ROBERT P. SHARP (I)
(1911-2004)

INTERVIEWED BY
GRAHAM BERRY

December 18, 1979–January 9, 1980

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Subject area
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Abstract
Interview in three sessions in late 1979 and early 1980 with Robert P. Sharp, Sharp Professor of Geology emeritus, who chaired the Division of Geology (later the Division of Geological and Planetary Sciences) at Caltech from 1952 to 1968. Begins with his recollections of growing up in Oxnard and of life during his undergraduate years [1930-1934] at Caltech, including his career as quarterback on Caltech's football team, and his one graduate year there. In 1936 he moved to Harvard for further graduate study, doing his thesis work on the Ruby/East Humboldt Range in Nevada. From 1938 to 1943 he taught at the University of Illinois; he discusses expeditions in the Grand Canyon (1937) and the Yukon (1941). After three years with the Army Air Force in Alaska, he joined the faculty of the University of Minnesota, then returned to Caltech as a professor in 1947. He discusses the early history of Caltech's geology division under J. P. Buwalda, the importance of the Seismological Laboratory, and the demise of vertebrate paleontology at Caltech after the death of Chester Stock. Discusses the expansion of the division under his chairmanship into geochemistry and planetary science and other events of his chairmanship; chairing the search committee for a new president upon the retirement of Lee DuBridge; and the advent of Harold

Brown. Recalls his participation in the efforts of Eugene Shoemaker and Leon Silver to raise money for a named chair by guiding trips in the Grand Canyon, and his establishment of field trips for the division non-academic staff. The interview concludes with a discussion of his interest in current geological phenomena, such as glaciers and wind effects, and his authorship of guidebooks on Southern California geology for laypeople.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH ROBERT P. SHARP

BY GRAHAM BERRY

PASADENA, CALIFORNIA

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INTERVIEW WITH ROBERT P. SHARP

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Robert P. Sharp **by Graham Berry**

Pasadena, California

Session 1	December 18, 1979
Session 2	December 19, 1979
Session 3	January 9, 1980

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SHARP: I'm a second-generation native Californian. My father, Julian H. Sharp, was born in Santa Rosa. My mother was born in Saticoy. My paternal grandfather, James M. Sharp, migrated across the plains to Oregon in 1850, when he was four or five years old. Later he moved down the coast into California. Obviously he was in Santa Rosa when my father was born, and then he moved on south into Ventura County to become one of the pioneer citrus growers there. My father worked mostly for my grandfather. He ran a lumber company in Oxnard when I was a little boy, which was a branch of the People's Lumber Company, which my grandfather Sharp had helped to found. Later my dad ran one of the citrus ranches in Ventura County that belonged to my grandfather's holdings, later organized into the J. M. Sharp Farming Company.

On the maternal side, my grandparents migrated directly from England to California, and they were another one of the pioneer families in the Ventura County area. Not in citrus so much as lima bean farming. So my maternal and paternal grandparents were pioneer Ventura County settlers.

My maternal grandfather's name was John Darling. There is a Darling Road near Saticoy which is related to the old family homestead there, and a Darling plot in the Santa Paula cemetery, which also harbors a Sharp plot.

BERRY: And your mother's first name?

SHARP: Alice. My paternal grandfather had a large family of eight kids—six boys and two girls. One of those girls just died recently at the age of almost a hundred and four, Aunt Grace. She was the last survivor of grandfather's children.

Before my dad settled down to work for my grandfather, he spent about five years in Alaska and the Yukon. He was an 1898 Klondike gold rusher. He went off to Alaska as a young man. Neither of my parents got beyond freshman year of high school, so I didn't come from a family that was intellectually oriented.

I notice you have a question here about outdoor interests. To begin with, I was born in the small agricultural community of Oxnard, then a town of about 5,000 people. My whole family was agriculturally oriented. My grandfather Sharp and practically all of his boys were also enthusiastic hunters and fishermen. My grandfather was fond of children and proud of his many grandchildren. He loved to gather them all together at the old home ranch. I was at the home ranch just last Sunday at a meeting of the J. M. Sharp Farming Company. I'm a member of the board of directors of this family-held corporation. We still meet at the old homestead, which was built probably about 1880. It's a grand old house. A beautiful place; it's fun to visit. It has been a focal point of family gatherings for a long time. The ranch is almost wholly in citrus now. My grandfather used to raise walnuts and had a beautiful big walnut orchard. But walnuts became less profitable, so he ripped them all out and put in citrus—lemons and Valencia oranges.

