Lyman Bonner, 1970

LYMAN G. BONNER
(1912-2002)

INTERVIEWED BY
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Subject area

Administration; chemistry

Abstract

An interview in two sessions in April 1989 with Lyman Bonner, who held several important positions in Caltech's administration from 1965 to 1989. He and his brother James spent the academic year of 1929-1930 as Caltech undergraduates while their father was on sabbatical here from the University of Utah. Both returned to Utah and received BS degrees in chemistry, James in 1931 and Lyman in 1932. Both then returned to Caltech as graduate students: James received a PhD in biology, joining the Caltech faculty in 1936; Lyman received his PhD in chemistry in 1935 (the second of four Bonner brothers to earn Caltech PhDs) and went as a postdoc to Princeton, where he switched to physics. He then worked at Duke University with Hertha Sponer on infrared spectroscopy, joined the faculty, and taught physics there in the wartime V-12 program. He recalls those early academic years and his wartime work on rocket propellants at Allegany Ballistics Laboratory and later at Hercules, Inc., ending as director of development for its Chemical Propulsion Division in Bacchus, Utah.

He returned to Caltech in 1965 as director of foundation relations. Two years later, he became assistant to President Lee A. DuBridge for facilities planning and in 1968 became director of student relations. He describes the careers of his scientist siblings. He recalls the student activism led by undergraduate Joseph Rhodes and the moderate unrest at Caltech during the Vietnam War. Among his duties under President Harold Brown was oversight of the student Health Center; he discusses its history, as well as the admission of women beginning in 1970. In 1977, he became registrar, holding that post until his retirement in 1989.

Administrative information

Access

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH LYMAN BONNER

BY SHELLEY ERWIN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Lyman Bonner **by Shelley Erwin**

Pasadena, California

Session 1 **April 18, 1989**

Session 2 **April 20, 1989**

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ERWIN: Dr. Bonner, would you begin by describing your first association with Caltech?

BONNER: Yes, I would be glad to. My first association was in the fall of 1929. In order to explain that, I should go back several years and put it into a degree of context. My father was head of the Chemistry Department at the University of Utah. He went there in 1915 from a teaching position in Canada, where I was born, and in 1922 he was eligible for sabbatical leave, which he took at the University of California at Berkeley, taking with him his wife and seven children.

ERWIN: Seven children. And which child were you in this family?

BONNER: I'm number two. James, my older brother, is [professor of biology] emeritus here at Caltech. I have a sister next to me, and there are four boys who followed in sequence—we might deal with some of them later. At Berkeley my father encountered a young man named Don Yost, who was in the navy in World War I and later took up the study of chemistry at Berkeley. He was married and had a child by then, so he was more mature than most and apparently somewhat unsure as to what he should do next. Father befriended him and encouraged him to continue in chemistry and, in fact, offered him a fellowship at the University of Utah the next year so he could continue for a master's degree, which was as far as the University of Utah could go in those days. So Don did come to Utah, got his degree, and then with encouragement and good

recommendations, went from there to Caltech for a PhD in inorganic chemistry. He completed that and stayed on at Caltech for the rest of his career. When there was another sabbatical in 1929, Don in effect invited my father to come and spend this period here, which he did—again with his wife and all seven children. James and I by then were in college, at the University of Utah. Priscilla was in high school, the others were in lower grades. We came, arriving in September of 1929, found a place to live up on North Raymond Avenue, north of Washington—almost up to Montana Avenue, in fact. James and I took the transfer exams and registered as transfer students at Caltech, I as a sophomore and he as a junior. My sister went to John Muir High School and graduated there in the spring of 1930, and the younger boys distributed themselves through the various grades in Washington School.

ERWIN: Was there any difficulty in passing entrance exams?

BONNER: I don't remember any difficulties or trauma. We came, we registered for appropriate courses. We both of us had to go into freshman physics, because we had never taken any physics courses yet at Utah.

ERWIN: Was that because they didn't exist at Utah?

BONNER: They existed, but we were both oriented at that point toward chemistry and didn't get around to physics until sometime later. At Caltech, it was required. Math was fine, though. We went on our ways and took our courses, some more successfully than others. In fact, my prowess during the year was not great. I think I felt over my head at Caltech. I was a skinny seventeen-year-old at that point.

ERWIN: Oh, you were rather younger than the usual sophomore?

BONNER: Rather young, yes.

ERWIN: Was that because your schooling up to then had been unusual?

BONNER: Well, I'd been—

ERWIN: You'd been fast-tracked, as they say today?

BONNER: Fast-tracked, yes. Graduated at fifteen and started university at sixteen, sophomore at seventeen, and so on. Graduated from college at nineteen, got my PhD at twenty-two. Got an early start.

ERWIN: Yes.

BONNER: I've tried to catch up ever since. But, as I said, I felt somewhat over my head and did not do well. In fact, at the end of the year I was invited to appear before whatever then was the equivalent of the [Undergraduate] Academic Standards and Honors Committee to justify my coming back the next year. Since I wasn't going to come back anyway, I simply went back to the University of Utah.

I could comment, though, on what Caltech was like in 1929. Quite different from this day. The principal buildings were Throop Hall and Gates Laboratory of Chemistry, which is the building where we now are. The Bridge Laboratories of Physics had been completed across the way, and Dabney [Hall] had recently been built to the east. The building which is now Sloan [Albert P. Sloan Laboratory of Mathematics and Physics] was then the High Voltage Research Laboratory. The first segment of the [William G.] Kerckhoff Laboratories [of the Biological Sciences] had just been completed. This, and Culbertson Auditorium, were, I think, about the only other buildings on the campus at this time. The student houses hadn't been built yet. The area from here across to Bridge and west to Wilson Avenue was dirt, except in the rain, in which case it was mud and you had to walk around it. No other amenities anywhere. It was a rather stark campus.

ERWIN: Your dad was on sabbatical. Now, was he actually teaching or was he observing?

BONNER: No, he was reading, attending a few classes. He had an office up here on the second floor of Gates. From the outside, I can identify the window of his office at that time.

ERWIN: Did he know someone at Caltech other than Yost at that time, or how did he get the appointment?

BONNER: I don't know specifically of any other contacts, but chemistry was a much smaller world in those days, and I think it was likely that he knew others: Dr. [Arthur Amos] Noyes, for instance.

ERWIN: Sounds a bit more informal—I guess that's what I'm driving at—than at the present day.

BONNER: It was distinctly informal. Also, there was no money commitment from the institute. He received half his Utah salary, and on this we could just barely get along. I remember it also, though, as a much more formal time. I remember formal Sunday afternoon calls from Arnold Beckman [then an instructor in chemistry] and his wife, from Stuart Bates [professor of physical chemistry] and other faculty members of that time. Also I remember the whole family being invited to dinner at the beach home of Arthur Amos Noyes, chairman of the division.

ERWIN: Of chemistry?

BONNER: Chemistry. And generally known as one of the founders of the institute. Portly, courtly old gentleman. We went to his home, in Laguna I think it was [Dr. Noyes's beach house was in Corona del Mar—ed.], were served a delicious Sunday noon dinner and then went back home again.

ERWIN: So you came back to Caltech in 1930-something. You completed your PhD in '35, right?

BONNER: We all went back to Utah and completed our courses in chemistry at the university there. James graduated in the next year, 1931, and came immediately back to Caltech and graduate school, following up on some contacts he'd made in the Biology Division with Theodosius Dobzhansky and others with an interest in genetics. They were glad to have him back. The following year, 1932, I graduated from the University of Utah with an excellent University of Utah record and was then accepted as a graduate student at Caltech myself, but without a fellowship. My parents considered it and decided I would be much better off at Caltech than going east to Brown, where I had been offered a fellowship. Caltech sounded good

to me, too, so with their help I borrowed the tuition money from the president of the University of Utah and came along.

ERWIN: That's an interesting twist.

BONNER: James was then living in a house at 290 South Michigan, just across the street and somewhat to the north of the Tolman—now Bacher—house there at Lura Avenue. Their place was owned by the Planas family, Joe and Maria, and was a center for much of the Southern California French community, amongst other things, but was also a haven for many Caltech students. By the time I got here, James had a small two-room—bedroom and sort of pseudo-kitchen—apartment arrangement, which I then shared with him. Other Caltech students lived in other rooms there, and, in fact, subsequently two more of my brothers lived at the same address when they came through on their way to Caltech PhDs. James paid the rent, so far as I know or can remember, out of his stipend. I got employment then in the National Youth Administration student program. Twenty-five cents an hour, folding cardboard to make crystal models for Linus Pauling. This bought some of the groceries and supported part of my expenses. We lived very frugally, ate lots of potatoes and lettuce.

ERWIN: Who was your advisor during this period? Or with whom were you closest on the faculty?

BONNER: I consulted first, of course, with Don Yost when I came, and then went out and around on his recommendation to talk to the other people in the department as to what they were doing, what I would like to do, and finally selected Richard Badger, who was at that point working on molecular spectra, molecular structures, bond strengths, bond force constants, things of that sort. He had, I believe, only one other student at that time, and I was welcomed. One other building, which I neglected to mention earlier, then known as the astrophysics building, now [Henry M.] Robinson Laboratory [of Astrophysics], had just been completed as part of the program for the new 200-inch telescope which was to be built but which was not yet much more than a gleam. There didn't seem to be any astrophysicists yet, so the building was empty, and of course vacuums absorb, and much of the Pauling and Badger groups simply moved in there. That was my home during my three years in graduate school. A laboratory in the basement and offices in

various places depending on where one could be found and how many people you had to share it with.

My work as a graduate student was interesting. I started out with a liberal course in woodworking and concrete pouring. In the process of building the twenty-one-foot grating spectrograph that Badger was setting up in the basement lab in Robinson, one of the first things we had to have was two concrete tables to set it on. So we set out to build them ourselves, which meant mostly by graduate-student slave labor.

ERWIN: Was this a typical pattern at that time—that a lot of the equipment was built here?

BONNER: Yes, indeed. The notion of outside research support didn't really exist at that time. The government support we all count on now didn't come until after World War II. In those days, if you couldn't do it yourself and couldn't gather up funds from the institute somehow, you weren't likely to get it done. So there was a considerable incentive to do yourself what you could and have the important parts made by the skilled mechanics in the instrument shop in the basement of Gates. That's where the important workings came from. Badger helped mix concrete and pour the tables. Then, in the course of the year, we assembled it. He did the important work and I did the scut work, like building a bellows for the spectrograph and things of that sort. Ultimately it did work well and was a very-high-resolution instrument used primarily for recording the absorption spectra of gases, mostly in the near infrared, where his interests lay. As a second part of the same enterprise, we built the next year a seventy-foot-long stainless steel pipe, optically aligned as an absorption tube, to feed light into the spectrograph.

The interesting thing about the whole matter was that my own research interests and the thesis with which I finally graduated had very little to do with any of this instrumentation. Instead I got off into other directions, stimulated in part by the continuing Linus Pauling graduate seminar course on, initially, chemical quantum mechanics, the developments there, and how they applied to chemical compounds. We then went on to crystals, diatomic and triatomic molecules, things of that sort. The seminar continued through my three years in graduate school and treated these and various other aspects of chemistry, Linus Pauling–style. Linus Pauling was certainly the most charismatic teacher I've ever had in my career. He could really make things interesting and attractive like very few other teachers I've encountered. Of course, he wasn't the only one

here. Richard Tolman, who was already well known and who went on to considerable later fame, was a member jointly of chemistry and physics. His office, however, was in the basement of Gates, and he was accessible. He came to all of our chemistry research seminars. At these seminars, incidentally, cocoa prepared by the Noyes recipe was always served. Always. No exceptions. His favorite graduate student would prepare the cocoa in a back room, put it in cups for the rest of us to pick up. Roscoe Dickinson was a member of the faculty then. He was the very first Caltech PhD [1920] and stayed on for the rest of his career at Caltech. He was also an impressive chap.

ERWIN: Was Noyes still around at that time?

BONNER: Noyes, by the time I was here, by ’33, was increasingly a sick man. He was suffering, I believe, from a form of rectal cancer. He had an operation, I remember, after which he returned, looking very pale and sitting on a pillow, which was a disguised doughnut. Very, very unhappy. He survived through my years here but died, I believe, within a year or less of my degree in 1935 [Noyes died in June 1936—ed.] .

ERWIN: So your best memories of him are really from the twenties?

