It was sort of a random matter whether somebody got promoted or not. I remember Albert Tyler—Tyler had been Caltech's first PhD in biology. He came here as a graduate student with the Morgan group. He got his PhD here, stayed on our faculty, and still, I think, until Beadle came, he was just an instructor, because nobody had got around to thinking it was time he moved up the ladder.

PRUD'HOMME: You had some women in your department as research associates, I believe, which was quite unusual for Caltech at that time.

OWEN: There's no one listed in the 1946-47 catalogue in that category—all of those names are males. Gertrud Oppenheimer was classified as a research assistant, but that meant something different then. There were, at the time, only ten research fellows—those are postdocs—and then below that was the category of research assistants, which included two men and Gerty Oppenheimer. I remember Gertrud Oppenheimer very well—another very quiet person, with a wire-haired terrier, I think it was, who was her constant companion. And, of course, when Mrs. Morgan was here she was recognized with an academic title. The women's washroom on the second floor, which was adjacent to Beadle's office, was generally regarded as the province of Beadle's secretary during the day, but anybody could use it at night, because there were no women around then. So there weren't women at the time in a conspicuous way.

PRUD'HOMME: Some people have said that Morgan—and this was carried on by Beadle—that Morgan was one of the first people to introduce interdisciplinary studies, such as chemical biology, biochemistry, biophysics, and so on. Is this true?

OWEN: Well, I guess it would be true, in general. Most biologists up to that time had been engaged in the descriptive aspects of biology, and chemistry was regarded as a completely different field. I remember, for example, when I studied biology at Carroll College in the thirties, we studied zoology and learned about the kinds of animals; and we studied botany and learned about all the kinds of plants. And we studied the structure of the vertebrates in comparative anatomy. Physiologists were interested in what went on in cells and tried to interpret them in chemical terms, but there was a rigidity to disciplinary designations which led to less interaction between what might have been regarded as core biology and other disciplines.

Morgan had been active in the National Academy of Sciences before he came here from Columbia. It seemed like an outlandish thing to do in those days, to come out here to the West to a place that did not have biology before. But I think one of the reasons Morgan chose to come to Caltech, and I've read some of the correspondence, was that he felt he'd like to have a free hand to develop a division of biology in an environment where chemistry and physics and mathematics were strong—where he could escape some of the traditional limitations of biology that were still imposed elsewhere. And he certainly managed to do that.

PRUD'HOMME: Did this vision come from Millikan, or did it come from Morgan himself?

OWEN: Well, the institute had developed with physics and chemistry and mathematics, as you know, in the very early 1920s. It wasn't until 1928 that they decided to start biology here, and their choice of Morgan, I'm sure, was connected with, first of all, his great distinction. They knew him in the National Academy of Sciences, regarded him as a great man. But also, he relished the idea of coming to a place like Caltech, where his interest—or at least, his ambitions for the division—would fit in with the basic physical sciences. He seems to have had a very strong respect for explaining things in terms of chemistry. He was an experimental embryologist before he worked with flies. When he had a free hand, after he more-or-less let flies go to Sturtevant, he went back to looking at the embryological development of a strange little marine organism called *Ciona*. So while he was not himself inclined or even competent to talk about things in chemical terms, he had a great deal of respect for people who could. James Bonner has said that Morgan was sure that most kinds of biology could be done with milk bottles and corks and bananas, and it was hard to get money out of Morgan for anything like advanced instrumentation. But he believed that biochemists needed good instruments, so he would authorize money readily for a spectrophotometer or a pH meter—I guess not those, in those days, but comparable kinds of equipment. The way to get something like that if you were a biologist was to get one of the plant physiologists or the biochemists to ask for it for you.

PRUD'HOMME: Was Morgan a saint? Some people say he was very anti-Semitic.

OWEN: Again, I didn't know Morgan. But I think he lived and worked in a time when there was an almost unconscious tendency to set Jews apart. A couple of years ago, when Norm Horowitz retired and I was giving a faculty talk about Norm's career here, I heard about how when Norm arrived here as a new graduate student from Pittsburgh—in 1936, I guess it was—he walked into Morgan's office and Morgan told him that he would be working with Albert Tyler. And, as Norm said, Morgan was a great man and Norm was a new graduate student, and in those days students didn't question such things. So he readily took the assignment to Tyler. But one of the reasons for it may have been that Norm was Jewish and Tyler was Jewish. So I suspect there was something—but I don't think from what I've heard, it would be fair at all to say that Morgan judged people in terms of their ethnic background, or modified his opinion of their merit or their opportunities because of it.

Begin Tape 2, Side 2

OWEN: There was another episode that might also have led to the conclusion that Morgan was conscious of whether a person was Jewish. There was a possibility of recruiting to our faculty a distinguished Jewish person from the East, and Morgan, I believe, wrote a letter indicating that the candidate would be comfortable here because there was a Jewish community with which he could identify, or perhaps it was that Morgan felt the man *wouldn't* be comfortable here because there was no such community—I don't remember which. Without having gone into it in any depth, I have a feeling that that sort of thing was not uncommon in those days; people fell into it just as they did with regard to blacks and women, let's say. Morgan probably was actually concerned that—in his experience and in those times—Jewish people didn't feel comfortable relating to people unlike themselves. I don't know. But nothing I've run across would indicate, as I said, that Morgan was anti-Semitic in the sense that he was hostile to Jewish people or would modify his opinion of their individual merit or their opportunities as a result of that.

You might be interested—as an aside, since you've asked this question about possible Anti-semitism—in one of the things I found most shocking when I came to Caltech and joined the faculty here, and I don't think it's been talked about by anybody. I became an associate professor in the 1947-48 academic year and immediately became a member of our Committee on Freshman Admissions. At that time, there was a convention in the admissions committee that I learned about only indirectly. It was called the "green check." When the committee got together to talk about the candidates for freshman admission we'd interviewed, someone—I think it was Harvey Eagleson—referred to one of them as a "green check." When I inquired into what that meant, it turned out that that person, that candidate for admission, was thought to be Jewish.

PRUD'HOMME: After the Second World War?

OWEN: Yes, this was in 1947. There was no indication that there was a quota, or that a candidate for admission with a green check would not be admitted for that reason. But it was apparently something that past committees had put down as an item of information. And I got the feeling that if the class was getting pretty well filled up and there were two candidates for a vacancy and one was a green check that might very well have resulted in the other being chosen. My wife remembers, much more clearly than I, my reaction to this. She seems to recall my concern, which never developed into bitter argument or confrontation, but just was surprise and a feeling that "Well, gosh, I saw a lot of candidates and it never occurred to me whether one was Jewish or not." She thinks my concern about this was part of the reason that the convention rapidly disappeared. All I remember is that only in the very early stages of my experience on the admissions committee was there any reference to a green check. It was a very short period of time before the thing disappeared entirely. But I mention it to you because, again, it's an example of how people felt and talked back in those days. It's easy now to feel terribly superior to people who talked about niggers and Jews, but there was a time when it was so conventional as to almost escape notice.

PRUD'HOMME: I think the reason I was appalled—also because I *am* of this time—was because so many eminent Jewish scientists fled to this country just before the war or were brought out during the war, and you would think that an intellectual institution such as Caltech would go overboard in the opposite direction.

OWEN: Yes, and in fact that right here on our own faculty we had by then a number of Jewish people whom we were extremely proud of; nobody ever referred to them as Jews or anything like that. But that there should be, when you looked at high school students, this kind of categorization repelled me.

PRUD'HOMME: In 1952, you published with Adrian Srb the book *General Genetics*, which is a text for beginning students. Was there a need for this material? And did you notice an increased interest in genetics by students?

OWEN: Yes. Adrian was a postdoc here and I was a Gosney Fellow, when we got to know each other. And at that time there was no textbook of genetics that embraced the most important and interesting developments coming out of biochemical genetics—the *Neurospora* work and so on. Certainly, Beadle and Sturtevant had been an excellent genetics text, but it was written in 1939 and it hadn't been revised. And so Adrian was asked by Bill Freeman, who was just starting the W. H. Freeman Company, if he would consider writing a genetics text. Adrian was known to be a very good writer; he majored in English as an undergraduate; he was an excellent teacher. Unbeknownst to me, he said, "Well, I wouldn't want to take up such a job myself, but if I could get Ray Owen to collaborate with me, I might do it." I was approached independently then—Beadle and Pauling had strong interactions with Bill Freeman at the time—and asked if I would write the genetics book. And without knowing what Adrian had said, I said, "Well, I wouldn't want to take this on myself, but if I could get Srb to join me, well, fine." So, together we worked on it, and in 1952 published *General Genetics*, which rapidly came into common use, and over the years was, I'm sure, the dominant textbook in the field. We didn't get around to revising it until 1965. Let's see, the first edition was published the year before the structure of DNA was worked out, so by '65, it was out of date, pretty much. We took on Bob [Robert E.] Edgar as a third author and revised it, and I'm currently supposed to be working on a second revision, with two new coauthors.

PRUD'HOMME: Were students more interested in genetics as a result of the researches going on here?

OWEN: Yes, in general. Genetics had been an attractive area for good students for many years, beginning with the rediscovery of Mendelism and the developments using *Drosophila* and corn, primarily, and identifying the genes and their arrangement on chromosomes.

PRUD'HOMME: It's a very precise study.

OWEN: That's right—it's quantitative; they needed to use mathematics in it. And there were clear questions about what these particles called genes were, and how they could affect the developing organism as they did, and be transmitted from one generation to the next as they were. Genetics had long been particularly attractive to young people with enthusiasms for these kinds of things. It was a very lively time in genetics, those late 1940s and early 1950s. It was a good time to get out a textbook. Then, of course, the discovery of the structure of DNA in 1953 opened up another area of enthusiasm quite rapidly. I've always felt that genetics is central to almost all biological science.

PRUD'HOMME: Do you want to talk a bit about Pauling?

OWEN: Yes. I came to know Pauling quite early—partly because of his interest in the origin of antibody and our common interest in aspects of immunology, and also because he was a very enthusiastic and interactive person. I was particularly close friends with John Singer and Harvey Itano, who worked with Pauling on sickle-cell material. And then in later years, I came to know Linus in other connections as well.

PRUD'HOMME: What connections?

OWEN: Well, we would occasionally both be speakers at some event. I remember one experience: Linus and I were the speakers at a Kaiser or some such symposium, up in San Francisco, in the evening; this must have been in the late 1950s. Linus was the first speaker, and he was talking about sickle-cell hemoglobin and gave his usual very eloquent talk, so very smooth and spontaneous, and so very effective. And then there was an intermission before I was to speak. We were out in the lobby, and Mrs. Pauling was there, Ava Helen. And I said to Linus, "Well, Linus, that was your usual eloquent talk. It's a hard act to follow." And Ava Helen said, "Yes, but he left out a paragraph." That was my first indication that when Linus gave one of these eloquent, spontaneous, extemporaneous lectures, he had actually prepared in great detail, and in fact had gone over it with his wife beforehand. He had the ability to remember word-for-word. He told me once that he prepared his lectures in Chem 1 by writing them out, then the next morning he could give the lecture word-for-word without referring to it. He used to do little tricks as a teacher—he was a *very* effective teacher, as you can imagine.

PRUD'HOMME: What kind?

OWEN: Well, for example, he would be talking about a particular crystal structure and he would reach in his pocket and supposedly just by accident he would happen to have an example in his pocket. Everyone knew that he'd put it there, but it was always a fresh discovery. I remember when he gave the seminar reporting the alpha helix structure of protein, in 119 Kerckhoff. He had a model with him, shrouded in wrapping paper and scotch tape or something, or with a string around it, and he started to talk about the studies of the protein structure and X-ray diffraction and so on. He had a little jackknife in his hand, and he would take a few steps toward this shrouded object, and we were all on the edge of our seats, waiting to see this model. And then he would think of something else and go back to the blackboard and write something down. So it wasn't until the very end that he cut the string and we finally saw the alpha helix structure of the protein! He was a great master of suspense—and a very, very congenial colleague.

PRUD'HOMME: You took a leave of absence in 1956 for a year, and you went to work at Oak Ridge. What did you do there?

OWEN: Well, I'll give you a little background on how this happened. I've mentioned the twin cattle chimeras, and I think I mentioned that some attempts were made here to repeat the twin cattle situation by establishing transplants in rats. In thinking of the immunological implications, I had developed the idea that perhaps you could make successful tissue transplants into immunologically mature individuals—like adult rats or mice, or even people—if you damaged their immune system and then established the transplant, so that it would be present as the immune system was re-formed. One way to damage the immune system was to use high levels of ionizing radiation. So I had written up a grant request to the Atomic Energy Commission, describing some experiments that I wanted to do. I'd chosen chickens as my experimental material. I would X-ray them and then inject them with blood-forming tissue from other chickens, to see whether we could establish transplants and even affect the radiation damage itself by that procedure. At the time, there was a good deal of concern about radiation effects. And then one day I got a letter from Dan Lindsley at the Oak Ridge National Laboratory. Dan had been a graduate student here; he took his PhD with Sturtevant. He was a *Drosophila* geneticist primarily. But at Oak Ridge—where Alexander Hollander had built up a productive and distinguished biology division, concerned in substantial part with radiation effects, mutation, things like that, a good group of geneticists—Dan wrote that they'd had a visiting speaker who had said that you could treat mice that had been given heavy doses of X-ray with bone marrow. Animals that had received potentially lethal doses of X-ray would sometimes recover after such treatment. The speaker thought some chemical in the bone marrow might aid in recovery or minimize the X-ray damage, and he was busily extracting bone marrow with solvents and trying to get this active chemical material out. It had occurred to Dan that maybe what this guy was actually doing was getting transplants from this bone marrow. And of course Dan had been here as a grad student and knew about the twin work and all the other things we'd been doing. So he asked me whether I thought it was possible that they were getting transplanted cells out of this bone marrow, and if so whether I'd give them some advice and help in testing that. As I say, it was a coincidence that I was starting such experiments with chickens, but it was clear that Oak Ridge was a good place for this work, because they knew all about radiation, and they had the equipment, and the experience.

PRUD'HOMME: It was a research institution?

OWEN: Oh, yes. The biology division, under Alexander Hollander's very strong leadership, was one of the national centers of active research in genetics. And at that time, Oak Ridge had some advantages over academic institutions; it could lure people with higher salaries, offer them opportunities to do research.

PRUD'HOMME: And they wouldn't have any teaching responsibilities.