My grandfather, as I said, loved kids, and he and my father and uncle were always going off somewhere on fishing and hunting trips. I don't know this for sure, but I think my grandfather always said, "If Bob wants to go, he gets to go." So I think largely through my grandfather's influence I got to go on a lot of these fishing trips, particularly those to Owens Valley, east of the Sierra Nevada, a great place in those early days. At the age of two, I was hauled off on my first camping trip into the mountains. I don't remember the event, but I had an accident. I cut my chin, and they had to bring me out on a horse. But I can remember that when I was about four, our family was always going camping somewhere so my dad could go fishing or hunting. It was the influence largely of my dad, my grandfather, and my uncles that got me into this outdoor business.

How the geology got into it is a little uncertain. I can remember as a small kid asking my mother questions, as we traveled on these trips, which I now realize were geologically oriented.

I'd see some big rocks or something and I'd say, "How did those get there?" Then she'd give me an answer. I didn't think much of the answer, even at that time, but she was doing the best she could. So I think I had a little interest in geology, just intuitively, from having traveled around the country a lot.

I went to the primary schools in Oxnard and to high school in the same town. None of the schools, I would say, were outstanding. They were average schools for California at that time, which would have been the teens and the twenties. So my education up to the time I graduated from high school was just average California primary and high school education. I was fortunate in high school in having a good general science teacher, who did give us a little geology, not much but a little bit. I liked it. But I had no concept of geology until my sophomore year at Caltech.

BERRY: How about scouting?

SHARP: I got into a Boy Scout troop in Oxnard early on and stayed in it until I left the area. There is no question that scouting had a considerable influence on me, a very useful and valuable influence.

BERRY: You were an Eagle Scout, weren't you?

SHARP: Yes, I was an Eagle Scout, but all you had to do was earn a certain number of merit badges.

Let me go back to the early childhood business. When I was very small, I was a sickly child. I had malnutrition and swollen glands. I was the next-to-smallest boy in my grammar school graduating class. When I was a freshman in high school, I was still the next-to-smallest guy in our class. The chap smaller than I was a Japanese, my closest friend. We happened to have—fortunately, I think—a very interesting racial mixture in Oxnard; we had a lot of Japanese, quite a few Chinese, and a lot of Mexicans in our community. I grew up in this kind of mixed group and have no racial prejudice, I think partly for that reason. I am very fond of Orientals, as a matter of fact. Many of my early closest friends in the primary and high schools were Oriental.

As I said, I was rather sickly. I managed to contract more than a normal number of

diseases. I had malaria, I had smallpox, mumps—you name it, I had it. Partly for that reason, I think, I had a yen to be physically active. I felt somewhat deprived and wanted to do something about it. I was an ambitious, aggressive little guy in a physical, competitive sense. I don't think socially I was aggressive, because I tended to be rather shy and withdrawn. So I tried all sorts of athletics, but I wasn't very good at them. In high school, I played basketball and football and everything else I could, but I was either on the small team—the B or C teams, the little bitty guys—or when I got too big for that I ended up sitting on the bench of the larger teams. I was no star high school athlete, I'll tell you that, but I wanted to be one. I was interested in track among other sports. I used to pole vault. As a matter of fact, one of the big disappointments of my high school career came in my senior year. I won the pole vault in the Ventura County meet that year, but after I'd gotten out of the pit the wind blew the bar off the standards, and the judge, who really should have supported me, yielded to the guy who protested and gave me another turn. I didn't make it on the second try. But I cleared it the first time, by God; I cleared it by six inches and never touched it. That loss was a bitter disappointment to me.

BERRY: You remember the height?

SHARP: About eleven feet, six inches. It was as high as I ever vaulted, as a matter of fact. I don't know why, I just hit everything right that time and I was airborne.

As to the rest of the family, I had a brother and sister, both of whom are considerably younger. My brother was about seven years younger and my sister nine or ten years younger than I. My brother's name was Merrill, and he had to assume my mother's maiden name for his middle name, so he was Merrill Darling Sharp; he didn't like that very much. My sister is Winifred Ruth Sharp. She is the only other surviving member of the family and lives in La Jolla.

My family was broken when I was about age ten. My dad moved out, and my mother subsequently divorced him. She then raised the family. So at about age ten I no longer had a father. This experience seemingly had an impact on me in terms of responsibility. I found that very shortly my mother and I, even though I was just a little kid, were handling the family affairs. She talked to me about the family problems. We'd consult and decide what we were going to do. This procedure perhaps had an important impact on my outlook.