BONNER: 1929-30. The work I did for my PhD thesis, as I said, was partly stimulated by Pauling’s course and other things that I’d read, and consisted of a form of quantum-mechanical analysis of the vibrations of a triatomic molecule—specifically, water. There’d been a straight-line carbon-dioxide model treated in the journals a couple of years before. I took a modification of that and applied it to the water molecule to get a better analysis of the force constants at work there and to determine values for those constants from the observed infrared absorption spectrum of that molecule. That was one part of my thesis. The other part was an analysis of the fundamental vibration modes of a more complicated molecule—the ethylene molecule—and an assignment of a specific frequency from the observed spectrum. A friend of mine, much later, noted that I had been the first to assign frequencies to the fundamental modes of ethylene. He said, “Of course you were wrong.” I said, “Yes, but I was close,” and he agreed.

ERWIN: Well, did this thesis, or dissertation, then set you on a certain track? Did you know what you wanted to do after Caltech? And how did you make the choice?

BONNER: How does one decide such things? It's particularly difficult when you're in the middle of the beginning of the Great Depression and jobs are not readily available. What do you do with a fresh PhD in chemistry? In my case it was easy, because my mother and father both impressed upon all of us children the importance of the academic way of life and the importance of education. Both of them came to educations late in their lives. They both grew up in Nebraska farm families, and it took a great deal of drive on the part of themselves, and their parents, too, no doubt. But they ultimately got off to college and, in my father's case, on to graduate school and finally back into the professorial route. So, as I've said, they felt these things quite keenly and urged all of us to at least consider an academic career, and in my case, I really didn't see any viable alternatives. I was young and impressionable—that was the only way to go. I had applied for—in fact, I was nominated by Caltech for—a National Research Council postdoctoral fellowship, of which there weren't very many. In fact, that year there were twelve given in chemistry in the country, I learned later. I was lucky enough to be one of them.

I went on with that, then, to Princeton, to follow up on my interest in polyatomic molecules and the fundamental vibrations thereof, which required a greater knowledge of the practice and principles of infrared spectroscopy. A man named [R. Bowling] Barnes was doing this at Princeton in a way that seemed interesting to me and acceptable to the National Research Council, so that's where I went. Except that at Princeton, Barnes and his version of spectroscopy—infrared spectroscopy—was in the Physics Department, so I found myself there living amongst physicists and learning physics language and going to some of the physics meetings with them. In the course of my two years there, I decided really I enjoyed physics and physicists more than I enjoyed chemistry and chemists, and I quietly made a switch.

ERWIN: And was this considered to be a defection by your parents at that point?

BONNER: Oh, no, no, not at all. It was still within the bounds.

ERWIN: I see.

BONNER: I went on from there, then, to a job in the Physics Department at Duke University—again to academia—to work first with Hertha Sponer, who was an important woman spectroscopist of the time. Then ultimately on my own, building my own infrared spectrometer and things of that sort. I was reasonably well supported and enjoyed my life there—enjoyed too, within limits, teaching and the teaching that I did in physics. I had to learn a certain amount of physics to do it successfully, but I didn't mind that. My own physics background wasn't really great. I probably would have stayed there indefinitely had it not been for the advent of World War II, which stirred things up. One of the first things that happened that affected me at Duke was that the navy set up, across the country, a variety of training programs—the so-called navy V-12 program for training naval engineering and deck officers. They set these up in a few centers. Caltech was one of the V-12 centers. Duke was another one, one of the largest. The V-12 program demanded of all the students, picked up from all over, a basic training course in physics, which most of them hadn't had. This started in 1942, so the next couple of years those of us who were able to survive the draft found ourselves up to our ears, or above, in teaching uninterested undergraduates the principles of physics, our style.

ERWIN: So you were teaching for the navy?

BONNER: They were naval recruits training to be deck officers or engineering officers. If they washed out of there, they went out as gobs, but they were in the navy. After a couple years of that, I was getting distinctly tired of teaching. I found myself frequently doing the same course three hours in a row, simply because we had so few people to do it, which is very dispiriting. As the V-12 program began to phase down, in 1944, I thought of other things to do, and one of these related to the fact that in the summer of 1942 I had been invited to Washington by a friend of mine, a former friend from Princeton, to engage in a high-pressure-reactions program with him at the Carnegie Geophysical Laboratory. This was part of a study of the thermodynamics, thermal chemistry, and other characteristics of the products of combustion of solid propellants, and our job was to determine spectroscopically the temperatures of the gases. When I went back to Duke in the fall of '42, I took with me a small contract with the Office of Scientific Research and Development to continue some related research on the closed bomb burning rates of these propellants. The bomb, in this case, is simply a high-pressure container in which you can burn

the propellants under controlled conditions, measure the pressure-versus-time relationships, and from this deduce the laws governing the relationship of burning rate to pressure and see how these apply to the actual ballistics of guns and rockets. I was the principal investigator—and, in fact, almost the only investigator. I got some fairly good work done, but by 1944 this was beginning to look relatively non-productive and the teaching commitment was winding down. I decided, then, to transfer my operations from Duke to what had then become the government agency with which I was most closely in contact in these matters, which was a place called the Allegany Ballistics Laboratory—ABL—near Cumberland, Maryland.

ERWIN: So that was a government laboratory.

BONNER: It was a government laboratory. It was started by the army and converted in 1943 from gun-propellant manufacturing to solid-propellant rocket development under the Office of Scientific Research and Development.

Begin Tape 1, Side 2

BONNER: OSRD worked largely with universities, and it was under contract with them that George Washington University operated ABL. ABL was, in effect, the East Coast rocket branch of the military OSRD studies. The West Coast branch was centered at Caltech. I was aware of no contact between the two operations, though. They were operating quite separately and didn't really talk much until much later.

ERWIN: Did you know the people involved in this?

BONNER: We were too busy doing our own thing. We had our chores, they had their chores. And it wasn't till after the war was over that we could get together and compare notes. I went down to ABL to see what I could do that was useful there and found myself engaged very quickly in basically engineering-type work—project engineering, designing, testing, proofing, and to some extent program managing. One of my programs there was directed toward a jet-assisted take-off unit for aircraft, which got as far as field testing before the war was over.

ERWIN: Well, now, you received an award from the navy, one of their very highest awards—the highest civilian award, the Distinguished Public Service Award.

BONNER: I forgot to bring it with me.

ERWIN: When did you receive it and for what?

BONNER: This is in the next chapter. At the end of the war, in the summer of 1945, all the things we were doing ground quickly to a halt. People started looking around or going back to where they had come from. What to do next, where do we go from here? In my case I had really mixed emotions. I was a little fed up with the teaching business. And Duke, which had started off in the 1920s with a lot of fanfare, didn't seem to offer that great a future. They were not remaining competitive by hiring bright young people but were instead—it seemed to some of us—bringing in a lot of has-been names to bolster their escutcheon. How much was that and how much was just plain discouragement with all those wartime activities, I don't know, but I was susceptible to offers from elsewhere. The navy—for whom much of the rocket work in World War II had been done anyway—decided that they would like to take the facility there [ABL] over from the army and operate it. Not with a university contract, because the OSRD disappeared as soon as the war was over, but with an industrial contract, which they negotiated with the Hercules Powder Company, which had run the principal manufacturing plants for rocket propellants during the war. The combination, then, of Hercules and navy offered me the option of staying on there as technical director at a pretty good salary and continuing to do the things that I had found very intriguing, and I accepted. I resigned my position at Duke—I was on leave of absence—and stayed on at ABL. In fact, I stayed there for the next ten years. We started off rather small, a total of two hundred and twenty-five people in 1946, when we officially opened our doors under the new Hercules flag. We grew some with time but stayed relatively small. Hercules shipped in there for me to train, do what I could with, some of the brightest young men they had in their wartime plants. With these, plus a couple of people who stayed over from the wartime activities, we went ahead in the matter of researching and developing improved propellants for rockets and of designing improved engineering structures, which would give better performance, better weight-to-impulse relationships than had ever been attained before. We were rather successful in this, and, in fact, over the next several years, we practically had a

lock on booster rockets in the big postwar rush for better antiaircraft missiles. The navy's Terrier, Talos, and so on, series of missiles was, of course, an important part of that, and our booster rockets and later sustainer rockets were critical to the success of those programs. So the navy thought well of us. Ultimately, in 1953 they decided we deserved some medals and voted to myself and my two principal lieutenants, one in structures and one in propellants, the Navy Distinguished Public Service Award, which they advertise as their highest civilian award. This was awarded with suitable ceremonies there at the laboratory with not the secretary but the assistant secretary of the navy there to do it in person. All the population of the laboratory gathered to applaud, and it was quite an occasion.

ERWIN: I see that this was now some time after the war.

BONNER: Yes. It was entirely, *entirely* after the end, although I did get a certificate from the OSRD citing my invaluable contributions—I think everybody got one of those—

ERWIN: —to the war effort or something. I see.

BONNER: So that's where I spent the next ten years. But by the end of ten years I was getting very tired of the rocket business. I may sound fickle, but I have a general feeling that in ten years with a particular job, you've given it anything new you had to give and it's time to move on. I was distinctly restless, so when the company offered to transfer me to the home offices, the company home offices in Wilmington, Delaware, and put me in charge of the research and development activities of the explosives department—this was the segment of the company which was in charge of ABL—I was quite intrigued, particularly so because this would include responsibility for their burgeoning industrial nitrogen chemicals operation. So I gladly moved in 1955 to Wilmington and took up offices in the Hercules Research Center. This was located in the pleasant suburban outskirts of Wilmington, in the middle of the Hercules Country Club, surrounded by a golf course and other things. It's hard to imagine nicer surroundings in which to work.

So there I was, enjoying my first and only look at chemistry as practiced anywhere. My first opportunity to pretend to be a chemist since I got my degree. But that didn't last long. I was, in effect, principal research and development counselor to the explosives department. A

potentially good-sized fragment of what the department might do in the future was in rockets and rocket propellants for the military and derived from the ABL developments. So somewhere there, after a year or two—I was keeping in contact with and visiting my former lab every now and then—I wrote a sort of a white paper to the authorities, giving them my view of what their future was in the matter of continuing in the rockets and propulsion business. The ABL work was very good and very helpful to many of the other government agencies. However, it had the big drawback that it was exclusively owned by the navy, and contracts for development for other people could only be done through transfer of funds to the navy. This did not go well with the army or the air force at all. Even within the navy, between the bureau of ordnance, who controlled ABL, and the bureau of aeronautics, who had responsibility for air-to-air weapons, it didn't work very well. Further, the ABL lead was being rapidly eroded by other companies. Aerojet [Engineering Corporation], which had been formed during the war and was an offshoot of the Caltech GALCIT [Guggenheim Aeronautical Laboratory of the California Institute of Technology] operation, had a substantial operation of their own. They were picking up quite a lot of work, not necessarily because they were better—we beat them in competition many times—but there they were, privately owned and free to contract directly with anyone. This was very appealing, particularly for the new and jealous air force, who didn't want a lot of people messing around. I pointed out some of these facts and factors to Hercules, as to what they might do. One of the things I said to them was they could leave things as they were and watch the business gradually disappear. Or, if they were interested at all, they could step out quickly and make a major commitment to a company-owned establishment, somewhere in the U.S., ready, able, and willing to contract with anyone.

ERWIN: And be competitive.

BONNER: And be competitive. I guess this had some weight with the powers out there, because not too much later the department presented to the board of directors, and got approved, a major project involving initially a $10-million expenditure to start construction of a Hercules plant in Utah. This entailed a new organization, the so-called Chemical Propulsion Division of the Explosives Department. And sure enough, I got caught up in it and transferred from out in the country down to the big building in the middle of the city as director of development for the new

operation. So there I was, after two and a half years, back again right in the middle of the rocket business. The Hercules move was very close to being too late, but not quite. They grabbed onto the tail end of the train as it went by. We got, right off the bat, a contract for the third-stage rocket for the Minuteman missile, which was the air force's new big thing. And that went nicely. We moved on from there to a major part in the production of Polaris for the navy. The plant at Bacchus—Bacchus, Utah, which has nothing to do with the god of wine but was named—

ERWIN: Spelled that way.

BONNER: Spelled that way—after one of the original vice presidents of the company, back in 1912. The Bacchus plant was a small dynamite plant in the Salt Lake Valley some twenty-five miles west of Salt Lake City. It was built there in 1915 to supply dynamite to the Bingham open-cut copper mines just down the street there. It seemed to us a good place for the rocket operation so we staked out a claim, bought some more land—

ERWIN: And so were you then back in your home territory?