OWEN: Oh, that was not an advantage, really, but to some people it would have appeared so. And they had excellent instrumentation, lots of space and support. Anyway, I told Dan that I thought it was quite likely that what he had picked up in the seminar was true, and that I didn't have reagents for studying this effect with mice, which is what Dan wanted to do, but I had, all set, the rat system. So I sent my rat stocks and reagents to Lindsley and his colleague T. T. Odell, Jr., at Oak Ridge, and then I went to Oak Ridge myself, in 1956, to spend a year there working on bone marrow transplantation into irradiated recipients. And that work on establishing transplants—on prolonging the lives of irradiated experimental subjects—became, of course, a matter of very great interest. There were two other groups in the world, working with other systems, who reported the same thing. But the work had an effect on the science as well, because it established that you could use an irradiated recipient—a mouse or rat whose immune system had been destroyed by the radiation—as a kind of culture vessel for growing cells. The injected bone marrow grew in these hosts and repopulated them, but populated them with the cells you'd introduced. And that became the way to separate out different kinds of cells in the bone marrow—or in the spleen or lymph nodes, whatever you used—and led to the techniques of distinguishing different kinds of lymphocytes: T cells from B cells, and things like that. This is still a very active field in immunology.

PRUD'HOMME: It must have been terribly exciting.

OWEN: Yes, it was. Oak Ridge was a good place to work. At that time, the people there, Dan Lindsley, Drew Schwartz, Kim Atwood, the Russells—it was just a great group of people, all of them gone now. They all left Oak Ridge, except for the Russells.

PRUD'HOMME: Were you ever tempted to stay there?

OWEN: Yes. Hollander rather urgently invited me to stay at Oak Ridge, and I almost decided to do so. Not that I didn't like Caltech and didn't want to come back, but it was a wonderfully productive time. I decided to come back to Caltech partly because of the student thing. I liked working with young people, and I liked teaching which I found rewarding and stimulating. Opportunities for that at Oak Ridge were limited.

PRUD'HOMME: What other work did you do during the late fifties?

OWEN: Here we were involved in all kinds of things related to immunology and genetics. I did some work on human blood groups related to the ABO blood types and to the possibility of a tolerance situation in the erythroblastosis—the Rh factor. And I collaborated with some plant physiologists, particularly Sam Wildman, on plant virus serology, and worked on enzymes as antigens and the inactivation of enzymes by antibody in collaboration with Clement Markert. I can't remember it all, but my group had a lot of different organisms under study, from *Tetrahymena*, ciliated microorganisms, to goldfish to rats to mice and chickens and people; and a good variety of problems. We never were a very highly centralized and focused lab.

PRUD'HOMME: Do you remember the TV program called *The Next Hundred Years*?

OWEN: Yes, that was a project that Rose Blythe worked out, if I remember correctly—programs on things that were going on primarily in science. Since I was involved in these questions of graft rejection, immunological tolerance, and bone marrow transplantation, she asked me to do one of the programs. It was an interesting experience.

PRUD'HOMME: What did you talk about?

OWEN: I started out talking about the twin calves, and then about bone marrow transplants as a way of saving rats' lives, and a little bit about graft rejection—that kind of thing. I wrote a short popular article for *Engineering and Science,* called "Facts for a Friendly Frankenstein." It was a cutesy title—but the idea was that Frankenstein used parts that ought to have rejected each other. I used the visuals from that television talk elsewhere—for example, at the University of Utah Medical School; they had an outreach television program, after hours, for doctors, and I gave a talk for them.

PRUD'HOMME: In the early 1960s, you began getting the usual recognitions and memberships in societies, and so on. Were any more important to you than others?

OWEN: Well, one thing that changed my life was becoming chairman of the biology division. By then, Beadle had become dean of the faculty—I guess it was acting dean of the faculty. And it was clear that although he retained the divisional chairmanship it was on an interim basis. We had begun to look for a successor to Beadle as chairman of the division.

PRUD'HOMME: Were you part of a group that was assigned to look for a successor?

OWEN: We weren't that structured; we worked pretty much all together. It looked as though the most satisfactory choice would be James Bonner, and I think Beadle had actually asked James if he would become chairman of the division. And Beets became increasingly irritated because James wouldn't say yes or no. Beets thought that if James said no, then we'd go look for somebody else, and if he said yes, that would be fine. But meanwhile, Beadle was discharging both responsibilities. And then, very suddenly, Beets was offered and accepted the position of what later was termed president of the University of Chicago. It was just before Christmas. He called a meeting of the faculty on short notice, without an agenda or anything like that—the group was loose, small, and interactive enough so that that was no particular surprise. But then he sprung it on us that he had decided to accept this Chicago job and wanted to leave as soon as he could, maybe right after Christmas. And somebody had to take his place as chairman. And I guess I was the unanimous choice. I agreed to be acting chairman for a year, and over that interval we would look for a real chairman.

PRUD'HOMME: You didn't envision yourself as being chairman?

OWEN: No. I didn't want to do it, and I didn't think of myself as a potentially very good chairman.

PRUD'HOMME: Why didn't you want to be?

OWEN: Oh, I was interested in other things—in teaching and research. And I didn't think of myself as a potential administrator. I was willing to do it for an interval, because it had to be done. So just after New Year's, January 1961, Beets vacated the office upstairs and I moved in as acting chairman. By the way, one of the nice things about it was his desk, the old Millikan desk. The division was a little shaken by Beadle's departure; he'd been here and chairman of the department since 1946. And insecurity develops when you lose such a person. You begin to ask, "Why is Caltech losing Beadle? Are we in terrible shape?" There's a lot of concern. One of the first things I did as chairman was start a committee I called the "GOD Committee," which stood for "Goals of the Division," and appointed a professor from each floor—by then, we had Kerckhoff, Alles, and Church, all connected to each other. Sturtevant, I remember, was the member of the GOD Committee for the third floor. And the mission of each floor representative was to talk to everybody else on that floor about what they thought we should do at Caltech. We obviously had lots of potential. I'd just had an offer, just a short time before that, to go to the NIH as chief of a laboratory there. And George Green—who was Caltech's wonderfully thoughtful vice president for business management—had come over to talk with me, and he said, "You know, if you want to do administration, then the place to do it is here at Caltech." And he went over all the assets of the division that Beadle had built up—the cultivation of donors, the divisional funds, designated funds, all the opportunities we had. And he said, "You won't find that anyplace else, even at the NIH." Anyway, we knew we had potential. We had space—Alles and Church were relatively new. Alles was completed in 1955 and not fully occupied. We had assets. The only question was "What should we do?"

Out of that deliberation came the decision to continue our strength in genetics with increasing emphasis on genetics at the molecular level—which was an easy decision to make—but also to undertake a new emphasis on neuroscience and behavioral biology. We had Dr. [Roger W.] Sperry on our faculty working on the brain. We had van Harreveld who worked with the central nervous system of mammals. And we had Wiersma, who was primarily working with invertebrate peripheral nervous systems. But we felt that the challenge in neurobiology, up to the understanding of behavior, was one of the fields for the future, and that we should try to be stronger, in the Caltech tradition, at picking out an area and trying to be as strong as you could in it.

PRUD'HOMME: Caltech seems never to try to be all things to all men.

OWEN: That's right, and biology in particular illustrates that. But we did want to make a new move. During the year I was acting chairman, not only did this prospect begin to take shape but we didn't succeed in finding a candidate for chairman people could agree on, either outside or inside the institute. And I became dissatisfied with trying to get things done in a temporary position, so at the end of that year, when DuBridge and the faculty asked me to do so, I became the regular chairman. And I served as chairman until 1968.

I didn't have a big auxiliary staff. I had an excellent administrative aide in Jerry Fling, whom I inherited from Beadle, but I didn't have an associate chairman or an executive officer or anything like that. My main obligation was the administration of the division, but at the same time I became quite active in some aspects of the science bureaucracy, I guess you'd call it, back in Washington. I served as chairman of the Genetics Study Section of the NIH with an excellent executive secretary, Kay Wilson. And I served on and later became chairman of the advisory board at the National Institute of Allergy and Infectious Diseases. After I finished the Genetics Section job, I became chairman of the Allergy and Immunology Study Section, and I served on the Advisory Committee for Biological and Medical Sciences at the National Science Foundation. One of the reasons I did all that—and I was able to do this because of Jerry Fling, the excellent administrative assistant—was that I didn't have to be gone long for any of these outside commitments; you'd take the "red eye" to some two-day meeting. Things went along fine while I was gone, because Jerry was such a great caretaker. But the main reason was that as chairman of these committees I had an office and a secretary in Washington. The executive secretary of the Genetics Study Section, and of the Allergy and Immunology Study Section, were both excellent people, whose obligations were primarily to conduct the affairs of the study section. You had somebody back there, and you kept apprised of what was going on. If you needed something done, you had an office back there, essentially. It seemed in the divisional interest to be active, and I think in fact it did work out that way. You knew about things, and you had people you could call for help when you wanted to know something or get something done. It worked out really very well. That work and the open-door way I handled the chairmanship, and the fact that I kept on teaching—the immunology course in particular—did make inroads on my research, of course. I was able to maintain a research program only by squeezing it down some and I bargained with the division faculty to hire a non-tenure-track assistant professor whose interests were similar to mine, who'd keep the lab running and join in some of the teaching. We hired Bobby [Bob G.] Sanders, who'd been a postdoc with me, for that, and that worked out wonderfully well.

RAY DAVID OWEN

SESSION 3

November 1, 1983

Begin Tape 3, Side 1

PRUD'HOMME: In 1961, you became acting chairman, and then permanent chairman of the biology division. Did you do things differently from George Beadle? What was the division like when you took it over?

OWEN: Well, the division was a going concern. Beadle, during his rather long period as chairman, had rebuilt it as an international center, particularly in genetics. And some people found it hard to understand why he should want to be president of the University of Chicago and leave us here at Caltech, you know. So, I sensed that many of us had this feeling of uncertainty, and the procedure I adopted was—and I mentioned this last time—to set up the "GOD Committee," to talk with everybody and kind of take stock and make some plans for the future. We had a very interactive period, when all kinds of thoughts and ideas came up. What crystallized out of that was, first, that we'd continue to be strong in genetics and develop our strength on the molecular level of genetics and molecular biology, and, second, that we would increase our strength in the areas of neurobiology and psychobiology. And we had unanimous support for the adoption of these two goals.

PRUD'HOMME: Support from within the division or from the institute?

OWEN: Within the division. Support from the institute was no problem in those days, with DuBridge as president and expanding prospects on all sides in science—and maybe especially in biology, and especially in this field of molecular biology—and with the support available from the National Institutes of Health, the National Science Foundation, and private donors and foundations. It was an expansive time, the early sixties. In fact, the whole decade of the sixties was. The first move I made as chairman was to identify Charles Brokaw as the first appointment to the faculty under my aegis. My reasons for that were not clearly related to the two larger goals of the division itself. I was concerned about our marine station down at Corona del Mar.

George MacGinitie, who since Morgan's time had been its director, had reached emeritus status. There were people who said the marine station was a white elephant and we ought to get rid of it. I felt that a time might well come in biology when it would be a valuable asset to have such a facility available, and if we once let it go our chance of getting anything like it back again in Southern California, on the coastline, would be nil. Charles Brokaw had been an undergraduate here, and I'd known him very well then—he was an outstandingly successful student. He had gone on to take his PhD with Lord Rothschild in England, and had returned to this country for a postdoctoral fellowship at Oak Ridge for a year. Then he became a cell biologist at the University of Minnesota. We'd had him here to teach our summer course in organismic biology. His research interests were at a level that people here could appreciate and value. He could talk the language with any of us; he was a good mathematician, a good quantitative scientist. But besides that, he knew an awful lot about marine plants and animals, and could teach well and handle the direction of the marine station. That was the first appointment for which I take major responsibility, and it has worked out very well. Charles is now associate chairman of the division and has made very important contributions on a wide front as a citizen of our community.

PRUD'HOMME: When, as chair of the department at Caltech, you decided on the goals of the department, or the GOD Committee got together, did you then have to clear this with the administration, or were you given some freedom?

OWEN: It wasn't so much a matter of having to clear it with the administration. I think conventions change some with the administration and the times; but at that time I felt immediately, even as acting chairman, that I was part of an interactive and mutually supportive administrative community, of which Lee DuBridge was the head. We didn't have a provost then, but Bob Bacher was chairman of physics, and Ernest Swift was chairman of chemistry, and Bob [Robert P.] Sharp was chairman of geology, and Hallett Smith was chairman of humanities and social sciences, and Fred [Frederick Charles] Lindvall was chairman of engineering and applied sciences. We all met once a month in DuBridge's office in old Throop Hall. Our procedures were very informal, and the whole idea was that we helped each other. So an untried division chairman like me, coming in, had the very great interest and support of somebody like

Bob Sharp or Fred Lindvall. And when we talked about things we thought we might do in our divisions, it was a matter of good conversation among the group of us. DuBridge was supportive, and so were the others. Sometimes we'd run into problems, but we would talk them out. It was a time when everything was going along very, very well. The future was unlimited; expansion, if you chose, could be readily funded; and people helped each other. Later, in another incarnation, when I became vice president for student affairs, I came back into that kind of community at a different time and with different people, and I found it much less pleasant to observe the interactions among people in what had become the Institute Advisory Council. There had developed a sense that growth was limited, or even had to be struggled for, not only in terms of internal funds from the institute, but considering the national shrinkage of support of basic research in many different areas. And when the chairman of one division would come up with something he wanted to do, it seemed more often than not that other division chairmen would think, "Well, now, if he gets to do that, does that limit what we can do in our division? There's a set number of appointments to be made. Does the appointment of a Charles Brokaw in biology mean we can't appoint a historian or a geophysicist?"

But back then, with the completion of the Alles Laboratory, we had plenty of space to accommodate people, and we had no objective of getting to be a great big place. In terms of professorial appointments during my eight years or so as chairman we added eight new people to our faculty, but we lost by retirement Sturtevant and Anderson, and Tyler died. So the net increase was not very great.

PRUD'HOMME: Did you make any policy changes in the division?

OWEN: No, I don't think so. Our operation continued much the same, the way Beadle had established it. The division administrative people were very, very good and experienced people I'd inherited from Beadle.

The second appointment I promoted was that of Giuseppe Attardi. We brought him here in 1962, soon after I took the chairmanship. Giuseppe had been interested in cellular immunological questions in which I had also been interested, and I thought of him as a good addition in an area that I was being diverted from a bit by administrative responsibilities. He turned out to be more of a molecular geneticist than an immunologist, but he was a strong appointment. We brought Bill [William J.] Dreyer here in 1963 to strengthen the molecular biology area, especially molecular chemistry, and Bill Wood [William B. Wood, III] in 1964, as a young assistant professor, a really wonderfully strong addition to our faculty. We had appointed Derek Fender as a joint appointment to the biology and engineering and applied science faculty. Derek's appointment probably was the first move we made in the direction of broadening our neurobiology and behavioral biology programs. Derek had been here for a year as a senior research fellow, working mainly with Gil McCann over in engineering and applied science. He was interested in information processing in the central nervous system and the role of very small, quick eye movements and in increasingly impaired vision, and things like that. His was the first joint appointment by biology and another division, as far as I'm aware. Another was Jerry Vinograd, who had been for years a senior research fellow in chemistry. I remember Ernest Swift coming over to talk to me about Jerry early in my chairmanship. We decided we wanted to keep Jerry here and give him the kind of support that would make the conduct of his important research easy. So he became a professor of biology and chemistry in 1962. Promoting interdisciplinary interests seemed to me to be important. Quite often, some of the most exciting things that happen in science happen in these interstices between the boundaries of disciplines.