I went to work at age ten. I had nothing but a paper route to begin with. Later I worked

in a jewelry store, and then in a service station, which was a very good thing for me. I was a shy, withdrawn kid, and while working in the service station you had to deal with people from all walks of life, from the town prostitute to the town drunk or to Buron Fitts, who was running for lieutenant governor of California, and to Gary Cooper, who used to come through on Sunday with Lupe Velez in his big yellow Dusenberg. This was a great thing for me. Working in the service station was definitely better than the jewelry store in that respect, and the rougher, outdoor aspect of the job suited me better. Between the time I graduated from high school in 1929 and the time I came to Caltech as a freshman in the fall of 1930, I worked in the service station sixty hours per week and went to school part-time.

Our high school didn't offer all the Caltech prerequisite courses. We didn't have solid geometry, so the mathematics teacher, Mr. Berlin, organized a special course for three or four of us who wanted solid geometry. We also took mechanical drawing as a requisite to apply to Caltech. I'd hardly ever heard of Caltech when I was a youngster, but in my high school class was one of these boy-wonder scientists. He had a chemistry laboratory set up in his garage at home, and he did all sorts of impressive things therein. He was gung-ho about Caltech, which dominated his thoughts and talk. He would go down to Pasadena, visit the campus, and go to Throop Hall. There weren't many buildings on the campus back in the late l920s. He'd take a picture of Throop Hall, and then he'd come back and he'd say, "You see that window? That's the window of R. A. Millikan's office." He knew all about the place.

BERRY: Who was he?

SHARP: A fellow by the name of Allan Mitchell. He was bound to get into Caltech if he could. He was a close friend of mine, and lived only a couple of blocks away. I said, "If Mitchell can get into Caltech so can I, because I'm just as good a student as Mitchell." So it was partly through stimulation by Mitchell and a competitive response on my part—if Mitchell can do it, I can do it. In those days you had to take special exams to get into Caltech. They didn't use the college boards. You had to come to the campus for two days to take the special Caltech exams, after you had applied for admission. We took them in late spring or early summer, as I recall.

BERRY: Do you remember the subjects the exams were in?

SHARP: They were pretty standard—math, chemistry, and physics, and you had to write an essay to test your English. Mitchell and I both must have passed the examinations or else they didn't have enough applicants that year. We were admitted. It was a lot easier to get into Caltech then than it is now, I'll tell you that.

Caltech had a second period of exams very late in the summer, apparently to fill out the quota in case they hadn't gotten enough students the first time through. So we persuaded one of our high school class chums, Henry Sieland, who was also at Oxnard and who had done exactly as we had done—stayed out a year and taken some of those special courses—to take the fall exams. He took it, passed, and got admitted. So three of us came from Oxnard to Caltech as freshmen in the fall of 1930. To the best of my knowledge, the only person from our high school to come to Caltech prior to that time was none other than Vito Vanoni. When I was in high school, our general science teacher, who came from Somis, the little town that Vanoni lived in, always held Vito up as the epitome of accomplishment, ability, and everything else. I'd never met Vanoni—I'd never seen him, although I knew his sisters—but ever since high school days I knew who Vito Vanoni was.

There were two local heroes from our high school. One was Vito Vanoni, who was the intellectual hero. He made it to Caltech and was a successful Techer. He had graduated by the time we got here but may have been in graduate school. The other hometown hero was not an intellectual figure but an athlete, named Bud Houser. Bud Houser was a shotput and discus man. He eventually went to the University of Southern California. I think while still a high school student he had gone to the Olympic Games and won gold medals—maybe not in both events but in one or the other. He did this twice. He became a dentist. So Bud Houser and Vito Vanoni were two heroes of the local community. I never really got to know Vito until I became a member of the faculty.

Caltech was, I think, an infinitely more regimented operation in 1930 than it is now. You didn't have any problem making out your academic schedule. You just opened the catalog to a certain page and copied down what was there. In four years as an undergraduate student at Caltech, I think I only had one optional course. That option involved a humanities course. I had to take a humanities course, but I had a choice of three or four different ones. That was all; there were no other options. All was cut and dried. The first year was the same for everybody. The

second year, you were required to decide whether you wanted to be an engineer or a scientist. If you wanted to be an engineer, you took an extra drawing course and a surveying course. If you wanted to be a scientist, you took a chemistry course rather than the drawing course. Up to the end of the sophomore year those were the only differences. It was only in the junior year that course schedules started to branch out a little bit.