BONNER: I stayed in Wilmington and commuted. This was early '58 that all this happened. So for five or six years I commuted more or less steadily from Wilmington to Salt Lake City and to the West Coast, where the air force had their ballistic missile headquarters. I got in a lot of travel time, but this sort of life, again, was not terribly rewarding. I believe it is generally true in industry that a science PhD is not the best starting background. Scientists are seen as OK in the research lab but can't be trusted to actually run things, because they don't understand.

ERWIN: Why is that? Why do you suppose that attitude is there?

BONNER: It's related, I guess, to the view that academics—and if you have a PhD, you're clearly seen as an academic—are much more interested in thinking and planning, not in getting things done. The only successful PhDs I encountered in industry were ones who had become, in their own right, demon salesmen. That's the clue to it all. I didn't pass that test. I give Hercules credit, though—they offered me an opportunity. As time went by and people moved onward and upward, I was offered the position of director of sales for chemical propulsion. I thought this

over very carefully and decided that really it was not within me to be that kind of a person. I'd enjoyed traveling with the previous director of sales—who went on within a few years to be president of the company—and joining in his dog-and-pony shows for potential customers was sort of fun. But to have the full responsibility for opening doors, for busting in, glad-handing and so forth, was not one of my traits, so I said no. This put me in a kind of backwater. The company continued to love me, tried to use me where they could, but there was really no place for me to go. I continued to get increases in salary but reductions in scope, and I finally concluded that the only solution was to quit Hercules and go on to something else. So I resigned. I needed to do something quite different and preferably, I said to myself and to Hercules, something with an academic connection. Hercules didn't see this as threatening, since I wasn't planning to join the competition, so they didn't tell me to go away immediately but kept me on the payroll there for another six months while I looked around. I had, effectively, offers quite quickly from two government agencies, the National Science Foundation and one of the government think tanks—IDA [Institute for Defense Analyses]—there in Washington. But this was not what I wanted. I'd simply be moving from one side of the government-contract table to the other. I really wanted out of it completely. I sent letters around seeing what there might be available in the way of teaching positions in smaller colleges, but that didn't get many bites.

ERWIN: What year would we be in now?

BONNER: This was the year 1965. The spring of 1965, I was actively job hunting. And through James [Bonner], this came to the attention of Chuck Newton, who was director of development here at Caltech. Chuck Newton was getting ready for a fund drive in 1967, the next major fund drive, and trying to enhance the overall development department. My overall background, with a Caltech degree and business and industrial experience and some savvy along those lines—

ERWIN: And government contracts.

BONNER: Government contracts—all of those seemed interesting to Chuck. I came to talk with him, talked with a lot of other people—talked with everyone here, far as I know. They must have been satisfied, because Chuck offered me a position here as director of foundation relations, a brand-new job that hadn't existed before. After thinking it over for a day or two, it seemed to

me very much the sort of thing I would like to try. As Chuck pointed out, it didn't mean necessarily staying in development indefinitely, but it was an entering step in the administration, a chance to learn what the administrative roles of a university were. On that basis, I was glad to take it, at a forty-per-cent cut in salary, but I'd expected at least that. It wasn't money I was looking for but a little more satisfaction. And I've never regretted it.

ERWIN: And so that started in July?

BONNER: So July 1, 1965, I showed up on the doorstep here and was installed as the director of foundation relations, with an office in the basement of Culbertson Hall, which you've never even seen. Hasn't been there for lo these many years. Culbertson Hall, built in 1922, was over near the corner of Wilson and California. It was destroyed in 1973 to make room for what is known colloquially as South Mudd [Seeley G. Mudd Building of Geophysics and Planetary Science]. The big geology building. That took it out of play. And many of us missed it for quite a long time. It was the only place for informal student gatherings of any size and a place that was sufficiently run down that it didn't matter too much what you did to it. It was a handsome building, handsome interiors, but they figured that Beckman Auditorium replaced all its functions.

ERWIN: Could we back up just a little bit, before we launch into what you did as director of foundation relations? To take in a little bit about your family? I've mentioned to you that I found something about your brother David being at Caltech, and you said that others of your brothers had been here as well?

BONNER: Yes. The family, as I said earlier, consisted of James and myself and my sister, and then, in order, David, Robert, Walter Jr.—father was Walter Bonner—and Francis. All seven of us went through the University of Utah and graduated with bachelor's degrees in chemistry. In the Depression, with our father an underpaid college professor, the only chance we had of going to college, really, was to stay at home and do it at the local university, where we got cut-rate tuition on account of being faculty children. Then we went on from there. James came here to graduate school. I followed him a year later. James got his degree in 1934, went off to Europe for a year. Came back to Caltech and has been here ever since. I got my degree in '35, went to

the East Coast and dawdled around there for thirty years and finally came back. Priscilla graduated in chemistry, as we all did, went to a position in Detroit in biochemistry, went on to achieve finally a master's in that and also married Jim [Walter James] Horton, who was acquiring a PhD in organic chemistry. They ultimately wound up back in Utah. Jim, after the war, went to the University of Utah as professor of organic chemistry. David, the next one, came to Caltech, following in the footsteps, in biology. In fact, most of my brothers went on to biology. A couple of us were steadfast. David came back in biology; he got his PhD in 1940, after living at 290 South Michigan. David was a contentious lad. In fact, James and I had a lot of trouble with him when he was a kid, because he was so ornery. He continued contentious—

ERWIN: He was the middle one.

BONNER: Yes.

ERWIN: Maybe that's something about his position.

BONNER: Right in the middle, yes. He defended himself very well. David may well have had a battle with the draft board.

ERWIN: There's a letter from George Beadle [then a postdoc, later chair of the Division of Biology, 1946-1960] to—it just says, "Local Board number-something-or other," on behalf of David Bonner.

BONNER: This sort of seeking deferment was standard. It must have worked, because I'm sure he was, in fact, deferred. David went on from here to Stanford with George Beadle and worked with Beadle then for the next several years. Some time in '46 or '47 he went to Yale and was finally lured away from Yale in 1960 to come back to California and help build the then brand-new University of California at San Diego, with headquarters in La Jolla.

ERWIN: That's a nice place, too.

BONNER: He's one of the founding fathers. Unfortunately, he acquired Hodgkin's disease a number of years earlier and finally died of that in 1964. In the spring of 1965, my wife and I—not Jackie, but my first wife—came out to California to the campus. I remember very well that one of the speakers at the memorial service there spoke of David's contentiousness and his continuous fighting with this, that, and everybody about almost everything. Said that David fought joyously. He wasn't mad, he just enjoyed it—didn't make them all that mad, either, and he got many things done.

Anyway, Robert started to be a chemist at Brown. That didn't work very well. He switched to mathematics. World War II had come on by then, and he went off into an army electronics training school, which put him back in the army, then, in the early forties. He did a variety of things, including a stint in the South Pacific. They sent him back from that to a stint at Officer Candidate School in the Signal Corps. When he got through with that.... He also played the piano; they pulled that up on the computer and sent him to be entertainment officer, whatever, at a training camp in the deep South. He had a tangled career. He went on to computer programming and such in the petroleum business. Much of it away—Venezuela, Near East, Saudi Arabia. He's finally now retired and living in Houston.

ERWIN: So that was a non-academic career.

BONNER: Non-academic. Robert and I are the only dedicated non-academics. Although I have some fair academic credentials in between, myself.

ERWIN: Yes, I think so.

BONNER: Walter, then, came here to graduate school, lived at 290 South Michigan. He got caught up in the middle of World War II activities, particularly the Caltech rocket project. And he and his either then or to-be wife, I've forgotten which, worked, got deferments working in the Linus Pauling rocket chemistry program. He didn't get his degree until 1946. Went off to England—Cambridge—for several years. Came back and is now a mainstay at the University of Pennsylvania. Francis went on to Yale for his PhD near the tail end of the war. Wound up in the Manhattan Project. Went to Tennessee for a few years and went back into academia, doing a variety of things, including a very successful textbook, back in the days when the way of life was

going to be all the sciences in a single book. The book he wrote, with Melba Phillips, Bonner and Phillips [*Principles of Physical Science* (New York: Addison-Wesley, 1957)], was a mainstay at that, for quite a number of years. So, anyway, Francis ultimately wound up as the first chairman of the Chemistry Department at SUNY Stonybrook, which he helped get started and where he still is. Walter is facing retirement this year, Francis in two more years. Academia still sticks to age seventy for tenured faculty.

ERWIN: For faculty.

BONNER: Tenured faculty, I keep telling my colleagues here, [they] are a specially blessed group. They know when they're going to retire.

ERWIN: You seem to have been remarkably persistent in your staying on. So that's, let's see, of the six boys—your sister wouldn't have been able to come to Caltech—but of the six males in the family, four of them—

BONNER: Four of the six are—

ERWIN: Are Caltech graduates.

BONNER: Yes.

Begin Tape 2, Side 1

ERWIN: So in 1965 you came back to Caltech and you were director of foundation relations. It seems to me that what went under the name of development in those days was somewhat different from what goes under the name of development today. Maybe we could talk a little bit about that in this section.

BONNER: Development then meant simply that: the raising of funds, the planning of campaigns, things of that sort. Later there was a new position developed—the vice president for institute

relations—which covered all branches: public relations, publications, development, alumni, all of those, which up to that time were separate operations.

ERWIN: Now, when would that have been? That was after [Caltech president Harold] Brown came, wasn't it—or maybe I'm wrong on that?

BONNER: The first vice president for development was in 1966. That fits into another operation, which we'll pick up somewhere along the line, I think. So in 1965 I was director of foundation relations. And the first thing I had to find out is, What is a foundation? How do you have relations with it? I knew almost nothing about that. The National Science Foundation I knew something about—we're talking about the private foundations.

ERWIN: So you handled that, as well as government contracts?

BONNER: Government was not really a part of my portfolio, as originally understood.

ERWIN: But it soon became a part? Well, we're getting ahead of the game now.

BONNER: Yes, yes. So I discovered there was an excellent book, a guide to foundations—a book about so thick, which lists them and tells you brief stories of how much money, who they are, and so forth, and refers you to the Foundation Library [Center] in New York City for further information. Before I came out here in '65, I paid a visit to my then opposite number at MIT to see how they played the game, get some notions. In the fall of '65, I made a trip to New York to visit the Foundation Library, maybe call on a couple of foundations while I was there, to establish myself, which I did. Found out what sort of information was available in the library, like complete sets of the government returns they are required to file every year about income and what they did with it. And I visited the [Alfred P.] Sloan Foundation, which had been quite good to us in the past. The Sloan building here, as you know, was supported by them. And this is an aside: I was lucky enough that visit to be present in New York City at the time of the great blackout.

ERWIN: Oh, I remember that.

BONNER: Yes, and everything turned off. In the early evening, I was in the lobby of the hotel where I was staying, just a couple of blocks from the Foundation Library. I was just about to get in the elevator to go up to my room on the 22nd floor when the lights began to flicker, and I stepped back a bit, and they flickered again a couple of times and went out.

ERWIN: So you weren't on the elevator?

BONNER: Not on the elevator and not on the 22nd floor—marooned on ground level. It was a fascinating experience. The crowds on the street were generally cheerful, crowding around people with battery radios to see what they had to say. The bars were working, restaurants were working—any of them based on gas for cooking. Candles on the tables. Principal trouble at the bars was that of money and making change, because they couldn't open the cash registers, on account of they were electrically operated. So you needed exact change or some combination thereof to buy a drink. I finally went back to the hotel, and they, at the foot of the stairs, in a friendly manner, handed me a packet of matches and showed me the way, and I climbed up to the 22nd floor, went to bed. And by morning the elevator was running.

So, anyway, I came back from that trip and started putting some of this in practice, writing, helping write some proposals to foundations. I did some of the final writing on the proposal to the Oscar Mayer Family Foundation, which got the funding for the Oscar Mayer Dome at Palomar, and worked with the Humanities [Division] people on a proposal to the Rockefeller Foundation for a sort of interdisciplinary project they had then in mind. I've forgotten what it was all about, except I do remember it was with a great deal of trepidation that I challenged the writing of [history professor] David Elliot and Hallett Smith, then the chairman of the [Humanities] Division, on how to write a proposal. I figured I knew a lot more about that than they did, having written many, many. I was quite relieved when they agreed with me that, yes, that was a better way to say it.