Then in 1964 we made our first real move in neurobiology, bringing Felix Strumwasser here. Felix has only just announced that he's leaving the institute, and I'm awfully sorry to see him go because he made important contributions over those years—a first-rate neurobiologist.

Then Seymour Benzer was here on leave from Purdue University as a visiting associate, and we persuaded him to stay in 1967. And just at the end of my chairmanship, we recruited Jim Olds and Marianne Olds, from Michigan. Although by then Bob [Robert L.]Sinsheimer was lined up as my successor, and it was really Sinsheimer rather than I who accomplished that.

So that gives you kind of an overall feeling for the changes in personnel. And we did move—deliberately, but I think strongly—in the directions of the goals we had defined for ourselves. The big change over the sixties was not in numbers of the faculty—because, as I said, the net change was not great—and it wasn't in postdoctoral fellows. I counted up in the catalogue: there were sixty postdocs the year before I became chairman and there were sixty postdocs in the last year of my chairmanship. But it was in numbers of graduate students and of undergraduate students with major interests in biology. There we increased greatly through that decade, both at the undergraduate and the graduate levels. The graduate student increases came about in substantial part first of all, because we had active programs in which professors were welcoming graduate students. But there were also sources of support for graduate students, and we were no longer exclusively dependent for their support on teaching assistantships. The National Institutes of Health had started the training grants, and we were successful in getting those funds. And we ended up more than doubling the number of graduate students in the eight years I was chairman.

PRUD'HOMME: When you say professors welcomed students, what do you mean?

OWEN: I mean most of our professors thought it would be good to have more graduate students studying for PhD's with them. There were some quite large empires, like James Bonner's, that absorbed large numbers, and there were some relatively modest operators. But on the average, there were plenty of good laboratory homes, with excellent professors, to which we could recruit graduates. And we could also offer them support for their graduate work.

PRUD'HOMME: Do the students' interests ever change the undergraduate curriculum within the department? Do you ever see a shift in interest of the students?

OWEN: Yes, and I think you see responses to those shifts of interest, too. I can think of much more recent examples. This last summer, after I finished being dean and came back to spending part of my time being the undergraduate advisor and set up the Biology Undergraduate Student Advisory Council [BUSAC], the students expressed interest in, for example, the continuation of a course in evolution, which with Norm Horowitz's retirement we might very well lose. And I think of a number of curricular areas in which the concern of the students was a major goad in getting the faculty to meet these needs.

PRUD'HOMME: You taught a number of courses. Which were your favorites?

OWEN: Let's see, I started out teaching immunology, first with Sterling Emerson and then by myself. I guess that has been, over the years, my major teaching experience, and I feel very good about it. I taught immunology for about twenty years, pretty much by myself, but using people who worked in my research group, and teaching a laboratory course as well. I have found that very rewarding. That drawer there beside you has my old class roll books in it, and when somebody like Howard Temin comes back after his Nobel Prize and has coffee here, my students enjoy my looking up and finding what grade Howard got in immunology when he was here, and so on. We've had awfully good students over the years, and it's nice to let the current students know what these past students have done.

I also taught general biology. At first, van Harreveld and I shared the course, and then I taught it by myself for a couple of years and enjoyed that. I returned to teaching general biology this last year with the support of an excellent group of graduate TA's, which went very well. I'm pleased with the treatment of the course in the teaching-quality feedback report.

PRUD'HOMME: Is this how you get to know your students? Because the three times I've been here, it's my observation that your students all know and love you.

OWEN: Well, yes, but this doesn't come in substantial part through the formal instruction, I'd say. In 1967, as I was approaching the end of my chairmanship and thinking of having more time available, and also of what was needed in a general sense at the undergraduate level, I started two new courses—Biology 2, called Current Research in Biology, and Biology 3, called something like Ethical and Social Aspects of Biology. Both of these courses were designed for freshmen; Biology 2 was limited to freshmen. I continued to be responsible for that course up to my retirement in July. It was a very rewarding course to teach. I limited the enrollment to two sections of about sixteen students at most; I called them the Robins and the Bluebirds. It was a first quarter elective for freshmen and met on Tuesday evenings from 7:30 to 9:30. Each Tuesday evening one of the two sections would be meeting in my home; since we live only a couple of blocks away, it was easy for students to walk up to Rose Villa Street and down to our house. If the Robins were meeting in our home, the Bluebirds would be meeting at the lab, and in each case, they met with a member of our faculty who had the leisure for a couple of hours to talk about what he was doing and what he was interested in. At home, my wife usually made cookies or something, and we'd break, and then have an interval afterwards just discussing more general things, like aspects of biology, or being a student at Caltech, or whatever. I think the students have really appreciated it, partly because when they first come here, many of them are in awe of Caltech professors and feel uncomfortable about approaching them. A student might have an idea that he wanted to work in somebody's lab but he wouldn't know quite how to go about it. So they met these professors one at a time, under conditions that made for good interaction. And then at the end of the quarter, we would have a Christmas party, and the Robins and the Bluebirds both came with no guest professors at all. I had the Robins tell the Bluebirds about the professors they'd met and what they remembered and thought of them, and the Bluebirds would report to the Robins. So altogether, that was exposure to about sixteen or eighteen of our professors just in the first quarter. If you became interested in what a particular professor was doing it was possible to start right away, if you wanted to, as a second-quarter freshman working in his research lab. And a very high proportion of them started out in research labs that way. Over the years—that was more than fifteen years ago—my roll books are full of names of people who as freshmen took Biology 2 and are now doing all kinds of things, having gone on to PhD's and teaching elsewhere, or graduating from law school and setting up a practice.

PRUD'HOMME: Did you find that students would change to biology from what they had planned to major in when they came to Caltech?

OWEN: This has changed over the years. In the fifties, say, there were very few students who came here with the idea of being biologists, and there weren't many switches to biology either. If a graduating class included four biology majors, that was regarded as a pretty big group. In the sixties, as biology attracted much more attention and seemed more interesting—and also offered a better prospect of making a living—the numbers increased. But still it was true that most of the students who took Biology 2 at the beginning of their freshman year had some interest in biology but thought it more likely that they would become physicists or something else.

PRUD'HOMME: What brought about the change?

OWEN: I think, for one thing, biology came to the attention of people in secondary schools as a field in which interesting things were happening, with great prospects for the future. Some of that was plain hype, but a lot of it was genuine. In the meantime—and a lot of our students are pretty attentive to this kind of thing—physicists were finding it difficult to find jobs, and maybe they were driving taxis someplace. This kind of thing gets exaggerated when young people hear it. Whereas, the National Cancer Program was plugging along at the rate of six hundred million dollars a year and people were getting support for research in biology. So students saw a future in biology and excitement in it. And then Caltech students find most respectable a field of science that relates to the physical sciences—math, physics, chemistry—and the kinds of biology they saw being done here were the physics, the chemistry of life. So it appealed to them.

PRUD'HOMME: I'm told that you changed the usual practice of having a professor's name appear on the research contributions that came out of a laboratory.

OWEN: That was just in my own publications.

PRUD'HOMME: But that's very unusual.

OWEN: Yes, and there are good reasons, I think, why professors who have research groups should continue to have their names appear on the publications that come out of their labs. For one thing, it helps bibliographers. They see this common name, and if they want to find out what was done at Caltech in some particular area, they look for this key name, whereas a lot of the young people are essentially nameless. And the professor deserves credit for having set up the environment in which the work is done. Often, not always, he guides the work and is substantially responsible for it. I decided in the middle 1960s that the kind of contribution I was making to my own graduate students and postdocs hardly justified that. I was spending a lot of my time in administration and teaching, and while I continued to provide the environment in which they could work, they were the ones doing the work. I kept in touch with what they were doing, and I think most of them would say I helped, and so on. By then, I was a member of the National Academy of Sciences and people knew my name. It bothered me to think that my graduate students, like Jeff Frelinger, for example, or Sue [Suzanne] Ostrand-Rosenberg, or Sue [Susan] Melvin, or Elizabeth Blankenhorn, or Tommy Douglas, who were investing a great deal of not only their own effort but their own originality and creativity, and doing very well at it, would publish papers in which what people saw would be my name because my name was a familiar one. They wouldn't even take note of Frelinger or Blankenhorn or Rosenberg or whomever. And I didn't need papers to build my status or prestige or anything like that. I wasn't after any more prizes. So for a variety of reasons I decided that I'd drop this convention, and I did not put my name on papers coming out of my lab.

There was an occasional exception; I published a paper jointly with Don Shreffler, because I felt I had made a key contribution, and it was nice to have one paper on the record that identified Shreffler as having been my student: I've been very proud of what Don has done since he left; similarly with Bill Hildemann. But I never wanted to build up a large set of records by including everything that everybody who ever had anything to do with me did.

PRUD'HOMME: Did this anger any of the other professors at the institute?

OWEN: I don't think most people were aware of it, or that it would have mattered to them. As I say, there are plenty of good reasons for the convention. I do think that in a more general sense, on the national scene, the tendency for people to put their names on papers for work that they have really very little direct knowledge of, or contact with, is pretty dangerous. The occasional instances you hear about—of fraud in the conduct and reporting of research—are often papers that include very distinguished people, with a high degree of identity and a justifiably high reputation, who just didn't know what was going on. And they *ought* to have known, if they were going to put their names on the papers. So I very much agree with one of the conclusions of the Morgan panel that went into the question of fraud in biological research, a year or so ago, nationally—not our Morgan; this was a different Morgan—that people shouldn't put their names on papers unless they are, at the very least, prepared to swear by the integrity of everything that's in the paper. It ought to be clearly specified in a footnote who's responsible for what—or the names of people who made lesser contributions should be acknowledged that way—instead of including them as authors.

Begin Tape 3, Side 2

PRUD'HOMME: Who were some of your favorite students during this period?

OWEN: Well, I never had a student I didn't like, beginning with Irving Rappaport and Henry Gershowitz, back in the fifties. We had a very lively time in the late 1950s: Jim Berrian, who's dead now; Leslie Brent, from England; Bill [William H.] Hildemann, certainly. Bill was a very interesting graduate student. He had served in the Korean War in the Marine Corps and was, I believe, a captain. So when he came back to start graduate work, he was already a leader of men. He'd done his undergraduate work largely at USC; his graduate record exam scores weren't all that impressive, and I had to make a special effort to get him admitted to graduate work, because the so-called objective criteria of his merit were not acceptable.

PRUD'HOMME: How did you recognize his merit?

OWEN: I talked with him, and by then I had become familiar enough with procedures on admissions committees and things like that to feel that the exam scores and quantitatives were not necessarily all that good a predictor of how creative a person might be. At any rate, he came in. One of his great abilities was to work with aquatic organisms. He thought he would look at transplant rejection, using scales of goldfish. He knew how to do the operation on goldfish very readily, and he also knew that it should be an ideal tissue for following the rejection reactions. Scales are beautifully designed structures for that purpose; because when you first make a scale transplant and there's a reaction on the part of the host, the scale loses its blood supply; it has a nice reflective plate. We can look at it under the dissecting microscope and see exactly what's happened. It's almost as if it had been engineered for close observation. Furthermore, because goldfish are cold-blooded animals, you can affect the rate at which reactions like this occur in them by regulating the temperature of the water in which they're kept. If you keep a goldfish in fairly warm water it goes through the process of scale-graft rejection quite rapidly. On the other hand, if you keep it in cool water—say down around four degrees—it will take weeks and weeks to do something that would be accomplished in days at the warmer temperature. By spreading this out on the time axis, you can separate processes that would normally overlap in a quick reaction in a warm-blooded mammal.

Bill went on to become one of the world leaders in comparative immunology. He developed the concept that even organisms as lowly as sponges and corals have a system for recognizing foreign tissues and rejecting them. So it was a great joy to have Bill as a student, and he did wonderfully well. Afterward, I arranged a trade with Peter Medawar. After Bill got his degree, I sent him to London to be a postdoc with Peter, and Peter sent Leslie Brent to be a postdoc with me. Brent had been Medawar's main collaborator in some of the key work on immunological tolerance.

When I became chairman, I cut down on the number of my graduate students, because I had other obligations. I never felt it was fair to take grad students and sort of turn them loose and be unable to give them attention, or turn them over to somebody else. But when I finished my chairmanship, I went through another period of accepting grad students. And I had a really remarkable group—Tommy Douglas, Jeff Frelinger, Sue Ostrand-Rosenberg, Sue Melvin, and then, very shortly, Elizabeth Blankenhorn. And we had some good postdocs here, too. So we went through a period from 1968 to 1975, until I took up the job of being dean, when we again had a very lively group. It's an honestly great pleasure to see how well all these people have done in every way. They were working on a great variety of problems, too. Remember in my lab, I never adopted the principle that grad students should work on a problem for which I was getting research grant support and be components in some kind of machine, all of them working on the same highly focused area. I don't mean to imply that the way I structured things is better than other ways. But it does have some advantages, because each grad student had his or her own material and problem and area, often things that they had dreamed up for themselves, and we talked over and tried very diverse kinds of things.

In reply to your question about favorite students, I guess I'd have to say that although I greatly enjoyed these grad students and postdocs, I've gotten my biggest kicks out of the undergraduates over the years—partly because Caltech has such a wonderfully good set of undergraduates and partly because I think you feel that you influence lives to good effect more easily, in a way, by helping them. Although a lot of freshman come in and take everything in stride, when a person comes to Caltech—even a very good freshman—it's a pretty big, different world, and there are adjustments to make. Sometimes it can get very difficult for them and they run into things they don't know quite how to handle; and sometimes you can do something that makes a difference. Although it sounds corny, if you think of how you're investing your life and figure that there are a fair number whose lives you've been able to make better, it's a big magnification factor as far as the significance of your own life is concerned. But that's not, I guess, the primary reason. The reason I decided to become dean was that I thought undergraduates maybe weren't getting the kind of attention they deserved, and I wanted to see whether I could help them out.

PRUD'HOMME: You were on the Ad Hoc Faculty Committee on the Freshman Year. Why did Caltech need such a committee? Who was on it? What did you do?