I guess the roughest three months I can remember as a young person was the first quarter at Caltech that fall. It was rough for a lot of reasons. First, I didn't come from a really good high school. My educational background was not as comprehensive as, say, the students from Hollywood High School of whom there were a lot. The program was regimented, and it was hard going. I went out for freshman football, and I was so sore and stiff for the first month that I could hardly walk. Caltech had closed down all its fraternities. The student houses were not yet opened, so we had to live in the town. My high school pal, Henry Sieland, and I lived in the YMCA for about the first month we were here; we ate all our meals in various restaurants and hash houses around town. We began to do a little better when we moved into the little rooming house on Steuben Street, just south of Colorado. It's just a little block-long street you may never have noticed, between Wilson and Michigan. We moved into the rooming house, which was better than the YMCA. Right behind our house was a boarding house where we could take morning and evening meals. There were four or five Caltech upperclassmen, juniors and seniors, at that boarding house and this, I think, was what made our freshman year tolerable.

BERRY: Did you have a car?

SHARP: Yes, I had a car—I guess I had a Model A at that time—and we would go home on weekends. It was a two-hour drive. We'd take a bunch of books, go home, get a little something good to eat, get our clothes washed, and come back refreshed. Later on, I didn't do that so much.

BERRY: You did a lot of studying, I guess.

SHARP: Oh, we worked like dogs. We really did. The upperclassmen who ate in this boarding house were a godsend to us, because they could put things in perspective. The thing that really got to you was that so much was unknown, uncertain. I had a mathematics professor—I thought

I was pretty good in mathematics, but I flunked the first math exam I ever took at Caltech. This man called me into his office and said, "Get out of here, you don't belong here. You ought to leave Caltech."

BERRY: Who was that, do you remember?

SHARP: William Birchby. But these upperclassmen would say, "Oh, don't be upset about that. It happens to everybody and Birchby's that way. Don't take it so seriously." That's what we needed, these guys to put things into perspective for us. I think the younger kids in the student houses don't quite realize what they are getting out of the student house in terms of guidance from upperclassmen. People put things in perspective for them. We had a certain number of snow merchants around in our freshman class who were telling us how good they were and how bad we were. The first term of the freshman year, for me at least, was a traumatic experience. But I survived with a reasonably good grade-point average. Not as good as I got later, but nonetheless I survived, and I began to gain confidence.

BERRY: Had you decided yet what you wanted to major in?

SHARP: No, I came down here thinking I was going to be an engineer, a civil engineer. The first year you didn't have a chance to think much about such things, and truthfully you were too busy to give a damn. You were so busy going through the lockstep of performing up to par that you didn't worry much about the future. I survived the freshman year; as a matter of fact, all three of us survived the freshman year all right. In the sophomore year we lost both Mitchell and Sieland—they were washed out. Let's say they left school. So I was the only survivor.

BERRY: Do you remember any of the profs you had?

SHARP: I remember Winch Jones very well. I had him in freshman English. John MacArthur lectured in freshman history. Most of the other instructors I had, unfortunately, were graduate teaching fellows, and most of them I don't remember. Linus Pauling occasionally lectured to us in freshman chemistry. Robert Millikan would lecture to us in freshman physics. But they

didn't teach the courses. We saw a lot of Earnest Watson; he gave a number of the freshman physics lectures.

BERRY: Any one of those that you particularly liked?

SHARP: Well, Watson was pretty good in that he was well organized and dramatic. He used to give his liquid air lecture in freshman physics. He also had a Friday evening demonstration lecture on liquid air. He'd give us the same thing—well, not quite the same but a more serious lecture. My impression of freshman year instruction was not very good. Although we saw some of the big names of the institute, we saw them only casually. They weren't really teaching us. We were taught largely by graduate teaching assistants, some of whom were lousy, just lousy—although some of them were quite good. None do I remember particularly now; fortunately this is all blanked out for me.

That wasn't true in humanities. There you did get the first-line staff, and I had Winch Jones. I can't remember who else I had as a freshman. I had Ernest Swift as a sophomore, and Ernest Swift was a soft-spoken tyrant. He taught you those things you had to know, with an unmerciful discipline. He was a perfectionist—you had to measure up. Bill Michael in surveying was a delightful guy, as was George McMinn in literature. Finally, I got a course from [William Bennett] Munro, which was just delightful. William B. Munro was a great man and a delight to have a course with.