ERWIN: So studying Shakespeare does not necessarily lead you to the right state.

BONNER: Does not necessarily [lead you to] write good proposals. And that proposal, too, was funded. But I became rather rapidly interested in other things as well. I didn't find the study of

foundations an all-consuming interest. I began reading the institute books, trying to find out how we ran. I knew something about reading business statements and things of that sort from my industrial experience, including experiences trying to stir up new acquisitions for Hercules. So I went back then to the institute financial reports over the last several years, see if I could make some sense out of them. One of the things I found out was that they had been consistently—I guess "falsifying" isn't the right word, but hiding a big part of the government contribution to Caltech, the part that came in in overhead and such—and maintained that the total government contribution to the Caltech budget was on the order of forty-eight percent. Less than half, rather than, in fact, if you read it more carefully, it was on the order of sixty to sixty-three percent. I complained in writing to [Caltech president] Lee DuBridge about this. Nothing particularly happened, except that ultimately they began reporting openly what we did with the government. I also tried to get a clearer picture of government funding—where it came from and what happened to it. There was a considerable hunk of it that went into the management fee for running JPL [Jet Propulsion Laboratory]. There were direct contributions from the National Science Foundation, National Institutes of Health, et cetera. They seemed an important part of our income. Somebody certainly ought to be paying more attention to this. I also, because we were trying to raise so much money for buildings, got interested myself in how we knew what these buildings would be like, how big they were going to be, how much money they were going to cost, what they were going to do. I found out that a lot of the information that went into development planning was simply horseback guessing by one man in the physical-plant operation, whose job it was, with relatively little input from anybody else. That I took a somewhat dim view of and tried to straighten out, asked more questions about who and what and why.

And, as is likely to happen when you ask too many questions, you're likely to be given the job of straightening it out. I asked too many questions. That's why about a year and a half, a little over a year and a half after my arrival, I was moved out of development and became assistant to the president for facilities planning. They tacked onto that something about government relations, too, but I never did a great deal of that, although I did pay a visit to the National Science Foundation, visited our congressman in Washington a time or two, that sort of thing. But the facilities, the buildings, the planning thereof, was of real interest, and I was able, I think, to help straighten some of that out and get our planning in shape. We were then working

toward the fund-raising campaign that was kicked off, I guess, in the fall of ’67. “Science for Mankind” was our slogan, so it was helpful to have a better feeling for what was really wanted and needed, in various buildings and building requests.

ERWIN: In a memo in May of 1967 to Dr. DuBridge, you said there were thirty projected building projects over the next five to eight years at a projected cost of more than $50 million, which doesn’t seem that much money right now.

BONNER: It took less to build a building in those days.

ERWIN: Right. But *thirty* building projects is an enormous number of projects, even if some are just rehab types of things. So why, all of a sudden, this big building boom?

BONNER: A big share of that was the enormous optimism, the projection that many people were making that since Caltech had been able to grow at a compound rate of almost fifteen percent a year since the end of World War II, it was probably going to continue forever.

ERWIN: Grow in what sense, people or programs?

BONNER: Funding, programs, and people. People, primarily at the graduate student and postdoc level. Faculty doesn’t grow that fast; the undergraduate student body essentially grew not at all. The graduate student body grew rather rapidly, because it takes, as I knew myself, all that slave labor to get the work done.

ERWIN: And were there also the returning GIs looking for a niche?

BONNER: That was a glitch that went up sharply in 1945, ’46, and was essentially over by ’49, ’50. Back into a more ordinary pattern.

ERWIN: You were talking about the optimism of the times.

BONNER: It was not until after World War II that government funding existed at all, really, and this was in effect a conscious decision to continue the policies and workings of the Office of Scientific Research and Development on a civilian basis and which ultimately resulted in the National Science Foundation. Because of the difficulties in getting the National Science Foundation through Congress, though, the navy, the Office of Naval Research, took over many of those functions and was the principal distributor of money for several years. So it was the Office of Naval Research supporting things like the accelerator work at Kellogg Radiation Laboratory and a variety of things of that sort.

ERWIN: Why the navy, as opposed to the army or the air force? They just had it in place?

BONNER: Because the navy had a more liberal record in dealing with such things, and they had a viable structure in existence. They understood how to do these things better than anybody else. They were able and willing to continue to the satisfaction of our folks here, and we had a lot of their money. Anyway, with the government in the act, the funding rose. The amount of money given to schools, particularly to this school, rose something like fifteen percent a year. I guess the number of people rose something like ten percent a year. If you project that, you see you're going to need an awful lot of space and an awful lot of everything. In fact, if you project it far enough, you find in the next twenty years that practically everybody in the U.S. is going to be a scientist working in a laboratory. You know that it's not likely to happen. As we looked carefully at the funding patterns in 1967, '68, there was a curve that had really turned over. We weren't quite sure what it meant yet. In another couple of years, we knew exactly what it meant: The money machine had been shut off. We weren't going to grow. We'd have to adapt to a more nearly level structure, which means a reexamination of all those building requests and a lot of reductions. They were just simply pie-in-the-sky sorts of things.

ERWIN: So in fact the thirty building projects did not all come to fruition?

BONNER: If you count the buildings on campus, you won't find anything like that many.

ERWIN: Even with rehabilitations and off-campus things?

BONNER: Off-campus things? I guess Harold Zirin's solar laboratory at Big Bear would have been one of those.

ERWIN: And the Owens Valley [Radio Observatory], would that have counted in that?

BONNER: Owens Valley—well, not really. That was a National Science Foundation instrument project.

ERWIN: It was already—

BONNER: More than a building—

ERWIN: Was that a government facility?

BONNER: Caltech owned and operated it, but the funding came initially entirely from the National Science Foundation.

ERWIN: I know you were pretty involved in that. I've seen a lot of paperwork on that project. But there was still an appreciable amount of facilities work to do, and a lot was accomplished during that period.

BONNER: All this, of course, went into the "Science for Mankind" program. The fund-raising kicked off in, I believe, the fall of '67. And if you look around the campus, you'll see a fair number of buildings that were accomplished during that period. Baxter [Donald E. Baxter, MD, Hall of the Humanities and Social Sciences], the humanities building. I guess the South Mudd development could count as part of that. It had some Mudd money and some National Science Foundation money—it was built in that period. What else? The new student houses were part of the 1957 campaign. It would help to go through a complete list, like the list in the catalog of all the buildings—see who was there and who was not. Anyway, that's what happened. And, as I say, as assistant to the president I had the responsibility for trying to keep it more or less straightened out and reasonably understandable by President DuBridge and other people. I also

found myself working with the provost quite a bit, as the president's number-one principal support.

ERWIN: Now, who was the provost?

BONNER: It was Bob [Robert F.] Bacher, the first provost, and provost through then until Lee DuBridge's retirement [1969]. One of the things I got involved in with my facilities connection was a joint proposal that wound up ultimately between Caltech, UC Berkeley, and the University of Michigan for a 100-meter radio telescope dish at Owens Valley. We formed a consortium, the Associates for Research in Radio Astronomy, I think. ARRA. I was signed up as executive secretary of the operation, went around collecting signatures, and so on. I helped present it at the National Science Foundation, but it didn't fly. The money went instead to a big operation in New Mexico, an assemblage of twenty-seven smaller dishes, in an array that could simulate a very large aperture. It's been a very successful operation, too. But we lost. We dismantled the organization and went on our way to do other things.

ERWIN: Speaking of the president and other people—you mentioned the provost. How about the trustees at this point? Can you say something about your relations with them?

BONNER: One of the things I encountered in my facilities-assistant days was working with Henry Dreyfuss, who was a very thoughtful, very helpful man. I guess he was chairman of the trustees' facilities committee and a member of the Campus Planning committee, of which I was then a member, so we met and talked about what was to be built and what it would look like and things of that sort. Henry had really very constructive views—how things should be built; form versus function. He was one of the real innovative designers, you know. Postwar designer of many things, like the new telephone, the new telephone booth. He described part of his operations, and part of his urge to do something about the standard telephone booth was owing to an occasion when he and several other people had to stand around an old-fashioned telephone booth—the door, at that time, folded in—watching a man die of a heart attack because they couldn't get at him. He was wedged against the door, and you couldn't open the door.

ERWIN: Oh, my!

BONNER: So they don't work that way anymore.

ERWIN: What an awful thing!

BONNER: Yes. It impressed Henry. And of course another trustee that I'd known for quite a long time was Simon Ramo, who was contemporary with me in Salt Lake City. I was a year ahead of him at East High School, also a year ahead at the University of Utah. Si was a really excellent violinist. He and I both played in the McCune School of Music Symphony. It was the closest approach we had at that point to a Salt Lake symphony orchestra. And we both played in the University of Utah Symphony Orchestra. My senior year there, I remember, he was the concertmaster. I was the oboe. As I've said to people since, that was one time that Si Ramo had to listen to me. He had to tune his violin to my A.

ERWIN: Right.

BONNER: He was, as I've said to him at another occasion, always a year behind me in everything except making money.

ERWIN: Yes. [Laughter]

BONNER: That was the extent of my contacts with Si. Other trustees I got to know casually through attendance at trustees' meetings later on. That comes in the next chapter.

ERWIN: OK.

BONNER: We can work up toward that by saying that another thing happened, even back in my foundation-relation days: The student movement at Caltech was beginning to move. This relates to Joe Rhodes—Joseph Rhodes—and what I've referred to for a long time as the Rhodes Revolution. This really had quite a lot to do with reshaping Caltech and its approach to things. Joe Rhodes was a very charismatic young black lad from Pittsburgh. Joe did not come on any kind of special admission program. He was admitted strictly on merit in 1965. In his sophomore year, the spring of 1967, he was elected president of the student body. In order to do that, the

student body had also to amend their bylaws, which required that the president be a junior at the time of election. So Joe was elected as president as a sophomore and re-elected the following year for a second term, which also has never happened to anybody else.

ERWIN: Even since.

BONNER: Yes. So then Joe—it must have been in the spring of his first year—Joe called a big meeting of students and faculty in Beckman Auditorium to discuss student-faculty relations, what the students should be doing, and so on. I did not attend it, but if I'd known what all was going on, I probably would have. At that meeting, there arose the proposal that students be made members of faculty committees so they could present student viewpoints. This was taken seriously, and within a couple of years there was an amendment to the faculty bylaws, assigning student membership to all of the faculty standing committees and inviting student representation to meetings of the Faculty Board. This has been happening ever since. In my own work with faculty committees, students have, by and large, been a responsible and helpful addition. Joe also had a very strong feeling that the students must do things by themselves; they must be not entirely dependent on the faculty for everything. They must have their own devices and projects. And it was he, then, who announced the creation of the ASCIT [Associated Students of the California Institute of Technology] Research Project, ARP. And the focus of the ASCIT Research Project was going to be pollution in the Los Angeles basin, smog, smog control, all the things that go with it. To this end, Joe was going to gather up Caltech students and visiting students from all over the country to come here as student research associates of ASCIT to participate in this activity. By the summer of 1968, he had a reasonable group assembled from here and elsewhere. Exactly how many I can't remember, but ultimately there were on the order of a hundred and fifty, possibly as many as half from other schools, associated with the various projects. The summer of 1968, though, was the first really serious start. Earlier, probably late '67, they had started trying to find out how and where they could raise some money. A small delegation came to see me about foundations. I told them what I could about private foundations, how they might get at them, and I let them read my foundation book. I guess they found me helpful, because they came back for advice on various other occasions, particularly

after I had become assistant to the president. Ultimately I was one of those who pushed for the feasibility of a student-operated research project and of outside funding to support it.

I discussed all this, of course, with the provost and told him I didn't know why they were coming to me. "I would have thought the dean of students would be handling these things," I said. Bacher's response was that I really didn't understand the situation, which was his way of saying that maybe that was the way it should be but that with the present dean, it wasn't. So I continued to advise. Ultimately [professor of materials science] David Wood, who became associate dean [of students] about then, and I became sort of the ties, the links, between the ASCIT Research Project and the institute, particularly the Sponsored Research committee, which needed to approve all proposals for outside research support. Initially Joe Rhodes and his group were able to wheedle money from a couple of small local foundations, enough to get started on, but to really move they needed support from the larger ones. A proposal was made to the Rockefeller Foundation, and Joe must have been very persuasive, because they liked the project but were leery of the idea of giving the money directly to the students. We, the institute, had to serve as intermediary. Remarkably, this was rather easy to work out. We would handle the money and certify to the foundation that it had been used in some useful way but without our telling the students how to do it. This was a little delicate, but with goodwill on both sides it worked out very well. Eventually a similar arrangement was worked out with the National Science Foundation.