OWEN: This was just the middle of the 1960s. It was a fairly unsettled time nationally and there was a lot of ferment about how this wasn't necessarily the best of all possible worlds and what might be done to improve it. Ernest Swift, who then was chairman of the faculty asked if I would chair an ad hoc committee on the freshman year. People began to think that maybe we should be doing something different, particularly about our freshmen. One of the factors in the ferment at the institute at the time was something that I was not a part of. The psychologist Carl Rogers met with a group of people who'd been selected from different parts of the institute community to have dinner over at the Honker and talk about things. Carl Rogers was a person who stimulated people to take fresh looks at things, question things, and talk about what might be done differently. As I remember it, although I was not a part of that Honker group, two people who were considerably influenced by it were Bob [Robert F.] Bacher, who by then was provost, and Ernest Swift. Among other suggestions, I remember Ernest talking about how our students were so good and it took so long from beginning as a freshman to getting a PhD that maybe we could develop a program specifically for Caltech in which a student could be admitted to graduate work early—at the end of his sophomore or beginning of his junior year—and make more efficient progress toward a PhD that way. Another idea the Honker group discussed—and Bob Bacher adopted quite vigorously—was that one of the problems for students adjusting to Caltech in the freshman year was the grades. These freshmen came from backgrounds in which they had mostly earned straight A's, and their parents expected them to continue to make A's; and here half of them were in the lower half of the class. They brought with them from high school a sort of competitive attitude and an emphasis on the grade. So Bob became interested in the possibility of going to a pass/fail grading system for freshmen across the board. As I can remember it, those were two of the germinal ideas.

When the ad hoc committee on the freshman year was set up—and I can't give you its full membership, though I remember Robbie [Rochus] Vogt, who was a young associate professor in physics—we got together in my home. I was chairman. It was an ad hoc committee of the faculty, not an administrative thing. We met in my home in the evenings, frequently and long, and drank a lot of bourbon. That was back in the days when it was conventional to drink a lot of bourbon and scotch and things like that—since then, we've gone to wine drinking, mostly. We had the student members of the Students' Educational Policies Committee over to talk into my tape recorder. It was interesting to hear what the students thought should be done to improve things at Caltech.

PRUD'HOMME: Admitting students into your decision-making process was in itself a tremendous change for the sixties.

OWEN: Well, it was a time when students were up. Caltech has always been a small enough and interactive enough community so that students have had input. Anyway, we talked about all kinds of things. And as you might predict, with a very unconventional group of profs, what came out of it wasn't necessarily what people might have predicted. But one of the first things was the inauguration of pass/fail grading across the board for freshmen.

PRUD'HOMME: What was the reaction of the freshmen to that?

OWEN: The students thought it was a great thing. There were a few who felt that they were being deprived of the feeling of security and success that getting all A's gives you. But mostly the students were among the most informed people about the kinds of stress that arose because of competition for letter grades, and their feeling was that it would be better to have things on a different basis.

PRUD'HOMME: You had mentioned that it was often the parents who were more interested in the straight A's.

OWEN: Yes. The parents had come to expect it. It's probably still true that most of the parents of our students are proud of how well their kids have done academically through grade school and high school—how many A's they got and how high their scores were on the college board examinations. Furthermore, parents are of course aware that Caltech is a very expensive place, and so there must obviously be something special about it. Then they find that their son or daughter comes here and gets a C in physics—well, that just can't be! The young person is goofing off or something! "Here I am paying all this money for this, and you're getting a C!" What they don't understand is that almost every single person in this two-hundred-person freshman class has almost exactly the same background and experience as their son or daughter, and if grades are going to distinguish between them at all, some of them are going to be getting C's.

PRUD'HOMME: Did the professors mind the pass/fail grade?

OWEN: We had quite a fight getting it established. Once we had decided to make that recommendation to the faculty, it was openly debated. We had discussions in the basement of the Athenaeum, I remember, and full-scale faculty discussions, and finally a vote on it. Nearly half the professors who came voted against it; it managed to squeak by only on condition that we would inaugurate it for a two-year trial period. There were terribly dire predictions: if the students didn't have A's to work for they wouldn't work at all, and the standards would fall.

PRUD'HOMME: How could you test whether the change was successful or not? How could you test the reduction of tension in the freshman class?

OWEN: Well, one of our committee's main concerns for that two-year period was to keep a check on how things were going. We did all kinds of things. The institute psychologist, who was very much interested in this, kept very close records of the kinds of student concerns and psychological problems he encountered. He arrived at the conclusion that although there was no discernible, statistically significant change in the frequency of students' worries, the character of their worries changed quite a bit over the interval. The student didn't have A's to worry about. And on the whole their concerns were much less severe clinically. In the second year, we kept in close touch with the teachers of the sophomore courses, because some people had predicted that the freshmen would goof off and wouldn't be as well prepared as sophomores as they would have been with letter grading. But we found that the students did better rather than worse after a freshman year of pass/fail grading—partly, I think, the students felt on trial and wanted to do well. I think the main effects, though, were on students' interactions with each other; because under letter grading, with a limited number of A's and all these reasons to compete for them, the students had been in very large part competitive with each other—though never quite as bad as the pre-med population at a big university. But when there were no reasons to compete with each other, they began to help each other instead. I think the beginning of pass/fail grading was coincident with a considerable improvement in the attitude of our students toward each other. It's set up in the freshman year, and then it continues; by the time they're sophomores, they've matured enough so that they don't feel that they have to cut each other's throats.

PRUD'HOMME: When I was talking with Chuck Newton [Charles Newton, lecturer in English], he said that he's always felt that a lot of the incoming freshmen were very inept socially because in their high school classes they'd been loners. They'd always been the ones who'd done extremely well and had gotten straight A's, so they'd been isolated. And when they came here, learning how to deal with other people was one of their great problems.

OWEN: Yes. One of the big ways that's accomplished, of course, is through life in the student houses. And I can't say that it began with pass/fail grading for house members to support each other. But I do think pass/fail made a primary change in this atmosphere of competition. Professors would still get students coming up after class when a marked quiz had been handed back. But the primary kind of concern used to be, "Why did you take off eight points on this paper, when so-and-so only got six points taken off?" or, "I really meant thus-and-so. Shouldn't I get credit for it?" These concerns were now replaced by questions like "What didn't I understand about this?" Anyway, the upshot was that when the faculty did reevaluate the continuation of pass/fail grading, it was adopted by a great majority. We felt very good about that.

In the interval, our committee had continued to talk about all kinds of things. We never did get around to discussing what Swift had talked to me about earlier—about making quicker progress toward a PhD. I think we generally felt that it wasn't a very good idea, or would be too difficult. But we remained concerned—and our concern was reflected in the students' opinions—about the rigidity of our freshman year. For many years, as you probably know, all Caltech freshmen took the same courses. They went through the freshman year in lockstep.

They were divided up into sections of sixteen and they stayed in their section—you went to physics, and you went to math, and you went to chemistry, and you went to chem lab, and you took English and history; and there were no options, no choices. Only a short while prior to the appointment of our committee, the
humanities and social sciences people had relaxed this rigidity to the extent of permitting either English or history rather than requiring both of freshmen. But that was the only option the students had. Students had begun to come in, as I mentioned, who were interested in biology, but there was no way of taking biology as a freshman—or geology either. Science students were required to take biology as first-quarter sophomores, but quite a few students wanted to get started earlier on biology. And they didn't want to be all alike. They wanted to feel they had chosen some of the things they were doing. They wanted heterogeneity within the student body.

So we worked on a curriculum. It was hard, because our students were already hard-worked. The requirements in physics and math and chemistry and the humanities were intense. And you couldn't just say, "Well, we're going to add some electives," because no reasonably human student would be able to add to that load. So we had to persuade the physicists and the chemists and the mathematicians to cut things down a little, to make room for students to take some options. Fortunately, we had very strong people from those divisions on our committee. The mathematician, [Henri Frederic] Bohnenblust, who was head of the mathematics group in Physics, Mathematics, and Astronomy—just a wonderful person; he'd been dean of the graduate school for a number of years—played a major role in making it easy for the mathematicians to cut down enough so that the students would have a chance to take my Biology 2 course, for example.

PRUD'HOMME: Do you mean cutting down the number of courses in each area?

OWEN: Not number of courses, but the expectation of hours of work per week.

PRUD'HOMME: So that the electives would be added on top of the course load.

OWEN: Yes, that's right. But the course load in the required courses was reduced enough to make that possible. Our students are still required to take physics, chemistry, mathematics, and a course in the humanities, but a pretty high proportion take Biology 2 or other elective courses their first quarter. And Biology 1 has become a course mainly for freshmen now, rather than for sophomores. When I taught it last year, I had fifty freshmen in that class. Geology, engineering and applied science, computer science—the students can spread out a little bit in their freshman year, and that's before they've chosen an option. This gives them the opportunity to try other things, and they may find that what they thought they were devoted to isn't what they thought it was and that something else looks more interesting.

Among the other things that the committee gave its attention to was a very unpredicted and unlikely thing—that we should admit girls to our freshman class. I think it was Bohnenblust who brought that up. He said, "If you really want a lot of change in the freshman year at Caltech, we should start admitting women." And I remember what a revolutionary idea that was at the time. But we took it pretty seriously and started collecting opinions and data. We found out some very interesting things. First of all, we sent questionnaires out to all of our alumni, asking, "What would you think about having women at Caltech?" When the responses came back, I took them all, from very hostile to very supportive, and plotted them out in terms of frequencies against the year in which the alumnus graduated. Alumni from the thirties and forties were mostly against the idea; they thought it would destroy this institution and its character to have girls here. But the closer they got to being current, the more likely they were to say, "Ah, gee, that's what you really need to do."

Other single-sex schools were thinking along these same lines. And I think it was Yale or Princeton, in association with Smith College, I think, and the College Board Examination people, who circulated a questionnaire to high school juniors and seniors, in which there was some way of indicating how good a student was—their College Board scores or their high school records were coded into it. And lots of questions were asked, but among them was this one, kind of buried: "When you go to college, would you rather go to a coeducational school or to an all-men's or all-women's school?" And it turned out that the very best men—boys still in high school—very often said they wanted to go to a coeducational school. I think part of the reason was—as Chuck Newton said—that a lot of boys who were very successful students with very lively minds felt socially inadequate. They were the ones who hadn't dated yet, and they didn't know quite how to get along with a girl. And they thought that when they're going to college there should be girls in the environment. Well, it's amazing what an argument that made, especially with our trustees but also with some members of the faculty—that we were losing some of the best male students.

The admission of women was, of course, debated by the faculty, and there were people against it and people in favor of it. But by then we'd learned how to operate effectively as a committee. We anticipated all the possible arguments contrary to our recommendations and divided up responsibilities. "In the discussion, this point is raised: Who's going to deal with it and what's he going to say?" So we went into this debate prepared, and it worked out wonderfully well. Some of the opponents of the proposal were taken aback because they hadn't prepared as well as the committee had.

PRUD'HOMME: Of course, the tradition had been in science that women were interested only in the biological sciences, never in the physical sciences.

OWEN: Well, this was something some people said. Actually, as you see from our experience, that's not really true. Our undergraduate women scatter among all the different options, very successfully.

Lee DuBridge, our president at the time, was not one of the strong proponents of this change.

PRUD'HOMME: Why not?

OWEN: I'm not sure. First of all, Lee was pretty well set in the idea that this was a place for outstanding young men. If you look at Lee's inaugural address when he came—of course, in those days, everybody talked about the young *men*, as though women hardly existed. But also I think Lee was afraid we'd have girls living with guys in the student houses or something like that, and how would we handle a women's dormitory. The chairman of the board of trustees, Albert Ruddock, had expressed concern earlier about a picture in the *Facts Bulletin*—which was sent out to applicants for admission to our freshman class—that showed a girl coming out of one of the student houses. I think Lee was quite concerned with how two sexes would mingle in our undergraduate community without having problems. Lee himself, of course, did his graduate work at the University of Wisconsin, and he came from a coeducational background.

PRUD'HOMME: But at this point, you had female graduate students?

OWEN: We'd had female graduate students for more than a decade.

PRUD'HOMME: Did you have any female professors?

OWEN: There were women on the faculty in the humanities—I don't remember any women in regular professorial appointments in engineering or the sciences. Olga [Taussky] Todd, who was a distinguished mathematician, was a research associate.

PRUD'HOMME: Did you have postdocs who were women?

OWEN: Yes. It's a curious thing, you know. If you ask people who their very best graduate students are, just about anywhere, very likely they will list a disproportionate number of the women. But when you look at employment of tenure-track people, it's still true that girls aren't equal yet. After we got this through the faculty—and there was some apprehension about it, but on the whole, pretty good support for it, and Lee DuBridge joined in, too, and thought we'd build a women's dorm—we had to present it to the board of trustees. I was chairman of the committee, so I got the privilege of making the presentation. It was fortunate that this came at the time of the annual meeting of the board of trustees which is held out in Palm Springs. The timing was fortunate first of all because it was a nice, relaxed meeting; but more important, the trustees' wives were there, too, and they knew about this. It was nice to see people like Marian Jorgensen and other trustee wives beating on their husbands: "You've got to feel good about it, letting in women at Caltech."

PRUD'HOMME: Of course, you had the ultimate ammunition in your shotgun, which is that the bright guys wanted coeducational schools.

OWEN: Yes. I didn't feel that that was a legitimate basis for deciding whether women should have suffrage or not, but it was helpful. Anyway, the decision was that we'd do it—we would admit women in two years. I got back on the Freshman Admissions Committee then, because I was very much interested in promoting—you know, we were told we wouldn't get many female applicants; and those that we did get would wear thick glasses and not be able to dance, and things like that. That's proved to be so wrong. [Laughter]

RAY DAVID OWEN

SESSION 4

November 7, 1983

Begin Tape 4, Side 1

PRUD'HOMME: We were talking about women and the admission of women at Caltech. I was wondering if there are areas that women are more interested in—whether their interests differ from men's, and whether women are more interested in the biological sciences? Have their interests changed since coeducation was introduced at Caltech?

OWEN: I might just say a couple more words in connection with the admission of women here. As I mentioned, one of President DuBridge's concerns was a matter of housing. He thought we would need to have a women's dorm prepared for them and wasn't quite sure where we would find the money for that, or even a place to put it. We had a two-year interval between the time when we resolved to admit women and our first female admissions. Over that interval, on investigating housing patterns at other coeducational colleges, we decided that we didn't need a separate women's dorm. In fact, other colleges were moving away from that. However, President DuBridge did have a sort of separate suite prepared for women, one of the wings or alleys in one of the student houses, which was nicely refurbished and decorated in a style thought to be appropriate to girls. It was rather amusing that when the girls did arrive, they didn't exactly like that idea of segregation and special treatment at all. It's always been my observation that our women students are students, and they want to be like other students. So very shortly, although the girls were initially assigned to rooms in this area, they began to move into the different houses and so on. As I understand, it was sort of a crisis at the time, but it worked itself out very well; so that now girls are distributed in coeducational housing in all of the houses, except that we do have a separate small facility, as you probably know, where girls can live if they wish to or if their parents wish them to.