One of the nicest memories I have about faculty relationships at Caltech was my sophomore year, when I moved into a house. They were first opened then. The last three years I lived in Dabney House, and it was infinitely better than the off-campus living we had had before that time. William B. Munro adopted Dabney House. Don't ask me why, but he did. He'd come and have lunch with us every now and then, and so we got to know him well. He usually threw a party for Dabney House each year, at the Valley Hunt Club over on Orange Grove Avenue. Through my later years at the institute, I got to know Munro well. I will note later, I spent one year here as a graduate student, which was a mistake. But during that one year I had to take a humanities course—it was required in the graduate study program—and I had one of Munro's courses, municipal administration or something like that, as I recall. It was very good.

I got to know some of the other faculty members, too—saw more of Pauling and Bill

Michael in surveying. I had Howard Smits as an instructor in drawing; he's been president of the Associates recently. By my sophomore year, I had developed confidence that I would survive at Caltech, and from there on things got better and better all the way through for me. Each year was better. My senior year at Caltech was a very satisfying year, in all things. In those days, by the end of the sophomore year—I think the third term of our sophomore year—we had to declare a major. We had had to make a decision at the end of the freshman year as to whether we wanted to go into engineering or science. At that time I shifted from engineering to science, focusing on chemistry. I think Ernest Swift convinced me I wasn't going to be much of a chemist.

BERRY: What shifted you toward chemistry? Was it the teacher?

SHARP: I guess you like those things for which you seem to have a reasonable knack, and I did better in freshman chemistry than I did in physics. Biology was just getting started, so when you thought of science in your freshman year, you thought of physics, mathematics, and chemistry, and of those three, chemistry appealed to me most. I thought on the basis of my freshman experience that I might be able to be a chemist.

I probably would have continued in that direction, except in the first term of my sophomore year I had to take a course in geology. It was required, as was biology. I had hardly ever heard the word "geology" before that time. There was one fellow in our freshman class who said he was going to be a geologist. We all laughed at him and said, "What's that?" His father had been in the land office of Union Oil Company and had seen something of oil company operations. Apparently this boy's dad had said that of all the guys in the company, the geologists seemed to have the most fun and that he ought to be a geologist. So this kid was oriented toward geology. But he did not become a geologist. He later became a petroleum engineer.

Anyway, we had to take this geology course, which just hit me right. Bingo! John Buwalda gave the lecture—and he was very good. It was a revelation to me. All of the things I had seen as a little kid, had made note of and didn't understand, suddenly began to fall into place. I had done a lot of trout fishing up in the Mono Basin; I can remember walking over ridge after ridge after ridge to get to Walker Creek to go trout fishing. Buwalda was lecturing about mountain glaciation one day, and it suddenly hit me, "My gosh, those ridges are lateral

moraines—that's what those things are!"

As I finished that course I thought, "My gosh, wouldn't it be marvelous if you could earn a living working in the field of geology?" Remember, those were the Depression years. Jobs of any kind were hard to get. As students, we were, I think, on the whole serious, older, and more mature than our present students. We were not as smart, not as well educated, nor as well trained.

In those days there was a much heavier emphasis on engineering. There were more students in engineering than in science then. The balance has shifted, of course, with time. In my freshman class, my guess would be that half the people had stayed out of school a year or had gone to some other college for a year before coming here. That's just a guess. But they were older, more mature, and they were deadly serious—because those were tough times. You came to Caltech to get an education that was going to get you a job, maybe, when you graduated. So I thought, "Gee, wouldn't it be marvelous if you could work in geology, have a job and earn a living doing that kind of thing?" I didn't have to make up my mind until the third quarter of the sophomore year. The course had been in the first quarter, and in between we had to take the biology course. I'm sorry to say that at that time the elementary biology course, which was required, was awful, just awful. The biologists had not yet organized it in a way that made any sense. A lot of different people lectured. We got to listen to Thomas Hunt Morgan and he was great. Henry Borsook took quite a bit of the course, and did a good job. Albert Tyler taught a good deal, but we couldn't understand Tyler. The course was oriented toward the research interests of the people in biology. It was not an attempt to tell you much about biology; it was an attempt to tell you about genetics and fruit flies and stuff mostly way over our heads. It was a bad course—I don't mind saying that. Anyhow, there were very few biology majors, but of course this was just a year or two after Morgan had arrived and things weren't yet very well organized.