ERWIN: This was quite an ambitious project.

BONNER: *Very* ambitious! Joe didn't think small.

ERWIN: What impressed me when I read about it at first was the fact that he was going to collect students from all over the country.

BONNER: And he did.

ERWIN: What was the reasoning behind that? Why should this be more than just a Caltech project?

BONNER: To get, I guess, a cosmopolitan view—not just a narrow Caltech science-oriented view but with non-scientists from elsewhere a broader, more society-oriented view. Though he had a major in physics, Joe was really much more of a humanist himself and ultimately graduated with a major in history.

ERWIN: Did this project have a relationship to what was going on politically in the country at that time—the Vietnam War?

BONNER: It was part of the general student unrest of the times but took an unusual form, a *constructive* one.

ERWIN: Constructive form. Exactly.

BONNER: We are going to save the world. You can't beat that for a starting line.

ERWIN: It seems to have gotten a fair amount of good press, this movement at Caltech.

BONNER: Oh, yes.

ERWIN: Because in other places students were doing very destructive things.

BONNER: It was well regarded. As I've said, we got ultimately a grant from the National Science Foundation. The terms and conditions we set up here for dealing with the foundation and for dealing with the students became, almost exactly, the basis of a National Science Foundation general request for proposals the following year. They set up their own program for student-originated research programs.

ERWIN: That was quite a good result.

BONNER: Yes, even the National Science Foundation decided there was something here worth following.

ERWIN: One of the other points of this ARP was to train so-called, at that time, underprivileged technicians. Did you extend some kind of scientific training to kids or young people who didn't have access? I wondered if that came off at all?

BONNER: Not exactly in that form. One part of Joe's intent was to bring in underprivileged black students, from predominantly black schools.

ERWIN: Bring them to Caltech? Or bring them into the project?

BONNER: Bring them into the project, really—not into Caltech. In the first workings of the ARP in the summer of 1968—last year was the twentieth anniversary of that great summer—they divided up into several teams. There was a teaching team to help get the message across in the schools. This one carried into the next year, and they were able to do a little team teaching in the schools. There were several science projects, like "What is the nature of smog?" "What do you do about it?" The sociology, like carpooling and all of these. They all wrote reports, none of which lasted—I don't think you can find a copy anywhere. They paid themselves small stipends from their foundation grants, and all of this carried on into the next year. In the fall of '68, Joe was successful in bringing in a group, in a body, of six or seven students from a small black college in New Orleans, the name of which I've forgotten. This was not an entirely successful operation, because these students seemed to feel out of place. They kept together as a group, rather than mixing in. There was some interchange, though, with black groups in other schools, and this led ultimately, in the spring of 1969, to what some people saw as a potential confrontation. A notice purporting to be from the Caltech chapter of the Students for a Democratic Society appeared on the bulletin board of a neighboring school and was sent on to us. This, presumably, had been sent all over Southern California and was an invitation to everyone else to come to a big rally on the Caltech campus on a particular day. SDS was big in those days as an activist organization, and this news caused a great deal of consternation in the administration. Their view was that because of the Caltech reputation, everyone from all over would stream in to meet on the Caltech campus. I thought they were overreacting, but that was the way we had to play it. The Pasadena Police got word of the event, wanted to know what they could do to help prepare for a mass march on our campus. We persuaded them to keep a very low profile if they possibly could. They did pretty well at that. Some of the students were

worried, too. They came to talk with me about how to organize, what they could do, who could do what. They brought in some of the leaders for me to talk to. They said, well, no, they really didn't expect very much to happen. And sure enough, the day came and not very much did. We were organized. People were out with walkie-talkies and so on. I was out there, watching what was happening, prepared to report to the provost and the president on a moment's notice.

ERWIN: Well, by this time, you had assumed a new official title, which we haven't mentioned yet, if this was in 1969.

BONNER: I had become by that time—you're right—director of student relations.

ERWIN: December of '68.

BONNER: I guess I didn't relate it in quite the proper order. That was another example of [how, if] you ask too many questions, you get the job. In fact, all of this is really part of another story. They all get so intertwined, it's very difficult to say which is which.

In 1968, Lee DuBridge was facing his sixty-eighth birthday, and sixty-eight was then the official retirement age. He was born in 1901, I believe. The trustees determined that Lee, with whom I had a very good working relationship, should retire. Lee didn't necessarily agree, but there was a certain amount of friction between Lee and Arnold Beckman, chairman of the Board of Trustees at that time. How and where it all originated, I don't know. Part of it, I think, went back to the time when the trustees were quite upset with Linus Pauling for his public pronouncements on atomic energy, atomic-bomb testing, all of those things, and his obvious pacifism, not to say communism. He was denied a visa, you may remember, for a meeting in Russia. The trustees—and particularly Arnold, who was one of the most conservative of all—apparently wanted to fire him. But Lee, even though he didn't entirely agree with Linus, stood staunchly in his defense and said, in effect, "Over my dead body." And they didn't. Which I give him credit for.

ERWIN: They didn't punish him?

BONNER: They did not punish him.

ERWIN: In the sense that they didn't fire him?

BONNER: I don't know what they could do, actually, except make a lot of noise. You don't fire tenured professors, except for really—

ERWIN: Or Nobel laureates.

BONNER: Double Nobel laureate.

ERWIN: Right.

BONNER: Anyway, the trustees had decided that Lee was going to retire on or about his sixty-eighth birthday. And—quite properly, in my opinion—they brought in a management-consulting organization, the name of which I've forgotten [Cresap McCormick & Paget.—ed.], to advise on reorganization under a new president. This is all in the files, too—their reports and all of those things. David Morrisroe came in with them, may I say. He was a member of the team.

ERWIN: Of the team, and then he stayed on.

BONNER: Yes. He went away and ultimately came back. So they brought this team in to analyze our structures. At that point, my guess is there were thirty or forty people who were convinced that they reported directly to Lee DuBridge. And Lee didn't seem to mind—he was willing to deal with anybody. But it made for a degree of confusion in all sorts of areas. I was one of those whom David Morrisroe interviewed in the survey, in my position as assistant to the president: What did I know about it, how did we do things? And this consultants' report strongly recommended that we organize in a much more formal manner, with a president and a group of, initially, four vice presidents in charge of various things. Provost and vice president, a chief academic officer who would be the principal vice president; a vice president for business and finance; a vice president for institute relations; and a vice president for student affairs. The trustees approved the report in the fullness of time, but Lee didn't wait for the full impact. In fact, he surprised everybody by accepting a job as science advisor to Richard Nixon and leaving for Washington sometime early in 1969.

ERWIN: So the implication is that he kind of one-upped the trustees?

BONNER: He'd left voluntarily. But in the meantime, here was this new structure, and Lee set about to implement it. Bob Bacher would be vice president and provost and Bob [Robert B.] Gilmore [would be] VP for business, et cetera. Lee then asked Bill Corcoran to be VP for institute relations and asked me if I would be VP for student affairs. I assured him that I was willing if he thought it was something I could do. He kindly assured me that he was sure I could do practically anything at all. So in the fall of 1968, these appointments went to the Board of Trustees at their annual Palm Springs meeting for ratification. After all, these were vice-presidential positions. I was invited to the meeting. By the end of the first day, nothing had happened, but Lee said, "Wait patiently." The second day, however, there were still no results, and the meeting was over. It turned out, as I built the picture later from Lee and from another trustee I had known rather well in the past, Arnold Beckman took violent exception to my appointment. Corcoran was just fine, but I wouldn't do, because, as he said, I had had no line-management experience. As a matter of fact, I had had quite a lot. Anyway, when Lee related this to me, I said, "Well, obviously, you'd better find yourself another boy." He said, "No, no, this is strictly between Arnold and myself. Has nothing to do with you, really." I said, "All right," because I was willing to go along with Lee. So what finally happened was that the matter was referred to the trustee personnel committee, which was a peculiar thing to do. Anyway, a small group of trustees then went around to look further into my background. Bill Zisch, who was one of the members, told me privately later that the worst thing they found out about me, talking to my former boss at Hercules, was that I was impatient with lack of decision. Which is quite true.

Anyway, the way it was resolved was to make it, then, not an appointment as vice president, requiring board approval, but a presidential appointment as director of student relations. Hence the title. But it was a *de facto* vice presidency. I reported directly to the president, and I attended all the various required meetings, such as the Faculty Board meetings, the Institute Advisory Council, the annual Palm Springs trustee meetings, and so on, but as director of student relations. That took effect in December of 1968.

Begin Tape 2, Side 2

BONNER: So it was in this context that the ’69 part of the Joe Rhodes activities took place. I was then officially the principal institutional representative in dealing with students. The fatal day of the SDS confrontation came. The Pasadena Police cruised around the campus in a beat-up jeep with a bunch of disreputable-looking people in it—you wouldn’t spot them as policemen from a distance—just in case something happened. The SDS contingent ultimately marched on campus. Probably thirty-five people, most of them our people, I think, had a small rally in Winnett Plaza, and everybody went away happy, I trust. We then had our own meeting on the subject. I gave a report to the faculty of my observations, what my part had been, what had happened. From all this, there evolved a remarkable thing called the Committee on Campus Disruption, something of that sort. It was chaired by [professor of geology] Bob [Robert P.] Sharp and was just simply known as the Bob Sharp committee. The charge to this committee was to discuss and consider all the possibilities and to set up an emergency organization to deal with potential future uprisings.

ERWIN: Oh, yes, the primary-alert list.

BONNER: The primary-alert list, yes. I was right in the middle of it. Of course, nothing ever happened. It was the sort of exercise, I guess, one feels one must go through.

ERWIN: Sounds like preparing for an earthquake, I suppose. Maybe even less important, I’m not sure.

BONNER: In an earthquake, you have a better idea of what to do.

ERWIN: That’s true . But at one point, I think, you wrote—this was when Harold Brown was president—you said we had more problem finding activists than having too many activists.

BONNER: Yes. True. We had very little. The closest we ever came, I think, to some real activism was the day after the Kent State, Ohio, shootings in May 1970. Joe Rhodes was no longer student body president. Anyway, we had at that point a mass meeting, which meant,

really, a relatively small group meeting—faculty, students, et cetera. At my invitation, the community relations officer from the Police Department came and was a very helpful, useful guy. We met in the second-floor meeting room in Winnett and talked about a lot of things. The student body president went out and addressed the student body later. A group of students came out and wanted the flag lowered to half-mast. The president and the provost were both absent, and Bob Christy, who had been appointed provost but had not yet taken office, refused to allow it. This resulted in a broken lock, ultimately, and the flag at half-mast anyway—that was the biggest violence. So we sat around in the Winnett clubroom, discussed violence and who did what and what our position should be. We didn't even make any great resolutions. As I said, that was the closest to activism that I ever observed. We sat and talked about it.

ERWIN: Yes. The intellectuals' approach.

BONNER: Yes.

LYMAN BONNER

SESSION 2

April 20, 1989

Begin Tape 3, Side 1

ERWIN: We'll be picking up where we left off two days ago. Dr. Bonner, you'd like to talk some more about the beginning of your tenure as director of student affairs.

BONNER: Yes, I would like to go back to that and pick up on a couple of items that I didn't put in before. When Lee DuBridge talked with me in the summer of 1968 about taking an active part in student administration, that pleased me. I also welcomed the opportunity to have more to do with students. I'd worked well with them in the few things that I'd done up to that time. Particularly, I wanted to try my hand at teaching. I said I'd gotten very tired of it at Duke, but really then it was the bulk of it, and the low quality of the wartime student body, that turned me off. Actually, in less pressed times I'd rather enjoyed it, and I think I was pretty good at it. So after Lee talked to me, I decided I would go and volunteer my services, see if anybody wanted a free teacher. I went first to Norman Davidson, executive officer for chemistry.