I think those first undergraduate women on campus had some very special problems; they were regarded as oddities and subject to a great many social pressures and demands. I had the greatest admiration for them and the way they handled the situation. It was hardest for the first ones, because from then on, there were sophomore as well as freshman women; and when they became juniors, there were freshman and sophomore women, and the numbers and the experience began to build up so that women students became much more comfortable here. I'm not aware now of any gender-related problems in our undergraduate student body at all. But it was sort of rough at the time. They responded extremely well, and they were aided by the leadership of a very small number of quite exceptional upper-class women who were admitted that first year.

PRUD'HOMME: You mean women who came in at the upper-class level?

OWEN: Yes, that's right. There were, I think, two who came in as transfer students that first year, and two more as juniors the next year. And I remember Sharon Long in particular, who was a wonderfully able, broadly perceptive young leader. She helped a great deal with the freshman women, helping them get started—although none of them regarded themselves as primarily women as such; I think very properly they thought of themselves as students. That relates to what I might say about their choices of fields. A fair proportion of the women have always been interested at the start in biological sciences and looked forward to careers in either biological research or in medicine or other biology-related things. But the women did spread around in all the different options. There were women who became engineers and chem engineers, physicists and mathematicians, geologists, so that I don't think of any of the options at the institute as being gender-related. The course in current research in biology—the first-quarter freshman course I had set up in the late 1960s as part of this same reform—continued to attract a disproportionate number of the women; I'd say maybe half of the girls chose to take that elective, reflecting the fact that biology was one of their interests. And in fact it was quite exceptional among freshman courses, because the class was small, in two sections of sixteen students each. And typically, just about half the students would be girls. It made for a very nice arrangement. As I mentioned, there was a social aspect to that course—the Robins and the Bluebirds would take turns meeting in my home. The girls as a population—if you can generalize, and recognize that there are a lot of individual exceptions both ways—the girls tended to be a little easier socially at that age, to participate readily in discussions, and to help get the cookies passed around, and so forth. On the whole, they added a lot, with their willingness to participate in discussions. Many of them were unselfconscious about relating to people and feelings and things like that. But I don't think one could say that they're primarily biologists; their backgrounds of interest are as varied as those of our other students. They're very able; they have done very well. The attrition rate among our women has been lower than that for men.

PRUD'HOMME: Was it hard finding the first ones?

OWEN: One reason that I went back on the admissions committee, as I mentioned, during that two-year interval before we admitted the first ones, was that I wanted to play some part in helping to recruit women applicants. And in that two-year interval, some of us on the admissions committee made a point of asking, when we visited a high school, whether there were sophomore or junior women who might apply to Caltech when they got to be seniors, and talking with them. I also had the help from a technician, Sandy [Sandra] Webb, whom I had employed to work in my research group. She came from Australia and had been a sort of housemother in an eight-hundred-girl dormitory back in Australia. I thought she would be a good person to have around because we weren't planning to appoint a dean of women or anything like that. And Sandy went with me on these interview trips and talked to the sophomore and junior women before we admitted them, and served as a kind of advisor. Later, she and her husband, John, became the first married RA's—resident associates—and they lived in Dabney House. They had their first baby there.

PRUD'HOMME: Tell me about your work on the admissions committee.

OWEN: Well, I began serving on the admissions committee back in 1947, I think it was—the first year after I took up a regular faculty appointment here. I served for a fairly prolonged interval, then rotated off and on again. So, I've been on and off the Freshman Admissions Committee several times.

PRUD'HOMME: Have the admissions standards changed?

OWEN: I don't think the standards have changed. Our techniques have changed a little bit.

When I first went on the committee, for example, we gave our own admissions examinations. It wasn't long after that that we went to using College Board Examinations entirely. We had increasing numbers of applicants, and the class size increased a little from, I think, 160 when I first went on in the forties, up to 200. When we admitted women, we enlarged the size of the freshman class by another twenty, because some people didn't want to reduce the number of men by admitting the corresponding number of women. But all through the time I was on it, the admissions committee continued to operate essentially with the same procedures. We welcomed applicants and rated them in terms of their performance on the examinations, selected a proportion who appeared to be the best applicants—five hundred or six hundred of them—to be visited in their high schools, where we talked to them and their teachers. There were twelve to fifteen members of the faculty on the admissions committee. It was a fairly expensive operation and involved some investment and a fair amount of faculty time. But I think this procedure was absolutely justified—not so much in terms of what it added to the precision of selection of the freshmen, because there's only a limited amount that you can learn in a brief interview, or even in talking to teachers, although you do learn some things. Teachers will tell you things, for example, in conversation that might be different from what they might put on a form or even in a note of recommendation. But the big advantage in using our faculty in this way is that it's our most effective contact with our sources of students. If you visited a high school for the first time, quite often you would initially be regarded as a college recruiter. There were recruiters coming all the time. But it very speedily became evident—say, in my territory in the Midwest—that I wasn't recruiting. I was there to see someone—usually one or more of their very best students, who had already applied to Caltech. And the purpose of my visit became more counseling than anything else. And not infrequently, I would see an applicant and talk to him about his interests and end up saying, "You'd probably get along at Caltech, but maybe that's not the school for you, because if you're interested in being a radio announcer as well as a physicist, maybe you can combine those two things better somewhere else than at Caltech." So that the next time you came back to that school, quite often they were looking for you and you were treated very differently.

PRUD'HOMME: You must have thrown some science departments into a state of panic when they realized that this was a visitation.

OWEN: Well, I thought the schools were, on the whole, extraordinarily cooperative because sometimes—since you had to schedule visits to four or five schools during the course of a day—sometimes you came when the physics teacher or the math teacher would be in class and have to arrange to step out in the hall and talk with you a little bit about the applicant.

PRUD'HOMME: How wonderful this kind of attention was for the students!

OWEN: Yes. And the people who have been willing to serve on the admissions committee are a special kind of Caltech faculty, too; they are willing to give up that time because they're interested in students and enjoy the visits and talking with teachers. Very few of Caltech's alumni are connected with secondary school education at all—it's rare that one of our graduates goes into high school teaching—and high school teachers tend to automatically direct their best students to schools they know. If they went to the university, why, they were likely to send their students to the university. By sending faculty members to visit the schools, Caltech had an in on what I think was its most precious asset: that is, getting the best students there are.

PRUD'HOMME: Do you have certain high schools that have been feeders for Caltech, who consistently produce top-quality students?

OWEN: I think there are excellent students graduating from all kinds of different high schools all through the country. But there does tend to be a repeat experience for particular schools, partly relating to the fact that a member of the admissions committee has visited there, partly related to the fact that if a student comes from Central High School in Omaha, then he or she is likely to go back at Christmastime and talk to the seniors about Caltech. It builds up with experience. There must be many schools throughout the country that still don't know much about us or think of us as a place to do undergraduate work, or that think of us exclusively in terms of JPL or physics, without knowing that we have distinguished biology and geology and chemistry here as well.

PRUD'HOMME: What about the recruitment of blacks? There seem to be, of course, many Asians here, and now many Indians. Have you actively recruited minorities?

OWEN: Yes, there's been very active recruitment of admissible black students over the years, primarily in the hands of Lee Browne, who is director of special student programs and secondary school relations. His work is in substantial part directed toward minorities—blacks, Chicanos, and so on. The problem there is to compete successfully for the qualified black applicants, because it's still true that a very large proportion of the black young people in the country graduate from schools that don't offer advanced mathematics or even many of the sciences, so they're at a severe disadvantage. And I think Lee Browne has done an excellent job over the years in identifying able and qualified black high school students, partly through his stature in that community and through MESA—a Minority Engineering and Science Association—for example, in the state of California; and then encouraging their application to Caltech and going through the admissions procedure, working with students who are clearly able but are handicapped because of their background, whose educational opportunities have had deficiencies. As you know, we bring a number of blacks, other minorities and Caucasian admittees who have such deficiencies, to the campus in the summer for six weeks. Their deficiencies are a long way toward being corrected during that very intense summer experience so that by the time school starts in the fall, these students are off to a running start.

The Special Student Program has, I think, a remarkable record in terms of the proportion of people it has brought here who not only graduate but do very well here. And we can be very proud of what they do after graduation as well. Of course, not all our black students need that kind of special treatment. One of the best students I ever had was Price Walker. His father had been in the military and he'd been at different kinds of schools. Price was able to compete for admission successfully here with anyone, and to do as well as anyone as an undergraduate. He's now a pediatric oncologist, having gone through medical school and taken up that specialty. And there are many examples like that.

PRUD'HOMME: How have the students differed decade by decade? Have the students changed that much at Caltech?

OWEN: I don't think they've changed as human beings. I would make two kinds of distinctions. One is that Caltech students are statistically different from the general run of students elsewhere. There are students who are certainly equal, and like them, in many different schools across the country; but statistically, since we can be so very selective and we select to such a relatively homogeneous standard, our students are exceptionally able, generally very much interested in intellectual matters, on the whole well directed toward careers in science or engineering primarily, but able to do all kinds of other things as well—music and literature and sports, all kinds of things. So I don't think that the population of Caltech students ever—in the sixties, for example, when there was a great deal of unrest elsewhere—showed the kind of activism and concern that you saw at Berkeley or Stanford or other less homogeneous university populations. Though within the Caltech population, I guess you could see behavioral changes that reflected the fact that they were part of their generation.

The period after the Second World War was much affected by the returning veterans, who were older, more experienced, more mature, more specifically directed. We had a wonderful set of students in the late forties, early fifties. The students in the fifties were fairly serious; of course, they had their pranks and their fun. In the sixties—as student discontent, concern with civil rights and things like that, and with the curriculum, cropped up in other universities—our students were also more outer-directed, I guess you would say, than they had been in the fifties. But their concerns were expressed differently than in many institutions elsewhere. There were no riots or window-breaking or anything like that. But they set up the undergraduate research program and so on, getting involved in community and national problems more than they had prior to that time. In the seventies, I think as a group, they became much more concerned about the possibility of making a living when they got out. They became more self-centeredly career-directed than concerned about problems in society—although the period when I was dean, '75 to '80, was a time when we had the first student/faculty conference and talked about aspects of student life on campus, and a good many reforms were initiated then, too.

PRUD'HOMME: What was the undergraduate research program?

OWEN: That was a student-designed, initiated, and operated affair, in large part under the impetus of the president of the student body, Joe Rhodes, who was a good political figure. He got the idea, with some faculty support, of getting grants from external agencies to conduct research by our students on such things as transportation problems in Southern California, using computer-based schemes for maximizing the efficiency of ride-sharing, and things like that.

They got this program funded, and it involved quite a number of students—as I say, a very constructive kind of social concern.

PRUD'HOMME: What was the Committee for Independent Studies?

OWEN: One of the things that, as a kind of curriculum enrichment, was discussed and finally acted on was making it possible for students to meet degree requirements here without necessarily having a formal major in one of our standard options. Our students get degrees in geology or biology or physics or chemistry or engineering; and there were some undergraduates who felt that they wanted to sample more broadly and have a less rigidly defined curriculum. They wanted to be able to take some courses in a sort of custom-tailored way so that they could set their objectives for themselves and do a certain amount of independent study under the aegis of a member of the faculty. That program was set up to accommodate such students, and there was a time when a fair number of students chose to do independent studies. I can think of a number of examples of students who were saved by the program—students who couldn't bring themselves to meet the routine requirements of degree work. Given independence and a chance to set their own goals, their own contracts, and then work toward meeting them—often including work in a research laboratory—they flowered.

PRUD'HOMME: They'd lose all that competitive pressure.

OWEN: Yes. Although I think, especially since the installation of pass/fail grading first for freshman, the predominant environment among our students has not been competitive but rather cooperative. Independent study reduced their feeling of being pressed into a mold. And in those days there was a lot of concern—more elsewhere than at Caltech—about personal individuality, and about being treated as a number. In recent years, the independent studies program has been less popular, so that now the faculty committee on independent studies is reduced to one person, who is a joint appointment between biology and engineering and applied science—Derek Fender. My impression is that after the curriculum was loosened up, it became easier for a student who wanted to pursue interdisciplinary studies to do that within the confines of the option requirements by making proper use of the electives. A student could major in physics, say, but still study a fair amount of biology. My impression is that now it's rarely the case that a student does independent study. It's still in our catalogue, though.

PRUD'HOMME: I want to go back a bit and discuss the Nobel Prize. When the Nobel Prize in medicine was won by Peter Medawar and F. Macfarlane Burnet in 1960 for their work on immunological tolerance, various people felt that you were treated unfairly.

OWEN: Yes—this is a subject I don't like to talk about because it's so easily misunderstood. So I'll tell you what it looked like from my point of view. First of all, the prize was given in 1960 for discoveries related to immunological tolerance. This was an area in which the work with twin cattle that I did in Wisconsin in the early 1940s—about which I told you earlier—and some of my later work, were key. The subject had been expanded experimentally, and very considerably, by Peter Medawar and his people at London, with who I was in very close interaction in the early 1950s. They were able, using skin grafting in mice, to show that one could induce immunological tolerance. In the late 1950s, I began—and this is what I don't like to talk about much—I began to get indications that a Nobel Prize might be given in this area. I got a request from the Swedish Academy for my reprints. I was very self-conscious about that. In the first place, I'm not keen on prizes, especially the kind of adulation that goes along with the allocation of a Nobel Prize. I didn't really think that my contribution merited that kind of treatment. I didn't, frankly, respond to this request for my reprints; I wrote a note back saying that most of my supply of reprints had been exhausted. I never made the kind of effort to be included in the Nobel Prize that people do. You know—if you want to get a Nobel Prize you go around and give a lot of talks and publish a lot of papers and call attention to your contributions, and you keep at it. That had never been my style. I felt about the prize, I guess, probably somewhat the way [Richard P.] Feynman reports feeling about it, except that I felt that way about it beforehand. In short, I didn't want it. I suppose this is not normal, and I'm not proud of it, it's just that I didn't feel I needed or deserved or wanted that kind of honor.

When the prize was given to Medawar and Burnet, there was a certain amount of concern expressed by some people; and I must admit that it's nice to feel that some people think you should have got it—it would have been bad to get it and have people say, "How did that guy get it?" There was an article in one of the French newspapers by some people who knew about my work and complained. The French felt left out of Nobel Prizes in those days and had some sympathy for people they thought deserved one. I got a nice letter from Peter Medawar, too, which I guess is in the [Caltech] Archives, saying that he just couldn't understand what he had done or might have done to have this happen. But I didn't feel bad about it at all—I felt relieved. I thought that both Medawar and Burnet deserved this kind of recognition. Medawar and his people had put immunological tolerance on a strong experimental base. Burnet had not made primary contributions of an experimental nature in this area, but he wrote about it extremely well and very clearly and enthusiastically; and altogether, over his career in Australia, he had made many very important contributions to biology and immunology. So I thought it was fine that they got the Nobel Prize, and I let it go at that.

PRUD'HOMME: And you didn't want it in the first place, anyway.