So came the third quarter of my sophomore year and I had to make a choice. I elected to give geology a try. I liked it so much that I decided just to let the chips fall where they may. It was fortunate that I could find a subject that I had such an intuitive liking for. In hindsight, I can honestly say that geology has been a ball for me, all the way. I'm going to say this again later, I think, but let me say it now. A lot of things happen during the progression of your life, and you look back and you say, "That was important in terms of what I ultimately did." Discovery of the

subject of geology was one of the great good fortunes of my life. If I hadn't come to an educational institution that forced me to take a course in geology, I might never have discovered it. There are a lot of educational institutions that don't require you to take any science. But at Caltech—small, regimented, limited in its offerings—you had to take a course in geology.

The other thing that in hindsight has been very important to me is the educational experience and discipline I got at Caltech, followed by graduate study at Harvard University. That combination gave me what I regard as a superb education. It really did. Without that education, I could have been just another guy working in a service station. Education, for me, was one of the fortunate aspects of my life. During that education, I discovered geology, which then became my prized profession. I couldn't have done better. Those are things I'd like to highlight in terms of educational experience.

Now let's discuss the matter of athletics a little bit. I was not an outstanding high school athlete. In track I held my own, but in football and basketball I was always on the second team. I was a little guy. One of the great opportunities offered by Caltech, if you had the will, was participation in varsity sports. A curious psychological thing happens because of such participation. Let me illustrate by drawing a parallel with kids who have been undergraduates at Caltech and then have gone on to graduate school somewhere. When I write a letter for them I try to transmit the message: "Don't be too hung up on this student's undergraduate record at Caltech. It may look average to you, and indeed he may have performed as an average student in this environment. I'll make a prediction that although he may have been a C student here, he'll perform as an A or a B student in a different environment." The new environment may be no less difficult, perhaps, than Caltech, but the student stops thinking of himself as average and he begins to behave differently. The intellectual excellence of Caltech has intimidated him. The same thing happens in athletics. If you sit on the bench, you never get the opportunity to learn to be good or to show that you can be as good as others. I think that happens here at Caltech intellectually, to some degree, but not athletically.

Anyhow, I went out for freshman football, and as an upperclassman did both football and track. I didn't try basketball or baseball. The first two sports occupied the whole year, because we always had spring practice in football in those days.

BERRY: Were you a quarterback?

SHARP: I came to Caltech having played a little quarterback in high school—

Begin Tape 1, Side 2

but I told the freshman football coach—who was Layton Stanton, the son of Fox Stanton—that I was a halfback. He took me at my word and I worked out at halfback, ending up on the second team of the freshman squad. We had enough freshmen out for football to have a couple of teams in those days. The quarterback was a fellow by the name of Gertmenian, who came from Pasadena High School. He'd been a guard in high school. He'd played a lot of high school ball at that position but decided he wanted to be a quarterback, so he was a quarterback to begin with. But Layton Stanton knew his history, and after a couple of weeks he said, "Gertmenian, you're no quarterback. Back into running guard you go, boy." The coach then had to look around for another quarterback, and one of the boys who had come from Oxnard with me—Henry Sieland, my roommate, who was also out for football—said to Layton, "That guy's a quarterback." So reluctantly, within the first two or three weeks of the season, I was back as a quarterback. This put me into the first team of the freshman squad. We didn't have a very good freshman team, but we played Occidental, Whittier, Pomona—the other Southern California Conference schools—on a reasonably even basis. We probably had about thirty freshmen out for football.

BERRY: They played both defense and offense?

SHARP: Yes, you went both ways. Fox Stanton had a very, very complicated and marvelous offense for the varsity, but he wouldn't let the freshmen use it. We had the old simple Pop Warner, wingback stuff. We had to learn the fundamentals. Although we didn't have an outstanding team, we had fun. I got badly hurt in the next-to-last game, the Occidental game. I twisted a knee badly. Nowadays they would operate on it, but in those days they didn't do anything. However, that knee has held up for fifty years, although it's now beginning to get to me. But fifty years is not bad.

Anyhow, in the spring practice of 1931, the varsity for that year was shaped up. We had a very elaborate spring practice, and prospects were for a good football team in what was to be my sophomore year. Red Watson was the captain, a big fullback, and the best football player I

have ever played with. We also had a guy named Bill Schuler, who played tackle, who later—you could do that in those days—went to West Point and played four more years.

BERRY: Became All-American?