This reminds me of something else I neglected to put in. In my first year here, in 1965-66, the Division of Chemistry and Chemical Engineering—Jack [John D.] Roberts was the division chairman at that time—invited me to come to one of their lunchtime faculty meetings in the Athenaeum. That was in the spring of '66, and I talked with them some about the outlook for federal funding in the areas of their interests. Not too long after that, I got a letter from Jack Roberts to the effect that they had unanimously voted to recommend to the president that I be appointed an associate in chemistry, and this was accomplished. So as an associate, I had a measure of faculty standing, which made me feel very good.

ERWIN: You say a measure of faculty standing?

BONNER: Faculty standing, yes. That was a faculty appointment, a non-working faculty appointment. But nonetheless it put me in the catalog. But this appointment also meant that my primary loyalties there should be to chemistry, if I wanted to do anything. So I talked to

chemistry first, to see if there was any teaching I could do for them. I prefaced it by noting that the only teaching I'd ever done, other than one year as TA in freshman chemistry at Caltech—Arnold Beckman was in charge that year—was teaching physics at Duke. So Norm agreed cheerfully that I wouldn't be much use to them, and I went on over to talk to Carl Anderson [chairman of the Division of Physics, Mathematics, and Astronomy] in physics. Here I was welcomed with open arms and inducted immediately as a volunteer teaching assistant in freshman physics. I did this for three years and then graduated to sophomore physics, where I stayed until 1980. It was hard work, but I enjoyed it. I had an awful lot to learn. A lot had happened since I last taught physics twenty-five years before, and furthermore, physics at Caltech was a distinctly different level of physics from that at Duke. I enjoyed it, and, if I say so myself, I was good at it. The recitation part of the course was taught in several sections, and my sections tended to be larger than anybody else's, so I guess the students appreciated it. Furthermore, teaching brought me into contact with students at a level that administrators don't often get to. It seemed to me important that students have a chance to see that there are members of the administration who can talk with them comfortably at the level of quantum mechanics, relativity, and linear algebra.

ERWIN: Well, in that way you were anticipating something that President [Marvin L. "Murph"] Goldberger later did himself—namely, he taught Physics 1—

BONNER: Yes.

ERWIN: —himself, as the president of Caltech.

BONNER: As the lecturer, not as one of the troops in the trenches.

ERWIN: Right. He didn't have to get his hands dirty.

BONNER: I got in and explained the nitty-gritty and worked the problems for them.

Anyway, back to the '68-'69 year. I was now director of student relations, and I looked around to try to see what the challenges were, how you went about handling the large number of

individuals and committees who reported directly to the president and forming them into a chain of command that went through the director of student relations. It had its delicate points. In the catalog for 1968-69, I counted eight different faculty committees that dealt with various aspects of student relations: athletics, foreign students and scholars, graduate student relations, health, musical activities, relations with secondary schools, undergraduate student houses, undergraduate student relations. In many cases, these committees dealt with budgets, dealt with personnel. The Athletics and Physical Education committee dealt with personnel matters and with budgets. The Foreign Students and Scholars committee had just recently created something called the International Desk, with Ingrid Gumpel presiding, and her hiring and salary and budget were their responsibility. Music [Musical Activities]—they had responsibility for Olaf Frodsham and all the glee-club activities and also for the conductors of a band and an orchestra. Undergraduate Student Houses had somewhat less to do directly, but there they had some responsibilities that were more business-oriented than faculty-oriented. Undergraduate Student Relations was in effect a committee set up to try and handle many of the things a director of student relations should be attempting to deal with.

ERWIN: Had these committees been in existence a long time?

BONNER: Quite a while—yes.

ERWIN: And had come into existence at various times, I assume.

BONNER: Yes. They sprouted as a need arose. The answer was frequently, "Well, make a committee, and they will take care of it." So the first thing I needed to do was to go around and become acquainted with the chairmen of the various committees, talk with them about their activities, how they felt about not being responsible for budgets and people. By and large, I found them responsive and helpful and perfectly willing to step out of that line, so during the course of a year or two we got things better organized. The music people, for instance, came to me for budgets, looked to me for hiring and promotions and things of that sort. I think student health is a separate matter that we might want to make a small chapter of later, but that, too, had to be dealt with. Anyway, that's just what being director of student relations entailed. I figure I was rather successful at it and had things going reasonably well after two or three years,

although, of course, no group of people can all be happy all of the time. I did manage to consolidate budgets, and in that period budgets were somewhat of a question, because Harold Brown had come and the rules had changed. Lee left early in ’69 to go to Washington and Harold left Washington about the same time—

ERWIN: They switched coasts.

BONNER: Yes, and he came west. About that same time, too, the whole impact of the federal funding turndown that we were talking about not long ago became apparent to all. The necessity for running a much more austere shop than had ever been necessary before fell on the shoulders of Harold Brown. He had to cool the growth, to reduce the appetites, cut the staff where possible, to save money. So this reorganization of budgets all had to take place in the atmosphere of an austerity budget program, which didn’t make it any easier. There was also an urge to cut personnel, not to hire a new coach when one of them retired, and things of that sort. This was hard on everyone all around. People gradually got used to it, survived it, and good times came again later.

ERWIN: Right. It seems that actually Brown was quite successful in steering the institute through that difficult period.

BONNER: He was very, very good. I have a great deal of admiration for Harold, his understanding of the problems, his ability to deal with them. He seemed rather a cold fish to many people, but in my dealings with him I found him always fair and responsive. If you had a question, he’d rather have it in writing. If you sent him a note in the afternoon, you’d have an answer, scribbled on your same note, on your desk the next morning. The kind of response we’re not used to in academia. He was fresh from big organizations and knew how to do it.

ERWIN: Apart from the budgetary problems, what were the big issues as far as the students were concerned during this period?

BONNER: The activities of the Rhodes Revolution continued, of course, through 1969 and 1970. Harold was quite accepting of that. He knew he was not here at the beginning of it, but he

realized that this was an important movement to students, and important to faculty, and was going reasonably well. He got involved, of course, in the case of the SDS invasion that we have talked about. He was also the one who appointed the anti-disruption committee to set up procedures to deal with the next one, of which there weren't any. He was also present at the time of the Kent State massacre, so-called, on May 4, 1970. On that occasion, as I said, there was some scuffling at the flagpole, but no riots, marching on the president's office, or things of that sort. Just a student and faculty meeting to talk about it.

ERWIN: How about drugs? Was this ever an issue, and how was it handled here?

BONNER: Drugs were, by that time, increasingly moving in. In fact, several years earlier, Kenneth Eels, who was a full-time psychologist working for the Health Center and very interested in students and student matters and also interested in and observant of the drug scene, sent out questionnaires, did what work he could, and prepared, sometime in the late 1960s, quite a thick volume on drugs and the Caltech student. He put down about as well as he could what he found out about usage, about the various kinds of drugs, their physiology, their physiological effects, where they came from, how much student use there might be, and couched in language to allay—he hoped and all the rest of us hoped—the fears, possible hysteria, of trustees and people of that sort. To show that it was not unique to us and was a manageable situation. In later years, we even had a drug bust or two on campus, when some ambitious students thought maybe they should deal a little, and incautiously. There has been student participation in all aspects of the drug scene—LSD, all of those. We went through it. Some students were badly hurt, most students were not. We were not a standout in that respect; we were somewhere in the mainstream. But, yes, drugs were here, still are.

ERWIN: Yes. Maybe that brings us to the question of student health.

BONNER: Yes. I had occasion to look into the history of health services at Caltech when I became responsible for overseeing the activities of the student Health Center in 1969. In the course of this, I learned, to my amusement, that I had participated in a very important phase of student health care at Caltech—namely, the very first physical examinations for incoming students. According to the catalog a couple of years later, this was in the fall of 1929, when I

came as a student. They hauled us all off to what was then Pasadena Hospital, now Huntington Hospital, on a Saturday morning for a physical examination by the doctors there. I passed the exam, but they told me I was too skinny—a hundred and twenty-five pounds. Furthermore, it was also proudly announced in the catalog, a medical consultant, E. D. Kramers, MD, would have an office in Kerckhoff Laboratory and would be available one hour a day for student consultation on health matters.

ERWIN: What we might call a modest beginning.

BONNER: A modest beginning. Dr. Kramers was one of the physicians who early took an interest in community health, group health, things of that sort. I remember in my graduate student days going to consult him, late in my career. Doing my thesis and so forth, I was having more headaches than I thought I ought to. He sympathized and gave me more aspirin. Well, things went along like that then until after World War II. Then, about 1946 or ’47, there came the great influx of returning GI students. The student population went to previously unheard-of heights, and along with it came a greater interest in better health care for students. Over the period of a couple of years, there was set up a genuine health center clinic and infirmary—beds for five or six people. It was set up in a leftover temporary wooden building, down at the east end of the campus, between Thomas Lab [Franklin Thomas Laboratory of Engineering] and the Athenaeum.

ERWIN: So prior to that time, there was no infirmary even?

BONNER: Until that time, there was nothing.

ERWIN: Nowhere you could go if you came down with something medium serious?

BONNER: No. For the first time, there was an infirmary, there was twenty-four-hour, seven-days-a-week nurse attendance, a physician on duty for two or three hours every weekday afternoon, and, as I say, a several-bed infirmary where students could be put to bed and watched over. After we went through the formative period, by about 1948 or ’49 it settled down to the pattern it had for many years. Dr. Richard Webb, by that time, was medical director, director of

student health services, and Alice Shea was head nurse. Both of them stayed in office until after I came into the picture in 1969. In fact, neither one retired until a few years later. In 1957 there was a major fund drive—the first the institute tried to put on, I believe. As part of this, the institute provided quite a shopping list of buildings that needed to be built. One of them was a new and properly designed student health center and infirmary. Mrs. Archibald Young, according to Dick Webb, went down the list until she found the cheapest one on the list and said, "There, I'll buy that as a memorial for my husband." Hence, the Archibald Young Health Center, located on the south side of California Boulevard, across the street from most of the campus. Sometime back there, too, Webb had persuaded the president that we needed to provide some psychiatric consulting, and a local psychiatrist, Dr. Dan Siegel, was brought on the staff for consultations, an hour or two twice a week, I believe. Then somewhat later, about 1960, Kenneth Eels, whom I mentioned a bit ago, was brought in as a full-time psychologist. He was the first full-time counseling person we'd ever had, and he was very helpful, talking to students and faculty, and, as I say, doing the drug survey. Those people, too, were in place when I came in 1969, although Eels, for reasons of health, informed me as the first item of business that he was retiring that year. So we needed to find a replacement for him, which we did, with Eels's assistance, in the person of Ian Hunter, who was with us then for the next six or seven years. A little later, in conjunction with the bringing of women students to campus, which I think we will also have a separate chapter on, we brought in a second psychologist, a woman, Nancy Beakel, to deal with women's problems, and also a very part-time woman physician. It turned out, in fact, that the women [students] weren't all that fussy. Some of them preferred to see a man, but some of the men would rather see a woman. So it worked, it just didn't come out exactly the way it was scheduled. Gave us a wider range of choices, available to everybody.

Later on, as part of the Brown austerity, we endeavored to cut costs wherever possible, to reduce our budget, year by year. If you're told you can have three-percent more money for next year, and you know there's a five-percent increase in the cost of everything, you have to look around for things to cut. After studying the Health Center records very carefully, several of us were convinced that one of the greatest wastes of money was keeping the infirmary open weekends. Students were perfectly willing to be sick during the week and kept in the infirmary. But come Friday they were ready to go and do something else. The population was so close to zero on weekends that I declared weekend closings. We stayed open Friday night till early

Saturday morning. Closed then, until Monday morning. This produced a great deal of antagonism, principally among the nursing staff, who saw their jobs being threatened. Also a certain amount of student bitterness, which hung on as one of the perennial gripes for years and years. May still be, for all I know.

ERWIN: Is the Health Center now closed on the weekends?

BONNER: Oh, yes. It never did open again. In fact, once, just to see how it would work, and in response to critics, we ran a small trial period, saying we were going to be open and if you have any interest at all, come on out. Well, nobody came out. In fact, gradually the infirmary function itself has gone, has completely disappeared. Styles in medicine have changed enormously.

ERWIN: Well, that's true.

BONNER: Back in the 1960s, if a student had the sniffles you put him to bed for three days and poured hot juice into him and all sorts of things. Now if you've got the sniffles, they give you a pill and send you back to your dormitory. I think it works much better.

ERWIN: Probably it does.

BONNER: It's more like what you get at home. Your mother's not going to put you to bed and coddle you. She's going to give you a pill, tell you to keep out of the way.