OWEN: No. And that's what people can hardly believe. There have been three or four occasions, more recently, when I've been embarrassed by these references as to whether or not I should have received the Nobel Prize—and especially because some people got the history mixed up. So one would find textbooks coming out saying that Burnet had predicted the phenomenon of immunological tolerance, when in fact, when he wrote about it in 1949, it was five years after my work with twin cattle; and he recognized that in the little book that he wrote with [Frank] Fenner, called *The Production of Antibodies* [1949]. But some people dealing with history got it screwed up and put things in less than a factual order. And so, of course, some of my friends, like Irv Weissman up at Stanford, decided to try to set this straight and put things in their proper order. And I urged them not to bring up this old issue, about whether or not I should have received the prize. The real history was straightforwardly presented by Peter Medawar, in his Nobel Prize lecture. Medawar and I have always been on very good and mutually supportive terms. So as far as I'm concerned, this Nobel Prize business is a tempest in a teapot—better not talked about than exaggerated into some big mistake the Nobel committee made, or some injustice that was done me.

PRUD'HOMME: Of course, the publicity is enormous, so you lose your privacy.

OWEN: Yes. You can see that happening with Barbara McClintock. Even just this morning, in the *Pasadena Star News*, and yesterday in the *Los Angeles Times*, they're trying to build her up as something different from what she really is, or was. I'm sure she must be uncomfortable with that. And it happens time after time to a lot of people. Now, somebody like Willy Fowler or Max Delbrück can take this kind of thing in stride. Willy had a good time with it and he greatly deserved it. And I think Max handled it very well. But it's artificial, I think, to pick out one or two or three people a year and subject them to this great public adulation when in fact they were plugging along doing some interesting things, and they deserve recognition for it. But there are also thousands of other people doing interesting things and enjoying their contributions and the rewards you get in feeling you've done something interesting, important, and useful, or affected the lives of other people positively. Those are much more substantial rewards, I think, than being given all this publicity, or even all this money, for that matter.

Begin Tape 4, Side 2

PRUD'HOMME: I'd like you to describe some of your colleagues, any other people that you think might be interesting. Can you give any sort of human insights?

OWEN: I doubt that I have anything unique to say. I've been associated with many people over the years. Ernest Swift was chairman of the chemistry division at the time I became chairman of biology, and was a real scholar and a gentleman, very helpful to me and played an important part in the history of the institute. Bob Bacher was chairman of physics when I became chairman of biology, later became provost, and contributed a lot. I can't offhand, in response to your question and without thinking about it, contribute a lot of insight. Linus Pauling perhaps was the single individual out of whose association I got most.

PRUD'HOMME: In what sense?

OWEN: Well, first, I guess, as a scientist—his interest in molecular medicine going back to the sickle-cell hemoglobin work—his serving as chairman of the Division of Chemistry and Chemical Engineering back when I first came here; and his interactions with Beadle. Linus has always been, still is, an extraordinarily creative person, wide-ranging in his interests, thoughtful about things, open to other people's ideas and idiosyncracies—and with some idiosyncracies of his own. Sorry to let you down on this.

PRUD'HOMME: Can you give me your impressions of the various presidents of the institute?

OWEN: Lee DuBridge I came to know first as a young professor on the faculty. He came here the same year I did; and then I knew him as president over the interval when I was chairman of the biology division. Lee was always a very warm personality, interested in all kinds of things, an easy communicator, devoted to the excellence of the institution. What I saw in Lee—and Doris, his wife who died of cancer some time later—was an interesting transition, I think, an example of what the demands of being Caltech's president lead to. It became evident, after Lee had been president for a while, that the demands of seeing that this institution grow and be funded, relationships with people like trustees and associates and foundations and funding sources, took Lee's energy and attention away from the internal aspects of the institute much more than he would have liked.

PRUD'HOMME: It was such a tremendous growth period.

OWEN: Yes. When you talked to the DuBridge of the late 1950s, as compared with talking to the DuBridge of the late 1940s, you saw how much both he and Doris had to be concerned with prospective donors and trustees and things like that. So that by the time Bob Bacher became provost, in 1962 or so, the presidency at Caltech had been defined as largely an office that looked outward from the institute to interfaces outside. And the internal operations—the academic, institutional things—became much more the territory of the provost. And Bacher and DuBridge worked extremely well with that kind of division. Bacher kept a close check on what went on and was in very close touch with the divisional chairmen. Not that Lee stopped being interested or following what was going on, but he didn't have as much time or attention for it as he had at the start. The division chairmen's meetings in DuBridge's office over that interval I've described briefly; it was an expanding time, and the feeling was that there were essentially no limits, except for those we wanted to impose on ourselves. The chairmen were mutually supportive, and Lee had his finger on things, but most of the internal, academic affairs were handled very competently by Bob Bacher.

After Lee left to become science advisor to President Nixon, we went through a brief hurried interval trying to figure out who should be the successor; and Harold Brown was chosen. I was no longer division chairman at that time, but I got to know Harold. And then in 1975, I accepted an appointment as vice president for student affairs, a new position, and dean of students. So I was brought back into regular interaction with the president's office and with the provost, who by that time was Bob Christy.

Harold Brown was a very different personality from Lee—quite reserved, very concise in his expressions of things, on the surface rather cold, I guess. I found him a good person to work with. For one thing, he was very bright, very quick to pick up cues. He was well organized. And he wanted to talk about things; if you had a problem, he was immediately available for conversation. If you went prepared, you could handle almost anything in a few minutes. If you presented him something in a concise and organized way, he gave a concise and organized answer. And he seemed to pick things up very rapidly. My association with Harold was especially good, I think, because since he was rather shy and not easily approachable by the students, I think he rather felt it would be good to be shielded from them. He didn't know quite how to interact with them. If a delegation of students came to him with an idea, he treated them just as I described his treatment of me—he was quick to perceive and he was responsive. But he wasn't very comfortable with it. When he was being considered for appointment to the presidency here, I was told by Bob Sharp that one thing Harold wanted to find out was whether he was likely to encounter the kind of student unrest that he thought had made things miserable at Berkeley, for example. And he had to be reassured about that. In any case, he turned over to me, as the new vice president for student affairs and dean of students, most of the real responsibilities for student programs and interaction with students. And, of course, that was what I liked to do. He was supportive, and he listened and responded. I found working with Harold very rewarding. I suspect this image of him, though, prevailed in the faculty community in general; they felt he was a considerably less warm person than Lee DuBridge—or than Murph [Marvin L. Goldberger], when he came.

When Harold left to become Secretary of Defense, we went through another interval of picking a new president while Bob Christy acted as acting president. I was not involved in that search. There was a committee on which the biology representative was Lee [Leroy E.] Hood.

And Goldberger was, as you know, chosen to be president. Murph, again, is a different kettle of fish from either DuBridge or Brown. With regard to my position as vice president of student affairs and dean of students, I found working with Murph less satisfying than working with Harold. Part of it was that Murph himself wanted to be right in there on the front line of student interactions and things like that. He felt experienced and competent in those areas, and was not about to think of his vice president for student affairs as an intermediary in such matters. Then, too, I wasn't really Murph's man—I don't mean that we didn't get along, or disagreed, or anything like that, but when he came there was an establishment here that he inherited from the preceding administration: the division chairmen; the vice president for development of community relations; Bill Corcoran, the development operation; Bob Christy, who had been acting president and continued as provost. You could see that Murph would feel more comfortable with having his own appointees in these positions.

It was an easy transition for me, because I had agreed with Harold to serve as vice president for student affairs and dean of students for a maximum of five years. I served until the end of my term. Furthermore, it was my sixty-fifth birthday in 1980, conveniently at the end of that five-year term; so it was very easy and comfortable to move out of that job. And I was very pleased that Murph chose Jim Morgan as my successor as vice president for student affairs. Jim Morgan had been dean of students before me, and I knew he would do a good job. Also, David Wales, who had been associate dean of students with me, became dean of students—they divided my two jobs. And these choices were very good ones as far as I was concerned.

PRUD'HOMME: In 1968, you retired as chairman of biology. What were your intentions then? What did you wish to do?

OWEN: I might make a comment or two about how that happened. I had been chairman for—counting my period as acting chairman—seven years. And it had gone well, but I began to be a little concerned that I had begun to think of my colleagues as problems because over a period like that you accumulate things, you know. You spend your capital. It seemed to me about time for me to move on out of that job. By then, there had been a nearly complete turnover of the division chairmen; Bob Sharp was the only one of the initial ones that was left. And one day Bob Sinsheimer came into my office and said he had been offered a chairmanship at the

University of Colorado and he was thinking of taking it. I said, “If you want to be a chairman, why not stay here?” He thought that would be a good idea; he’d be interested in it. Other members of our faculty recognized that I had kind of served my term.

PRUD’HOMME: That was very generous of you, though.

OWEN: No, it was, first of all, a very graceful way out of a position in which I thought I’d about done my part. And I’ve always felt that in a position like that your final big obligation is to see that there is a successor who will do the job well, and who is a good choice as far as colleagues are concerned, and Bob was available for it. I was also afraid it wouldn’t be good to lose Bob. He and [Arthur] Kornberg up at Stanford had just recently done the key experiment of getting DNA to replicate itself in the test tube. This had attracted a good deal of attention; and Bob, in biophysics and molecular biology, was our strongest and best-known person. I hated to think of his going off to Colorado to be a chairman there if he would be pleased to stay here and succeed me, which he was. And that worked out very well, with a short lame-duck period, when Bob began to take over the responsibilities—including, as I mentioned, recruitment of Jim and Marianne Olds to our psychology staff. One of the nicest trivial things that happened was that Bob came in one day, looked at the chairman’s office, and said that he was going to get new furniture for it. He didn’t like this antique stuff I had in my office. And one of the things was this big desk, which had belonged to Millikan and which I had inherited from Beadle. So the only thing I was really sorry to leave I was able to move down here. Bob took over, and we interacted well, and the transition went very smoothly.

PRUD’HOMME: You had a party, too.

OWEN: Yes. They had a wonderfully nice party, celebrating my years as chairman and Bob’s succession to the chairmanship. It was called “All the King’s Men.” It was a musical written primarily by one of our graduate students, Sandy [Sandra] Winicur, who had great abilities along that line. I have a record of the musical. One of the nicest things about it was that they gave me a sundial that had been constructed in the Palomar shops and could keep time by being properly set at the solstices—deviations from correct time aren’t larger than two minutes all through the year; a beautiful thing and a lovely piece of sculpture in its own right—and Max Delbrück made the presentation. The reverse side of that record I mentioned has Max's lecture on sundials and how they're constructed and how they operate. It was a very warm celebration. Bill Wood, who is a widely known folksong guitarist, a young member of the faculty whose appointment occurred while I was chairman, played the part of King Ray, and his princes were people playing the parts of members of our faculty. It was a very nice time.

So the sixties, when I was chairman and took on those external jobs for the NIH and the NSF and so on, keeping my research program going mainly by having Bobby Sanders here, that was one phase of my career. And I was looking forward to the next phase, in which I would stop running back and forth to Washington and instead reestablish an active, ongoing research program with a new set of young people. I got five new graduate students all at once. I started teaching Biology 2 and continued to teach the immunology course. And until 1972 or so, I stayed here, and we had a wonderful time in the lab. I think I've mentioned some of the young people who were here then as graduate students and have done well since. We had a lot of different things going on. It was a lively and interesting time, and I was enjoying being a professor again, doing research and teaching right here on campus.

PRUD'HOMME: What were you doing your research on?

OWEN: Well, as I said, our program was pretty varied. Jeff Frelinger was working on a substance in the serum of pigeons called transferrin, which was also represented very widely in many other different kinds of animals. This substance carries iron around and binds iron very tightly. A puzzle about the transferrins is that there's a great deal of inherited polymorphism—variation—in all kinds of different species and no explanation of why so many individual differences should be maintained in such a wide sample of living forms. Since we knew that this wide variation occurred in pigeons, Jeff set up a procedure for finding out why that should be—why different forms of this gene were maintained in the pigeon population. He used wild pigeons trapped up on the roof. Tommy Douglas was working on a marker for a particular kind of lymphocyte deriving from the thymus, the so-called Thy-1, and found especially in rats that same specificity in the brain and studied its development. Libby Blankenhorn started working on wild mouse populations that varied in their incidence of leukemia, to find out why there were these variations in the formation of this kind of cancer. Joan Klotz was working as a postdoc on a particular compound that was highly active in inducing mutations in the gene, or a molecule that substitutes for serum albumin in the course of embryonic development. Sue Melvin was working on lymphocyte populations that arose in the development of the immune response in individual mice. Sue Ostrand-Rosenberg was working on molecules on the surface of red cells that could be identified with antibody reagents, working on how they were distributed on the cell surface and whether they were linked or independent of each other genetically. So we had all kinds of different things going on. It wasn't a highly focused program. We were very well supported. I might mention that having been responsible for the biology division during the sixties and much involved in the fund-granting activities of the NIH, I had fairly early resolved not to depend on the NIH for my own research support. I felt that I could seek funds for the division but that there was a potential conflict of interest, because I was making judgments about fund allocations to people who were applying for funds, wearing my one hat; and I thought it would be better if I got my own research funding from another agency. I did seek funds for the division—for instrumentation and general purpose grants—quite successfully. My own research had been supported very generously by the Atomic Energy Commission, going back to my time at Oak Ridge and my interest in things related to radiation effects and their treatment. And as the AEC went through its successive manifestations and became a part of the Department of Energy, my research funds continued to come from that agency, which relieved me of the embarrassment of going back to the NIH for funding. But we were very well supported, and it was the kind of support that didn't involve a lot of time spent on grant applications and reports and so on. I'm grateful to the Atomic Energy Commission and its successive incarnations for supporting my research until I decided not to seek any more support at all.

PRUD'HOMME: This was the period when you were on [President Nixon's] cancer panel?

OWEN: That began the next phase of my life. It overlapped the period when I still had these good active young people and a good research program. The National Cancer Program was sort of a political device established by some enthusiasts—Mary Lasker played an important part in it—to try and get increased funding for cancer research. It was established in the early 1970s. It had been promoted by President Nixon—I think maybe largely, as he saw it, for political purposes. There had been a general shrinkage in the rate of growth and support of biomedical research, and this seemed to be a way of getting more money for research—focusing explicitly on the compelling problem of cancer. The program began at a level of funding of around four hundred million dollars a year, as I remember it. It was not a popular program with biomedical scientists. In my own judgment—and I haven't heard this talked about much in this context—biomedical scientists who were routinely supported in their research enterprise by the National Institutes of Health had become habitually identified with particular institutes. You see, the NIH, except for the National Institute of General Medical Sciences, was organized by categories—the National Heart Institute, the National Eye Institute, the National Institute of Allergy and Infectious Diseases, the National Institute of Arthritis and Metabolic Diseases. People in biomedical disciplines had begun to identify themselves with particular institutes. In my own field of immunology, for example, there was the National Institute of Allergy and Infectious Diseases. Or in connection with my interests in genetics, it was the National Institute of General Medical Sciences, which included genetics as part of its empire.