SHARP: Became All-American and also captained the West Point team in his senior year. Watson was better even than Schuler. We had a good football team. We won the conference championship when I was a sophomore. That's the last, I think, bona-fide football championship we ever won. Anyway, in spring football I ended up as first-string quarterback for the varsity squad, which really boosted my feeling of confidence. We had three quarterbacks. Of the others, one was a junior and one was a senior. In the real season, I got to play probably half the season as the starting quarterback. I finally fumbled a couple of punts somewhere along the way, which reduced the Fox's confidence in me.

I can remember that when we played Occidental as freshmen, the Occidental freshman team included people who had been first-string varsity players from my high school squad. I had been sitting on the bench while they had been playing for our high school. Yet here at Caltech I was able to be on the same field playing against them. We lost that Occidental game by a single point; 12 to 13 or something like that. So we were essentially as good as they were. That's a terrific boost to your confidence. I think it demonstrates the principle I was talking about earlier—you need the opportunity to show what you can do. I was really not a good football player, but the opportunity to play football at Caltech was to me a great experience, a really great experience. Probably if I had been at Whittier, Pomona, or Occidental I would not have been a varsity football player. I honest to goodness believe that.

BERRY: Were you a quarterback on that championship team?

SHARP: Yep! I have a little gold football for the conference championship; 1931, that would have been. As long as we're talking about football, let me talk about Fox Stanton, who was the ideal coach for Caltech. He was a fiery little guy. He had been trained originally as a clergy-man, a preacher. Some of the finest sermons I ever heard in my life were in the Rose Bowl locker room. We used to play in the Rose Bowl frequently—that was our home field. When I

first went to the East Coast after graduating from Caltech, people would find out that I had come from Pasadena. They'd say, "Oh did you ever see a Rose Bowl game?" I'd say, "Yes, we saw two or three Rose Bowl games, but I played there a half-dozen times." They'd say, "Really!" Then I'd explain why. We played in the Rose Bowl with a hundred people in the seats on either side. We played there frequently and I always got a big bang out of it.

Fox Stanton was great as a football coach; he was inspirational. He was good in that he recognized that he didn't have a lot of talent in terms of physical ability. So he had to rely on something else. He had a bunch of kids who were relatively intelligent, as a matter of fact. I think that when I was in school the football squad had as big a representation in Tau Beta Pi, the honorary scholastic fraternity, as any unit you could name on campus except Tau Beta Pi itself. Our kids were smart. Stanton knew this, and he was way ahead of his time. In those days, the rules for playing football penalized the offense. Two incomplete passes in a series of downs, and you got penalized five yards. If you passed the ball across the goal line, you lost it; it went to the other team and they got it on the twenty-yard line. So when you got down close to the goal line, you didn't dare pass. Also, you had to be five yards behind the line when you passed. It was a different ball game in those days. There was no T-formation; all the backs had to be at least three yards behind the center. That was the rule.

However, Stanton had made, in those early days, what was as close to a T-type offense as possible. The quarterback got the ball on almost every play, except when we would go into a single wingback, or to punt. We had seven or eight different formations, but the basis of Stanton's system was that the quarterback handled the ball on every play. Although the hand-back from center was truly a pass, it couldn't be a hand-off as it is now. I had to be three yards behind the center. He'd pass it to me, and I'd turn my back to the line and then proceed to administer the football in various different ways to the other backs. Stanton had worked up a beautiful offense. It was very complex. People who were scouting would come down to Stanton during a game and say, "My gosh, Fox, I can't tell what is going on out there." And Fox would reply, "Well, I can't either but sit down in this chair alongside of me, and if I see something I recognize, I'll tell you what it is." Besides being very complex, the offense changed every week—we'd vary it in some way. We also had defensive signals. Nobody else had defensive signals. Ours were audible defensive signals. One of the guys on the line would look around at the other team and he'd hold up his right hand and say, "Aye, Aye." The other team wondered,

What's that? Well, it was a particular defense lineup. So you see we had a lot of fun. We didn't play well, but we had fun.