ERWIN: Right. But as that aspect has sort of dwindled, the counseling aspect, I'm sure, has increased.

BONNER: The counseling aspect has increased. Several years later, one of the bits of lore I learned in trying to be administrator of the student Health Center, offering both medical and counseling services, is that the two services are never entirely happy together. And that was one of the points in the great nurses' rebellion, when we started closing weekends. In a reorganization when Dr. Webb retired, I appointed Dr. Greg Katabgian, who had been an

assistant there for a while, as medical director, and appointed Ian Hunter, our new psychologist, to be the administrator—the person to keep the salaries, budgets, things of that sort, in line. But it turned out ultimately to be a disaster. His [Hunter's] style of humor, for instance recommending to the nurses that *Deep Throat* was a movie they must see—to a bunch of conservative, old RN's—was a very bad show. This and other things ultimately led to a bust-up, which resulted in our letting Ian Hunter go, and also the then head nurse, and sort of starting over again. At that time, we brought in a consultant on student health centers who had been recommended to us. He assured us that our pattern was practically standard in the industry. You try to mix the two disciplines under a management of one or the other, all hell's going to break loose sooner or later. What we gradually managed to do was to make some physical rearrangements in the building, separate the two services completely, separate entrance doors, separate receptionists. And they get along very happily now.

ERWIN: Does this bring us to the whole issue of women on the campus?

BONNER: If you consider women an issue, yes.

ERWIN: Well, it was an issue, I know.

BONNER: It was, it was an issue. Rumor has it that old Arthur Amos Noyes said at one point that "No, there should not be a regulation prohibiting the admission of women to the California Institute of Technology. Because if it was a regulation, you can find exceptions to it. If it was a custom, you can't." That custom was maintained for many, many years. In the early years of the institute, there had been women, back before the big revolution of 1910, when we started moving away from the Polytechnic Institute and toward the California Institute. At that point, a decision was made that women would no longer be admitted. That held true until 1954, when a woman graduate student was admitted. This was alleged—although Jack Roberts insists that it's not strictly true—to be part of the price of inducing Jack to come here from MIT. He would bring some of his graduate students with him, including a woman, and that was the first break in the pattern. Over the years, then, we had a few women graduate students. But never any undergraduates. This was considered again, though, in the late sixties by a faculty committee appointed for the purpose. They came up with a series of recommendations, saying that, yes, it

was their opinion that women could not logically and properly be excluded from the benefits of a Caltech education. This recommendation then, passed by the faculty and approved by the administration, went to the trustees. In setting up this recommendation, they anticipated an objection that had come up, I believe, in earlier discussions—that admitting a certain number of women would displace an equal number of more deserving young men who should be at Caltech. The faculty recommended that in the process of this change—no connection whatsoever, you understand, just in the process—it would be a good time to increase the average size of the incoming freshman class from about two hundred to about two hundred and twenty. The two hundred and twenty was related to the fact that, as far as any of us could guess from figures from the College Entrance Examination Board, the ratio of high school women to high school men taking the Level 2 mathematics achievement examination, which we require as part of our admissions process, was about one to ten. Our best guess was, then, that if we recruited vigorously, maybe we could get something on the order of twenty women in the class. So this is what went to the trustees. We also had to present the rest of the picture—how we would house them, take care of their health and general welfare, and so on. One of the things we had set up for the summer of 1969 was a rehabilitation of two of the older group of student houses which had fallen into great disrepair. We were able to assure the trustees that this rehabilitation would not only bring them really up to date but it would include expansion of the so-called RA, or resident associate, quarters. Resident associates had always been part of each house, usually a graduate student or junior faculty member.

ERWIN: A single male, right?

BONNER: A single male, of course. What else would you have in that kind of community? For a coed community, though, we would improve the RA quarters to accommodate a married couple, so there would also be a woman consultant available. Also we would provide means for separating the women's section of the quarters from the men's section. Dividers that did not have to be in place but could be invoked at the will of the student. I think these amounted to curtains, as a matter of fact.

ERWIN: Nothing very rigid, then.

BONNER: Nothing that would violate a fire code or anything of that sort. Also, as I mentioned earlier, we said we would bring in the services of a woman counselor and also seek participation of a woman MD. On the basis of all of this, the trustees, I guess in late spring or very early fall of ’69, gave their go-ahead. It had to be early ’69, because we had to plan the campaign to announce to the world, “Caltech wants women.” So in the 1969-1970 admissions campaign, letters went out to high schools all over the country saying, “Women, qualified women students sought.” We received a quite respectable number of applications from interested women. I was a member of the Freshman Admissions committee at that time, and one of our functions was to go out and travel our territory interviewing prospective students. My territory was in the Middle West, and in the spring of 1970 I interviewed my very first woman candidate. I was a little baffled when, to one of the questions we frequently asked, [which] was, “What hobbies do you have?” she said, “Well, I sew my own clothes.” I didn’t know how to evaluate that in comparison with, for instance, grinding telescope mirrors or building computer networks or something of that sort. Anyway, she was admitted and did very well, is now a successful MD somewhere in the Middle West. So it worked. And in the fall of 1970, then, we brought in a class of two hundred and twenty students altogether, which included thirty women. This was at the freshman level, and in addition we had two upper-class transfer-level students. So we started off with about fifteen percent of the entering group at that time. And we were able to maintain an average of close to fifteen percent. It fluctuated all the way down to ten at one point, then got built back up again. I understand the outlook for the coming fall is even better than that.

So women have been a success at Caltech. We began, as I said, making a show of protecting women from the men, and vice versa. After about the third year, we had some women students in each of the seven houses, married RAs in most of them, and no pretense any longer that we could control the migration and intermingling that inevitably takes place, the diffusion process. Ever since, the houses have been—to the extent that the various students want it—completely integrated. I’m sure it’s hard on some people, makes special problems of its own. We’ve tried maintaining separate women’s quarters for those who wanted them, but the number who really wanted to set themselves off that way has been so small that that’s not really been practical either. There are problems there that haven’t been solved yet.

We have had some very good women students. We've had some very good women graduates. Our retention record with women is, on the whole, slightly better than with men. In academic performance, the two sexes are approximately indistinguishable. We find approximately the same percentages in the upper quartile or the top half. So it's been a successful experiment.

ERWIN: Was there a significant resistance to the idea of admitting undergraduate women, do you think, and if so, what segment of the community did it come from?

BONNER: In my observation, the principal opposition came from certain macho groups of students. Said they didn't want their house to be defiled by women. Of course, they all ultimately did [accommodate women], but there was a great deal [of resistance], particularly the first few years. I'm sure it still persists—a lot of chauvinism there, male chauvinism.

ERWIN: Among the students more than anywhere else.

BONNER: Among the students, yes. After all, here's a minority, and given a minority group, the tendency is to jump on them, I guess, and put them down if you possibly can. Takes a strong character to resist a lot of that.

ERWIN: Right.

BONNER: Well, anyway, there are now women, and I am sure they are here to stay.

ERWIN: So with this advent of women, there was certainly a different atmosphere on campus, I would imagine. Not only, of course, with the advent of women but after the upheavals of the late sixties and early seventies. One of my questions was asking you to compare the campus atmosphere, if this is possible, of the late sixties and early seventies with your own student days, and add anything else about your own student days, if you would like to.

BONNER: My own student days were quite different. Students in the 1930s, 1940s, even into the 1950s, were perfectly willing to be regimented, to be told what to do and then go and do it. You

didn't have any such luxuries as the ability to drop a class near the end of the term if you decided you hadn't liked it or were about to fail it—things of that sort. You signed up for something and you did it. You didn't ask any questions. You did what you were told. That spirit very much pervaded the era. I know when I taught at Duke, we had to call the roll every day, every class day. Or if it was a larger class, we'd assign them seats, then put down the numbers of the empty seats. And this was reported to the administration, and the student was allowed, in a standard class, three cuts in a semester. And if you took more than that, you were out. Really, really grim. At Caltech, when I was an undergraduate and also a graduate student and then well into the latter fifties, incoming freshmen were assigned to a section, typically twenty to a section—section A, B, C, D, and so on. Eight sections back in the 1930s, when I was there. They liberalized that later.

Begin Tape 3, Side 2

ERWIN: So things were a lot more rigorous?

BONNER: And furthermore, the students, through that entire period, had no electives in the courses they were going to take. I was a member of section G, for instance, and section G marched in lockstep from physics to mathematics to English to history to chem lab to physics lab and back to bed, so far as I know. That kind of an atmosphere. Electives didn't begin to creep in—the idea that students should have a choice of courses they were going to take—really hadn't been thought of, I think, until the latter fifties. Rigidity gradually began to go away, and it didn't completely disappear until the sixties and subsequently. Requirements for graduation also became less rigid. You could take the courses you wanted from a certain group, not the particular subset that the faculty thought you ought to. You had your choice of whether you would take history or literature or whatever. You still had to take your twelve terms of humanities and social sciences in order to graduate, but you didn't have to take them in any particular order or combination.

ERWIN: And I also note that at that point certain humanities offerings became options. You could major in history.

BONNER: In 1960, early 1960s, it was decided that students could actually take undergraduate majors in history, literature, economics. Joe Rhodes, as I mentioned earlier, dropped out of physics and finally graduated with a degree in history.

ERWIN: Was there still a kind of campus-wide prejudice against following that course of study here?

BONNER: It's not necessarily a prejudice, but the group of students we actively recruit and bring in here are students whose previous background is exactly the other way. They're students who've been actively engaged in—frequently to the exclusion of all else, to the extent they can get away with it—mathematics, chemistry, biology, physics, whatever they've been doing in high school. Nothing could interest them less than courses in humanities and social sciences. Some of them tend to resist, but they ultimately do it. Freshman humanities is typically what we used to call the freshman lit requirement. Write your themes, pass the course.

ERWIN: Now, as registrar, you would, no doubt, have a handle on this sort of question. I know I'm jumping ahead a little bit here. But was it true, for example, that in the late sixties and early seventies there was a greater interest, a sort of resurgence of interest, in the humanities? Did you see more people majoring in the humanities then, and then did it subsequently fall off?

BONNER: Yes, there was more of that in the era of social—what do you call it?—social conscience, social awareness. It swept campuses from coast to coast.

ERWIN: I don't know that anybody really knows what to call it yet. It's still too recent. It's usually the big, bad sixties or something like that.

BONNER: And students suddenly became aware that they were people and that there were other people out there that had legitimate interests in what the rest of the world was doing. And they came out of their shell. They did the Joe Rhodes things. They majored not only in English literature and such but in something called the Independent Studies Program. The Independent Studies Program was invented about then, in which a student, given sufficient faculty advice and approved by a special committee, could invent a program of his or her own that met enough of

the Caltech standards to be respectable but didn't fit any of the standard options. There was quite a surge of interest in the ISP. We had a number of ISP graduates in that period. But then, as it got into the seventies, it all went down again. The social activism, political activism, disappeared almost completely over just a few years. We got back to the old priorities of GPAs [grade point averages] and jobs.

ERWIN: Yet during this time, the late sixties, early seventies, Baxter Hall, the humanities building, was completed. Was Beckman Auditorium completed around that time?

BONNER: Beckman Auditorium was built before then. Beckman was built in 1964.

ERWIN: Well, I'm a little off on that.

BONNER: Baxter was built because it took a larger faculty to handle the larger number of courses that were being offered, and furthermore the humanities and social sciences people also sat up and began to enforce scholarly standards for their faculty. Instead of finding a nice, comfortable literature guy who'd like to come here and spend the rest of his life reading Shakespeare to Caltech freshmen, they wanted publications, books, reviews. Also back around 1970, social science was accepted as a graduate discipline. This brought in graduate students and an increased faculty in economics, social science, political science, all of those things. They did need more space, and they were going to expand from their then quarters in Dabney Hall into the new Baxter building, which would accommodate the overflow. That worked fine, except that then came the big earthquake of 1971 and Throop Hall was condemned. Many of us administration folk moved from Throop over into Dabney, evicting the few humanitarians that were left, crowding them back into Baxter.

ERWIN: Well, they've come back now, haven't they, to Dabney?

BONNER: They came back, and then a couple of years later they were evicted again, because the development folk needed more room. So now there are just a few—a few on the second floor, a few in the basement.

ERWIN: Well, you know, that reminds me of a topic that I think I should ask you about—that green space in front of Beckman Auditorium that is known as the Court of Man? Would you care to comment on that? Not Woman, I might add.