Now along comes one of those institutes, the National Cancer Institute, and makes a big public deal with Congress for funding. And I think there was a good deal of apprehension on the part of biomedical scientists that more money would be going to cancer and the National Cancer Institute, and that their interests would correspondingly suffer. It didn't work out that way, although I think very few people outside realized it—and I've tried to emphasize this point when I have the chance. We'd gone through, as I said, an interval when support for research had been dropping for all the institutes, including cancer. With the establishment of the National Cancer Program, cancer moved up. And it moved up in ways that permitted the funding of other work which would otherwise not have been funded through these other institutes. For example, immunology is an important part of cancer research. Some of the immunologists were afraid they were no longer going to get support from the National Institute of Allergy and Infectious Diseases—but in fact a good deal of money became available through the National Cancer Program for research in immunology. Altogether, I think the National Cancer Program had a very salutary effect on research funding. It reversed that declining trend. Although still, it's looked back on—the program is still in progress; but it's looked back on as having been an objectionable way to get funding. Part of that attitude was because laymen began talking about "solving the cancer problem" in the same way as we put a man on the moon, not recognizing that what was really needed was a lot more basic knowledge about living organisms and how they function, and what cancer really was, and what it came from. The people who directed the program knew that, but what was said in public, about the so-called "War on Cancer," didn't sound that way. Also, the program was attacked as being too highly targeted. In order to justify these funds to the president and to Congress, the bureaucracy had to set up a set of goals and a set of statements about how we were going to approach these goals, in a kind of systems approach to the subject. It looked as if the freedom of ordinary scientific inquiry was being sacrificed to this kind of phony progress. The fact is, the way the funds were actually spent and allocated largely promoted free-ranging basic research. But there's still that feeling on the part of the scientific community that there was something wrong with it.

The responsibility for the program was set up differently, too. The National Cancer Institute was not just given the funds to run it, like the other National Institutes of Health, but had a special body set up to guide it, interposed between the president and the actual operation. There were three people on this guiding body: the chairman was a lawyer and investment banker from New York City named Benno Schmidt; then there was a person with a primarily clinical orientation—Lee Clark, who was director of the M. D. Anderson Hospital & Tumor Institute, in Houston; and the third member was a scientist. The first scientist appointed was Robert Good, but in his first year of service he changed jobs—he left Minnesota and went to Sloan-Kettering as director of research. Benno Schmidt was also connected with Sloan-Kettering, so in order that there wouldn't be an untenable concentration on one institution, Bob resigned, and then I was appointed. So I got into the National Cancer Program early, but not at the very start.

I found Benno Schmidt just a remarkable chairman. He had great insight into the best ways of supporting basic research, and his devotion to it and his effectiveness were far greater than many of the scientists who were talking about it. He was a wonderfully well-organized person, courageous and able to express himself in a wide variety of contexts exceptionally well, to react quickly in conditions of free give and take. This was a three-year term, and I served on the National Cancer Program for three years, from 1972 to 1975. Benno continued as chairman for an interval. Lee Clark was replaced by another clinically oriented person. There was a replacement for me. And now there's a new chairman, and I'm not sure just how the program is going. Armand Hammer is now chairman of it, and he is a different person from Benno Schmidt. But it was a very interesting experience, because while I was on the panel, our funding rose from $400 million to $500 million to $700 million; and it was a wide-ranging program. We had to set up programs sometimes on line-item bases, for improving the clinical interface at the patient care level. We were involved in setting up comprehensive cancer centers, a dozen or more of them around the country, designed to bring scientists and clinicians into good communication with each other. The people who were interested in viruses were gung-ho that virology was the way to study cancer. That was unpopular at the time, and we were criticized for spending five million dollars on that program, although since then it's proved to be a good investment. All through my period of service I felt unpopular with some of my colleagues just by virtue of the fact that I was connected with the National Cancer Program. But I learned a lot from it. I think I contributed. It's one phase of my career that I look back on with pleasure and some pride.

It was a very time-consuming job, however. We met monthly to guide the large budget. We had to meet with the National Cancer Advisory Board, and we had regular conferences with President Nixon. So I let my research drop, with the group of young people I've mentioned, and never in fact picked it up again. By 1975, after I finished my term on the cancer panel—I refused reappointment—I had decided to undertake a new phase in my career, in which I'd stay home and spend my time and energies in substantial part with our undergraduate students. While I was still on the cancer panel, Harold Brown asked me if I'd become vice president for student affairs. So I'd finish out that term, and the two jobs would overlap.

PRUD'HOMME: You said you met President Nixon. What was your impression of him? You said he was basically not very interested in science.

OWEN: I think he thought of the National Cancer Program primarily in political terms. It sounded like a good thing politically.

RAY DAVID OWEN

SESSION 5

November 14, 1983

Begin Tape 5, Side 1

PRUD'HOMME: We were talking about your work in Washington before. And you served on the president's cancer panel. Can you tell me a little bit more about that?

OWEN: Yes, I think I indicated who my colleagues on the panel were. Our obligation was to oversee this very large program and report to the president. And the president at that time was Richard Nixon.

PRUD'HOMME: Did you actually report to Nixon?

OWEN: Yes. We saw him at monthly intervals in the Oval Office and talked for twenty, twenty-five minutes with him about aspects of the program—how it was going, and he asked questions about it. I'm not a Nixon supporter politically, but I came to have some respect for him, at least in terms of the quickness of his intellect in this context. He was obviously not trained in biomedical science—far from it—but I remember occasions when we would spend fifteen or twenty minutes talking with him about cancer-related issues. On one particular occasion—it was National Cancer Week or something like that—after talking with him we went immediately into the Cabinet Room, next to the Oval Office, and TV people were there. And he sat at the table and made a statement, off the cuff, in which he very intelligently and competently and in his own words repeated some of the things we'd been saying to him. He had understood, and he put it all into a good framework. That was at a time when, we later found, he must have been very troubled; it wasn't long after that that he left office and we were supposed to report to Gerald Ford. That was very different—in the sense that Ford never found time to see us. I don't think he thought of the National Cancer Program as something of his own, and he had other things to be concerned about. So we never did talk with President Ford. I left the cancer panel then, after a single term, because I wanted to return to Caltech and spend a few years and a major part of my time and energy mainly in the interest of undergraduate students here.

PRUD'HOMME: It was in '75 that Harold Brown appointed you as dean of students and vice president for student affairs. That was a new position, wasn't it?

OWEN: The position of vice president was new, yes. The student-affairs operation includes more here than elsewhere; it relates to the activities of the dean and the registrar and the master of student houses and the health center. A great variety of things fall into that operation—almost everything that relates directly to undergraduate life on campus except for the curriculum, which is under the aegis of the faculty. There had been, every ten years, a review of the student-affairs operation with proposals for change and improvement. The latest review, in the late 1960s, I believe it was, under DuBridge's aegis, had led to the appointment of Lyman Bonner as—I believe he was called—director of student relations.

PRUD'HOMME: What was your function?

OWEN: Well, there had been another review, then, by a faculty committee, under Fred Anson's chairmanship, which had uncovered some problems relative to the morale of the student-affairs staff and also student concerns with various aspects of campus life. And Harold asked me whether I would be willing to take on a new position, that of vice president for student affairs, and also serve as dean of students. Jim Morgan, who had been dean for the preceding three years, didn't want to continue so that position was open. And Harold thought that combining the deanship with this new post of vice president for student affairs would accomplish change in the undergraduate student-related operation. This didn't include graduate students, by the way; there was a separate graduate office and a separate dean of graduate studies—although one of the hopes expressed by the Anson review committee had been that eventually student affairs would all be under one umbrella. In fact, that's been done, because now Jim Morgan is not only vice president for student affairs but also at least acting dean of graduate studies.

I guess that my responsibilities included almost everything that related to the life of undergraduates on campus. It was challenging and frustrating in some ways, but very rewarding. I accepted the position of vice president for student affairs in January 1975, but took up the deanship in August. In that interval, we had had two undergraduate suicides; one was in the spring, one in the summer. And then the first thing I encountered, within the first month of becoming dean of students, was a third undergraduate suicide. We were, of course, concerned as to why this should have happened and why there should have been a cluster of suicides so close in time. We investigated but found no common factor associated with their life at Caltech; they were all successful students; there was no indication of drug or alcohol abuse or anything like that in their records. The one who committed suicide just after I took office was regarded as a very successful student, had been designated the sophomore in chem engineering most likely to succeed, or something like that. About the only common variable we could identify in the three cases is that they were all rather lonely people. Not that it showed much on the surface of their lives, but when we checked afterward we found that none of them had anyone he could call a close friend. One of the boys—this was a kind of pathetic discovery afterward—used to work hard and long hours in the laboratory; and for refreshment he would get on a city bus and spend the night riding the metropolitan bus system, using transfers, all by himself.

PRUD'HOMME: Was there no counseling service?

OWEN: Yes, there were counselors in the health center. One of the things that we did in response to these tragic incidents was to give a good deal more attention to counseling as it related to possible and potential suicide. I got a lot of help on this from Jim Mayer, who took office as master of student houses at the same time I became dean. Jim was a very humane and approachable person, concerned about counseling, including peer counseling within the houses. And we got expert opinions about how to deal with this. It's a remarkable thing that during the five years that I was dean, there were no further undergraduate suicides, and I'm still not sure why those three happened together or why there were no more over the five-year period. I suspect it's just an accident of statistics.

PRUD'HOMME: You're an extremely approachable person. Just being here talking to you, I see the students come in and out. They have great freedom of access.

OWEN: That was one thing we tried to accomplish; the students were in and out of the dean's office all the time, talking about personal as well as academic problems. The counselors in the health center, the peer counselors, who were students themselves, and David Wales, my associate dean of students, who later succeeded me as dean, were all active in this. Jim Mayer was very active. So we did try to establish a lot more accessibility for students with problems, and to promote constructive, creative student enthusiasms as well.

I also undertook to improve the morale of the student-affairs staff as a whole. One of the problems that had come out in the overview of the Anson committee was that the student-affairs operation was broken up into separate offices with different responsibilities and relatively little communication among them. Sometimes they would see themselves as competitors rather than as collaborators in a common enterprise. So I started having regular staff meetings, with agendas, and spent some time writing up the results of these meetings in the form of minutes, and trying to develop an attitude on the part of everyone who touched the lives of students that we had common interests and concerns and could help and support each other in these. This was also a time when there was a lot of student interest and concern about making life better for students on campus. We had a president of the student body, Ray Beausoleil, a very active young man, who with others and with our collaboration set up the first faculty-student conference, out at JPL, which turned out to be a great success. Faculty members came and heard things from students that they had not suspected before, about aspects of student life.

PRUD'HOMME: It's interesting that this didn't happen in the sixties, but in the seventies.

OWEN: The issues weren't quite the same as in the sixties, which was substantially concerned with problems of the larger society outside—although, of course, it included the expression of student dissatisfaction with aspects of campus life. But our students were largely trying to make it clear what it was like to be an undergraduate at Caltech, what some of the problems were, and to enlist the support of sympathetic faculty in improving things: the academic load that was placed on the students; the physical condition of the houses—the houses had fallen into great disrepair, and we had to get money for rehabilitation, especially of the older houses—the contract for the food service in the houses had to be given some attention. All kinds of things like that.

PRUD'HOMME: Did you learn anything new from this? Were there any surprises?

OWEN: In my case I would say, probably no—because by then I had delved pretty deeply into the life of the students. But for many of the members of the faculty who served on student-related faculty committees and so on, I think there were a good many eye-openers. We tried to put feedback on a more solid basis—feelings about courses and teaching, and things like that. I have always emphasized the importance of the teaching-quality feedback reports, for example.

PRUD'HOMME: Do undergraduate students ever feel that the graduate students get most of the attention? Is there any resentment?

OWEN: No, I don't think so. The two populations of students are separate in some respects. The life of an undergraduate is different than that of a grad student, and they don't mix very much. There are attitudes that, partly I think, are assumed just out of a sense that it's funny, but sometimes feelings are hurt. Undergraduates sometimes refer to the grad students as "grad turkeys," and consider themselves intellectually and otherwise superior as a population. Grad students who are working hard as teaching assistants resent being referred to as turkeys. So there's a certain amount, I guess, of lack of sympathetic communication between the two populations. But they do overlap, and a good many of our undergraduates, especially in the junior and senior year, take classes in which graduate students are also members. They're working in research laboratories, perhaps even shoulder to shoulder under the immediate supervision of an advanced graduate student. In some ways, many of our undergraduates take up a kind of graduate life, in essence, while they're still undergraduates.

PRUD'HOMME: Is there any feedback if a teaching assistant is bad?

OWEN: One of the discontents that was expressed early, and which we tried to do something about by improving the feedback machinery, was that people were assigned to teaching assistantships who were not competent to teach. There were horrible examples of people assigned to sections in important undergraduate courses who couldn't speak or understand English, for example. I've felt for a long time—and the undergraduates in general do, too—that graduate students make a useful contribution to undergraduate instruction if they do it well. From the institutional point of view, it's a way of supporting graduate students and getting teaching done cheaply. But the graduate students who teach should be our very best ones and they should be rewarded both in terms of status and other kinds of rewards for performing this most important and most demanding kind of service. There's been a tendency over the years for the best graduate students—the best intellectually—to leave aside teaching responsibilities in favor of research assistantships, because they can do their thesis research and get paid at the same time. Although more of the really competent TA's are doing important teaching jobs now, I don't think we've yet solved the matter of seeing that the very best ones are recognized and rewarded for doing it.

PRUD'HOMME: Because you can be a wonderful scholar and a dreadful teacher.

OWEN: Yes, that's right. That holds for faculty as well [laughter], not just students, for that matter. From the graduate students' point of view, if they are given a constructive opportunity to teach, it's good for them, too, because many of them will end up in academic jobs elsewhere. In courses I've taught over the years in biology, I've tried to see that the TA's who have worked with me got practice and experience in more than just paper grading or routine laboratory preparations and instruction, so they could emphasize that variety when they applied for academic jobs later in their careers.

PRUD'HOMME: So you really coordinated many different activities in these jobs.

OWEN: It's a pretty far-flung operation. I've only mentioned a few of the several different offices, with a good many people working. The Placement Office was not under my aegis—it was at that time part of Bill Corcoran's, as vice president for institute relations; we had many very warm interactions with the people in Placement. And now I'm pleased to know that it is under the aegis of Morgan as vice president of student affairs.

PRUD'HOMME: Did admissions come under you?