BONNER: Court of Man, that's right. So far as I know, and I think I do know, that term was coined by Robert Alexander, who was our campus consulting architect for many years. I got to know him rather well in my years as facilities consultant and sitting with him on the facilities committee and things of that sort, the Campus Planning committee. Bob and I talked quite a lot about various things. He was the design architect for the Baxter humanities building. He takes, I guess, a great deal of the blame for the fact that it is a thoroughly inefficient and, to many people's view, undesirable design. According to rumor, he and Hallett Smith, then chairman of the division, got together and agreed that what they needed, really, was a collegial atmosphere, with corners for scholars to meet and talk about this and that, strolling through, with your cap and gown on, and meeting a fellow scholar from another discipline and saying, "Tell me some more, now, about Plato, and what did he really think about that?" A totally unrealistic picture, but it resulted in a building which is designed on the inside like a honeycomb: hexagonal quarters, zigzag halls, and very inefficient use of space. Then there came a proposal for a companion building to be built to the west, opposite Baxter. The two of them, then, would flank the approach to Beckman Auditorium from the south. This new building would be, presumably, for the study of behavioral biology. It was hoped that Arnold Beckman and his wife could be attracted to this project to the point of being willing to put up money to help fund its construction. Bob Alexander was hoping to get the design contract, and after talking to enough of the biologists here, he realized he couldn't have hexagonal interiors. You can't build hexagonal laboratories. Still, he created a design that matched Baxter on the outside and was acceptable to the biologists inside. In order to help promote the project, he had his office build a scale model of the whole area: Beckman Auditorium here, the humanities over there, and behavioral biologists over here. The space defined by the three buildings would be the Court of Man. You'd have the practicing psychologists, the thinkers, over on this side, the experimental, the physiological behaviorists over there. And just like these little alcoves are to meet and talk in Baxter, they'd meet in the middle and discuss their joint problems. That turned out to be a good name, in the minds of many people. I think it pleased Arnold Beckman very much. The funding

was forthcoming, and the building was built. The term "Court of Man" was never posted there anywhere, but it's in the backs of the minds of many people. But it's not official in any way that I know.

ERWIN: I see, but you think the idea of it really played a big role in getting funding?

BONNER: I think it did. The fact that it was purely pie in the sky had nothing to do with it. It was an attractive idea.

ERWIN: Well, yes, I can see why.

BONNER: But it didn't take into account the strange nature of Caltech people. They don't want to talk to anybody anyway. Particularly if it's not in their discipline.

ERWIN: Right. Well, I think we've covered the ground up to about the period of 1975.

BONNER: Should we move up to 1975 quickly and then decide where you want to go beyond that? Nineteen-seventy-five was another watershed year in my life. As I said, I was appointed director of student relations, reporting directly to the president in 1968. In the spring of 1975, Harold Brown, who, after all, had not made the appointment—he'd inherited me—appointed a committee, chaired by Fred Anson of chemistry, to review the state of student affairs and to report to him any recommendations. The original proposal, as you know, was for a vice presidency. Exactly what form the committee recommendations took, I'm not sure. But clearly a part of it was that the committee felt strongly that it should be a vice presidency, and it should be occupied by a recognizable senior faculty member—a point of view with which I had no great quarrel. I had come in six years earlier as an outsider, really—an outsider in terms of Caltech experience. I didn't have the experience or the recognition that goes with being a senior faculty member. He can throw his weight around in the various councils. I didn't have any weight to throw. Harold accepted the Anson committee's recommendation, and in the spring of 1975 he called me in to tell me that he had appointed Ray Owen as vice president for student affairs and dean of students, effective September 15th. Beyond this, though, he said only that he'd leave Ray and me to work out our joint functions. Very strange kind of a thing, because it left me

essentially hanging in mid-air. He said, "Well, this space is now occupied by this guy. What are you two going to do?" I thought this would be no great problem. I didn't resent the move as much as perhaps I should have because, (a) as I said, I could see the logic of it, and (b) as I think I said earlier, I didn't return to Caltech for fame and fortune. I was surprised any came to me at all. I came for a pleasant, quiet career, and I was getting that and was very happy with my life here. I was perfectly content to continue in whatever other niches I could fit into. So Ray and I got together to try to work things out. I would continue to do many of the things I had been doing in relation to music people, health people, PE people, and so forth. Also I would retain the principal administrative responsibility for budgets and things of that sort. It turned out, though, that Ray Owen—I'm not alone in this opinion, I believe—can be a very difficult person to work for. He can display a violent temper, and he has his own notions as to what to do, despite what he may have said about delegation. If anybody comes to him, he'll take up their cause and run with it himself, and the fact that he may have delegated that responsibility to you is immaterial. Over a period of time, it became quite clear that most of the other people, the music people, and Lee Browne, the secondary schools person [director of secondary school relations and special student programs]—I had been rather hard-nosed with them—found that Ray was much softer of nose, more likely to sympathize with their causes and try to do something for them. So naturally they didn't come to me, they went to him. He welcomed it and spent infinite amounts of time with them, leaving me feeling distinctly unemployed. I also felt uncomfortable occupying the second-floor suite in Dabney that I shared with him, simply because I had been there, and he simply moved into the dean's office, which was distinctly less posh. So in spring 1977, Bill Schaefer, the registrar, got an offer from chemistry—I'd appointed him registrar, by the way.

ERWIN: Yes.

BONNER: He got an offer from chemistry to come and be their divisional administrator, and also to have more time for research, and this he couldn't refuse.

ERWIN: Was the registrar at that time considered more-or-less half-time?

BONNER: Half-time. Yes, Bill and I agreed at the very beginning that despite what it may have been once, it was not really more than a half-time job for anyone willing to work at it. So Bill

left. This was in February, so I volunteered to Ray to take over, on a temporary basis at least, as registrar, and also to continue as administrative assistant. This would get me out of the top management suite. I'd have a place of my own on the first floor, and I'd have a separate realm in which I could operate with a minimum of outside interference. And in fact it worked that way. I was very comfortable as registrar. Ray was replaced as VP [for student affairs] by [professor of environmental health engineering] Jim [James J.] Morgan in 1980, and he and I worked out very much the same sort of a reasonably comfortable arrangement. I don't know whether you want to get to the matter of retirement ages, things of that sort, or—

ERWIN: Well, why don't we just go with that? It's an interesting question, I think.

BONNER: It's an interesting question. I, like many of my confreres, had believed during all my adult life that one retired at age sixty-five. It was on tablets handed down from on high. Everyone retired at age sixty-five. But when I came to Caltech, I found that things were a little different. Faculty retired at age sixty-eight but could continue for another two years to seventy, and most of them did. For staff, in 1965 they were in the midst of a transition from age sixty-eight staff retirement to age sixty-five. And so right where I fitted into that transition, my normal retirement age would be sixty-six. Then I got a faculty appointment that moved me up to sixty-eight. This gave me a little more leeway. In 1977, I was rapidly approaching age sixty-five. I'd agreed this could well be a temporary appointment, because I no doubt would be retiring in the relatively near future. But along about that time, the federal government passed a law saying that thou shalt not retire anyone on account of age, at any age under seventy, except tenured faculty, a specially chosen, lucky few, who would continue to be retired on whatever agreements they had with their universities. Shortly thereafter, the State of California passed a law saying that thou shalt not retire anyone on account of age at any age, except tenured faculty. So then, as time went on through 1977, '78, '79, it was left up in the air. It was up to you: What would you really like to do? My wife and I talked it over. She was working, happy in her job, recently promoted to editor of *Engineering & Science*, and wanted to continue with that for a while. We agreed that there was nothing either of us would rather do immediately, so we simply stayed on, although beginning about 1980 I began to view the possibility of cutting back on my commitments. I stopped my teaching. About the same time, I stopped the consulting work I'd

been doing for my former employer, Hercules, ever since I left them. We had parted in 1965 on good terms, with a consulting contract in my pocket, and I'd continued to talk with them, off and on, for the next umpteen years. I quit that by mutual agreement. A couple of years later, Jackie decided to step down from her position as editor. After five years, she said, she didn't really need the excitement anymore, and she was getting a little old for it. She stepped down, cut back to half-time and stayed on as senior editor in publications. A little later I started talking with Jim [Morgan] about my cutting back to half-time, by staying on primarily as registrar and letting him find someone else to do the administrative things. This he was finally able to do, and I cut back to half-time in about 1984. It took a little longer to get rid of the Health Center, though, because it was hard to find anybody else to do it.

ERWIN: So you really had a long association with the Health Center.

BONNER: Too long, maybe. I've been comfortable at half-time, and I've found it pleasant. Not overly demanding, but with a level of student involvement pleasant to me. I see students in trouble with the Undergraduate Academic Standards and Honors committee and also students coming up as seniors and coming to talk about graduation requirements. They're the ones who can see the light at the end of the tunnel.

ERWIN: Yes. So does the registrar have a greater degree of importance to them? Because you're the keeper of all the facts and figures regarding conferring of that piece of paper.

BONNER: That piece of paper that says BS. A year ago, Jackie retired finally, decided she'd had all she needed. And it seemed to me, then, maybe it was about time for me to do the same. So a year ago last June, I turned in a notice of intention to retire, without saying absolutely when. Of course, they could stall indefinitely on that, but they advertised and looked around. It was rather difficult to recruit someone at that level for a half-time job, unless you can find the other half to go with it. Finally Judy [Caltech archivist Judith R. Goodstein] expressed a willingness to spend half of her time over here, and the appointment was made. Now we're sort of working out the final detachment details, and on June 30th I'm officially free.

ERWIN: Well, in conclusion then, maybe I could ask you what's probably an impossible question, and that is, if we compared Caltech today to the Caltech of the 1930s, what's better and what do you think is worse?

BONNER: Just off the top of my head, I can't think of anything that I can really say is worse. One thing that has suffered with the growth—although the growth really hasn't been as much as one could imagine—is the closeness of faculty members to one another, the acquaintanceship across option and division lines. It surprises me every now and then to find, for instance, at the Athenaeum two faculty members sitting next to one another and introducing themselves and discovering both of them have been here for fifteen years. You would have trouble believing it, but it happens. That's one thing that has suffered somewhat, but still, compared with many schools, I think we have a very high degree of interdisciplinary communication. We have more students, more space, a lot more money, which, of course, brings a lot more problems. And there again, another thing that has suffered is the willingness and ability of faculty to take on more responsibility—for instance, for faculty committee activities, for the occasional part-time student administration appointment. Dean of students, for instance—this is typically a full-time faculty member, taking a five-year half-time leave.

ERWIN: Do you think that will continue to work—this recruitment of the administrators out of the faculty?

BONNER: This is something that has suffered in today's climate, this highly competitive and highly money-oriented climate, in which the price of science has gotten so high. The only source of money out there, for all intents and purposes, is the government. You have to spend an inordinate amount of your time writing proposals, going to Washington to defend them, serving on government committees, evaluating other people's proposals. To decide you're going to take five years off, give up your career—for the active sort of younger person you really would like to have in these jobs, it's a price they can't afford to pay.

ERWIN: Yes.

BONNER: They lose. Once you get out of the mainstream, you're likely to go off in the shoals and die.

ERWIN: So what do you predict as a possible outcome or solution there? Are we in academia going to have professional administrators?

BONNER: Increasingly, we're going to have professional administrators. And this is not necessarily all bad. Certainly changes the flavor, though. The provost, so far, has continued to be not necessarily an active faculty member who expects to go back [to research] but at least a senior faculty member with a great deal of experience—one who's maybe come to a good point to switch, who's willing to shelve his research for a while. I don't know of any provost who's gone back into active research life—Christy, Bacher before him, Jack Roberts. [Professor of physics] Robbie [Rochus E.] Vogt, I guess, is doing something, however—not as a researcher but as the manager of a project.

ERWIN: He's a touch younger than the others. Is that not correct?

BONNER: Yes. I guess [the current provost,] Barclay Kamb [professor of geology and geophysics], is also somewhat younger. Whether he'll have anything left when he gets out of that office, I don't know. Takes a lot out of you.

ERWIN: Caltech probably uses up a lot of people.

BONNER: Yes. Anyway, I feel very comfortable with my own life here, very grateful to Caltech for taking me on. And putting up with me all these years.

ERWIN: Good. Well, that's probably a very good note to end on.

BONNER: Yes.