OWEN: Yes, admissions, freshman admissions, upper-class admissions. Financial aid was an area of ferment at that time. In earlier years, we had enough money so that we could say in our bulletins that no one should hesitate for financial reasons to come to Caltech as an undergraduate. We would meet everyone's need; we had the assets to do it. But as expenses went up, and as institute endowment for that purpose didn't rise correspondingly, we had to go more and more to loans rather than gifts, and to federally related programs, which involved a new order of people, with careful planning and bookkeeping and accounting. Bill Schaefer was registrar at that time, and one of my first actions was to get him his own enterprise as director for financial aid. Schaefer contributed a great deal in that regard; it got to be a very big job. We hired a woman, Ursula Hyman, who was a professional in the field of financial aid. And then when the chemistry division stole Schaefer away from me—he was a chemist and they wanted his help in the management of laboratories over there, and offered him more money than I could pay him in Student Affairs—I made Lyman Bonner the registrar and made Ursula Hyman director of financial aid. And she set up a very competent, businesslike financial-aid operation, became a national figure in the field of financial aid. She left then, to go to law school. She's recently completed her law training and has joined a firm in Los Angeles, but she helped in choosing her replacement—another very active, able, enthusiastic young woman, named Linda Berkshire. So we managed to set up a responsible and effective financial-aid operation. Those were the kind of administrative things I worried about over that period.

PRUD'HOMME: But you were still dean of students all this time.

OWEN: Yes, I was still dean of students. By the way, all of this was supposed to be a half-time job for me; I continued as a professor of biology. And I made a point of getting over here [Kerckhoff]. I originally tried to spend my mornings in the dean's office and my afternoons here and shortly found I couldn't do it that way. But I got over here a couple of hours a day and continued to do some teaching in the biology division, including especially my freshman class, which I enjoyed so much.

PRUD'HOMME: What problems continue currently in your work?

OWEN: Well, you know this period we've been talking about ended with my completing my term of service in 1980, so I've had a three-year interim now before reaching the status of being a professor emeritus. I became in the interval the undergraduate advisor for the biology majors, setting up a carefully monitored advisor's system to select faculty advisors for the students, developing the Biology Undergraduate Student Advisory Council [BUSAC], which turned out to be a very good idea, and it has functioned very well.

PRUD'HOMME: Can you tell me more about that?

OWEN: I thought it would be good to have a centralized and responsible student-owned-and-operated organization able to represent the concerns of biology students to the faculty, essentially. The way we set about that was to make it an elected body, inviting students to declare their candidacy to be one of the two representatives for their class—sophomores, juniors, seniors—in a contested election, in which there developed a great deal of interest.

PRUD'HOMME: That would be six students from within the biology division?

OWEN: Yes, that's right. Actually, in the first election, three rather than two seniors were tightly clustered in the final ballot, and I thought we could use three seniors. And the same thing happened in the junior class. For the first year, we had two elected sophomores, three elected juniors, and three elected seniors. And then, knowing the freshman interest in biology from my freshman class, and consulting the undergraduate TA in that course, the council itself designated two frosh to be members. So there were actually ten undergraduate members of the council. We met at the beginning of each term at lunch at the Athenaeum, and again just after midterm, and again just before finals. The BUSAC—the Biology Undergraduate Student Advisory Council—provided ombudsmen for all of the classes, one ombudsman who was a student in the class and another outside the class. They got out an annual—a very nice publication, useful for students considering courses in biology—listing the courses that were offered and making straightforward comments about the teaching and content of these classes. It's a very useful document. The BUSAC was the organization that really accomplished the inauguration of the undergraduate thesis—the senior thesis in biology. The faculty—and this is true for the institute as a whole—is generally very responsive to thoughtful contributions that come from students. But what's needed is to implement the effective expression of student judgments and to put them in contact with the faculty and try to see that action is taken—and BUSAC served that purpose.

PRUD'HOMME: Did other divisions copy you?

OWEN: Not with that kind of organization. Chemistry has had ombudsmen in their classes, with, I think, a meeting of the ombudsmen once a quarter, with a professor who feels interested in and responsible for undergraduate instruction in chemistry. As far as I know, that's the only other example of a division-wide undergraduate organization. Theirs is quite different from ours. In physics—Physics 1, for example—a professor there had ombudsmen in all of the physics sections and was responsible for that. There has been a feeling—actually, I think, official expression from the faculty board—that each of the divisions should give attention to having rapid feedback of student opinions. But some of the geologists, for example, I think have felt that that was not really very necessary. There aren't many geology students, and they go out on field trips with their professors. And other divisions, like engineering, are so big and diverse that they find it difficult to visualize such a structure.

PRUD'HOMME: What happens in a department if a professor is appointed who is extremely unpopular with the students? How is that recognized?

OWEN: Well, in biology, this would become very evident—probably before midterm—from the comments of the ombudsmen. And the buck for responsibility in this regard stops at the division chairman's desk. The chairman, of course, has ultimate responsibility for the teaching load of members of the faculty. Some are good at teaching and popular in it, and for others teaching just doesn't fit well with their personalities or interests or abilities. Such a professor would probably end up teaching only the graduate course in his special field of interest, and the chairman has to be careful to provide a reasonably equitable load for the rest of the faculty. As you might expect, the ones who are good at it also enjoy it. But our faculty also has extensive research interests and activities outside the institution.

There's a general feeling that when tenure and salaries are considered, it's a person's research status and research contribution that is *the* overriding consideration. And it's certainly true that here, I guess as it should be, that's a very important consideration. But one of the things that Harold Brown had in mind in making me a vice president was that I would then sit in the Institute Advisory Council, with the division chairmen, and participate in discussions about hiring, promotion, tenure, and even salaries. And while Brown was here, that was in fact the case. After Brown left, it got to be a little more uncertain. The provost and the division chairmen felt they didn't particularly value having this student-directed interest represented. And I believe that Jim Morgan—unfortunately, from my point of view—isn't permitted to take part in those kinds of deliberations. I found it very effective, though, to be present and to chip in the word I was getting from students about professors for promotion or even salary considerations—to comment, particularly constructively, about the ones who were doing a very good job. And it had an effect.

PRUD'HOMME: It's terribly difficult, of course, in an institution which is both a research organization—or an organization which permits individual research—as well as a teaching institution, to have any quality control or any evaluation of performance. How do you evaluate the performance of a teacher?

OWEN: Well, the student feedback is important, but you also get peer judgments. This is a common problem at many universities that have ambitions to be top ten universities; they're judged on status considerations that are generally related mostly to the research productivity of the faculty. Here we have a great advantage, in that we have such a large faculty relative to the size of our undergraduate student body. So that if you're in biology, with a faculty now of about thirty, I guess, there are ten who enjoy and are good at undergraduate instruction. They can carry a major part of the teaching load without its being a disproportionately heavy load. We have the luxury of having lots of people to choose from, and therefore, we can fit professors into what they do best. I must say, as I approached the end of this three-year interval—when I knew I was going to become a professor emeritus and could no longer teach or serve in a formal capacity, one of the things I was concerned about was who would succeed me in this interest. And we were very fortunate that Jean-Paul Revel, who is an excellent professor in every way and an excellent teacher, was also interested in becoming the undergraduate advisor and taking the responsibility for that freshman course that I valued so much. And the tutorial program that we had promoted so strongly here and has helped so much to broaden the curriculum for our students, into which I put a good deal of effort—Jim Strauss has taken responsibility for that.

PRUD'HOMME: What's it like being a professor emeritus?

OWEN: In general, our life—June's and mine—hasn't changed discernibly. I miss teaching, especially that freshman course. And I don't serve on university committees and things like that. So some of the things I spent time and energy on in the past are over now. But we live only a couple of blocks from the campus, and I have this space here, with young people coming in and out all the time, and I'm supposed to be working on the revision of that textbook. As you know, I've been visiting colleges and universities, and will continue to do that; I find that quite stimulating, especially interactions with undergraduates at other schools. So I feel, in general, that things are much like they were. I'm not trying to maintain an active experimental research program, but on the other hand I keep pretty well up on what's going on, especially in my field.

PRUD'HOMME: Do you find that students still come to you with research problems?

OWEN: Yes, postdocs, grad students, and undergraduates are in and out all the time. We talk about all aspects of their lives and what they're doing in science. Lee Hood's program is very large; he has something like fifty people in his research enterprise, and he is gone a lot. And since my field is immunology and genetics, and that's an area of interest to him, too, I see people in that particular research group. Of course, I subscribe to journals and things like that. We have a journal club that meets in my office, continues to do so. And undergraduates, as you saw, come in and out, in a very congenial way. I suspect that that will decrease with time, because there's still a population of undergraduates here who knew me as a teacher—or at least knew that I had been a dean. Since my teaching is no longer active, and I'm no longer involved in student affairs, I suspect this will dilute rather rapidly with time, as the undergraduate population is replaced by people who have never known me.

Begin Tape 5, Side 2

PRUD'HOMME: Have you gotten involved in the general brouhaha over genetic change and cloning?

OWEN: I was involved early in the expressions of concern for possible dangers connected with this new technology. This was quite a long time ago, when people began to worry about that. I was at the time a member of the National Science Foundation's Advisory Council in Biology and Medical Sciences. And I guess we might have been among the very earliest groups to suggest that this possible danger should be evaluated, and we were involved in setting up the program at Asilomar that became quite famous, where people got together and talked about the need for safe facilities and things like that. I didn't continue actively in that front because it became the primary concern of people who were directly involved with it—though I've followed what's happened. I think there were legitimate questions, early in the introduction of this technology, as to whether it might generate potential pathogens for which there would be no defense. And that's when the safe facilities were called for, and so on. But the technology rapidly developed in a way to make it quite clear that the dangers are very, very minimal. For example, the vectors that carry these engineered genes—the bacteria or viruses—are tailored in such a way that they simply could not exist outside the lab area. They require an environment and media that are highly artificial. Any ordinary escape would be lethal to the vector.

Of course, biomedical science has dealt with potentially dangerous pathogens over prolonged periods of time. I was involved for a while, back in the fifties or sixties, with the appearance of strange new hemorrhagic fever diseases in Latin America and South America, for example, and there were fatalities—in fact, among the laboratory workers who tried to find out what the agent was and how it could be controlled. In principle, these potentially very dangerous viruses that arise in the natural environment are just as much a problem as the kinds of things that geneticists might engineer. Biological agents of this sort have the faculty of being able to multiply rapidly, and so a stray particle growing in the population can become a major problem. But I don't feel now that continued major concern is necessary for the safety of general genetic research, though some possible applications—to develop agents for biological warfare—of course are a matter of great concern.

PRUD'HOMME: What do you think has been your most effective research work?

OWEN: The part of my research interests that began with the fortunate discovery of chimerism in twin cattle led to the recognition of immunological tolerance. But more than that, our extension of that into marrow transplantation and irradiated recipients led to the development of experimental chimeras produced in that way, which could be used to study fractions of cell populations, especially of lymphocytes. And I think that has had the broadest influence and is the most interesting of the things I've been involved with.

PRUD'HOMME: And in terms of the administrative work that you've done here at Caltech, the many, many jobs you've held?

OWEN: I guess the most rewarding would have been not the vice presidency for student affairs, which was largely a business of helping to see that various offices operated effectively, but the job of dean of students, and then later the similar job of undergraduate advisor in biology. I think, as I look back at it—and I think I probably expressed this rather corny sentiment before—I've had a very fortunate and satisfying life. But when you get a letter from a student or get some word back about somebody who's gone out into the world, and it appears that you have done something to influence a young person's life or made a difference in his life for the good—I think that's the most ego-rewarding aspect of one's life. And I've had a good many opportunities along those lines.

I've been fortunate to be here. My wife and I tend to like wherever we are, I guess. We liked Wisconsin, we liked Oak Ridge. But Caltech has a certain unusualness about it. For one thing, there's our students. They are a wonderful group of young people—very, very able in many different ways—and there aren't so many of them that they get lost in a crowd. You know them as individuals; they often can use the kind of help you can give them. And they respond extremely well to opportunities you give them. Your peers on the faculty here are high-status people; they're good people, they're bright and productive and stimulating as colleagues—with extraordinarily few exceptions. The institute has a kind of status that rubs off on us, so that we more easily get elected to the National Academy of Sciences by virtue of the fact that we're here, than people of comparable merit at lesser institutions. We have an administration that over the years has been interested in promoting excellence and knows what it is, and is prepared to do what's necessary to maintain the continued excellence of the institution. And minimal interference with the professors doing what they want to do and can do best—offering professors the best kind of support. June and I enjoy living in Southern California; we like the community of Pasadena and all the things that go on, and we like Los Angeles. So altogether I can't see how one could be better off than to have lived life as a professor at Caltech.

PRUD'HOMME: Do you have anything else you would like to say?

OWEN: I was going to mention a couple of things. This book, *Frontiers in Immunogenetics*, which was edited by one of my students, Bill Hildemann, who recently died, was a celebration of my sixty-fifth birthday—a little bit before the birthday, I think—and it brought people back who had been my students to get together. And that was lovely.

And another thing I was going to mention was that when I first came to Caltech, there was an organization called the Anaximandrian Society—we're going way back in time now, back to 1946. One of the things that a group of interested faculty and students did, mostly in biology, was to get together at monthly intervals. One of them would give a paper about some aspect of the history of biology. This society, which had been started ten years earlier, bound and put in the Archives the papers that had been given. I have volumes I and XII here. This was the last one that got put together this way. And I know this society continued beyond that point, because volume XII is '44-'45, and my paper on Richard Owen, which I know I did, isn't over in the Archives. So, for some reason, it fell off. But it was a very interesting group. For instance, in '44-'45, the members were James Bonner and Henry Borsook and Arthur Cohen and Sterling Emerson, and van Harreveld and Jeff Keighley, and some other people—Frits Went, Wiersma, [Laszlo] Zechmeister, Sturtevant. It was all regarded as really great fun, but they're respectable contributions, too, for the careful work on either the history of some concept or some person who had been important in biology. I don't think that nowadays you would be likely to find a group of people with that kind of energy and enthusiasm. Maybe it's because history is being made so very fast; and as the pace increases, people don't on the whole take much time to look back, until they get old and gray.

PRUD'HOMME: Is it helpful in your research work to delve into the history of biology?

OWEN: Certainly, having lived through a good deal of that history, it helps to keep track of what's going on; because the things in the present evolve out of the past, and if you understand the past, it makes it easier to understand the present.

There's one curious little note here, in this last bound set, volume XII, '44-'45. It says, "Early in the year, the members decided to open the society to women who are interested in the history of biology. They may attend the meeting as guests and may also become members of the society," which was a very unusual thing for Caltech at that time. And I don't like to think that that had anything to do with the fact that this society stopped soon after that.

PRUD'HOMME: Does the Archives have those?

OWEN: In fact, I got these from the Archives. I'll take them back to Judy [Goodstein, Institute Archivist]. But I did remember that that society was one of the fun things about coming here as a Gosney Fellow that first year. I'd been interested in the history of biology myself.