When I was a junior, we played UCLA once in the Coliseum. They just licked the tar out of us. Stanton came to me before the game and said, "If you win the toss, elect to receive the ball." Then we were to execute a sequence of plays that he had designed specifically for the situation. He was daring. Well, we did win the toss and elected to receive the kickoff, and we ended up with the ball on our own twenty-yard line. We ran three plays and just bounced off of that massive UCLA line. It was fourth and ten on our own twenty-yard line. The player who punted for us was a guard, Phil Craig, who came out of the line to punt, so everybody knew when we were going to punt. Instead we ran a screen pass by letting the UCLA line charge through. I'll never forget the expression on some of those guys' faces as they charged through there. They were just going to kill the punter. Instead, he faked the punt and tossed the ball over their heads to a little guy named Demolita. We got down to about the UCLA twenty-yard line before they caught us. We didn't score, unfortunately. I threw a pass on the next play and got down to about ten. But we cou1dn't get it over the goal line. Because we couldn't pass across the goal line without losing the ball, we were handicapped. We had fun, and that was Stanton's way. He knew damn well that UCLA was going to murder us, but along the way we had bedeviling fun. When the UCLA guys got the ball, they nearly always scored. But in the interim we did all sorts of screwy things. So it was great.

I think Fox was the kind of football coach Caltech needed—very imaginative. Other teams were scared of us, not because we could overpower them but because we'd do something unexpected. We did, and we won some games, but we didn't win a lot. When I was a sophomore, we had a great team. After that it was mostly downhill. We'd win two or three games in the Southern California Conference, which Caltech can't even play in any more, but in those days, we'd beat Pomona, we'd beat Occidental, we'd beat Santa Barbara—that was Santa Barbara State in those days.

I regret the fact that my left knee is shot, and I also broke a leg once in the Rose Bowl. But I think that the psychological toughness acquired from rough physical contact is beneficial. You learn that you can keep on doing things even though you hurt, and you learn to ride over pain and keep going. I think the football experience was a significant contribution to my total makeup. I treasure those memories of old Fox Stanton, and of course the guys I played with.

BERRY: He coached track too, didn't he?

SHARP: Yes, he coached track.

BERRY: He had to be awfully versatile

SHARP: Well, you had to be in those days. He was good. I broke my leg when I was a junior, so Stanton wouldn't let me pole-vault that year in track. He came to me and said, "I think maybe we'll make a javelin thrower out of you for this year." He had been over watching the 1932 Olympics and had seen the Finnish Olympic javelin throwers. Fox had looked very closely at their technique. He tried to teach me this Finnish javelin thrower's technique. I never was very good at it, but I won a few points in the javelin in track meets.

BERRY: Stanton acted like a preacher?

SHARP: Oh yes, he was terrific.

BERRY: He acted more like one than a coach?

SHARP: He could be a very caustic old guy, a very stern disciplinarian, given to those little phrases that just cut you right to the quick. He'd give us a new play, and then he'd turn around to me and say, "Sharp, don't you ever run this play, you're too slow." We had a little shovel pass which we practiced in Tournament Park under very poor lights, semidarkness. We had black practice jerseys, as I remember. I had to shovel a pass to a guy in the midst of a whole bunch of other guys all in black jerseys. In the poor light I couldn't see who was who. One time I got ready to unload this little shovel pass when I suddenly realized I had the wrong player in my sight. The ball just rolled out from my hand. Stanton came storming out on the field and said, "What are you doing, feeding the chickens?" So ever after I was "Feed the Chickens" Sharp. He was terrific that way. No question, he built character.

Now, I did spend one year at Caltech as a graduate student. That was a mistake, in the

sense that it would have been better for me if I'd gone away. I stayed for a couple of reasons. Our undergraduate curriculum was highly regimented and there were some courses taught here at the graduate level that I wanted badly. They were given by famous men—William Morris Davis and F. L. Ransome, for example. I couldn't take them as an undergraduate, but as a graduate I could. Davis died the summer before I became a graduate student, so I lost out on that one. Although I had applied for graduate study to a number of schools, I was offered very little help from any school. I had an offer of a tuition scholarship from Princeton, as I recall. So I stayed here for one year as a graduate student. I lived in the loggia in the Athenaeum. We got room and board in the Athenaeum. We didn't receive any money, but we were housed and fed in the Athenaeum. The year wasn't a complete loss, but I realized when I got away that it would have been better if I had left sooner.

BERRY: Why would you say that?

SHARP: The change of intellectual environment is stimulating. The change in your outlook, intellectually and socially, is good, and I was very provincial. I was born in Ventura County. I went to school in Pasadena. I had been taught by a certain circle of professors, given certain insights into geology. My world was Caltech—Southern California, West Coast centered. When I got away, I realized that I was a very provincial person.

I went about as far away as I could get, to Harvard. When I applied for graduate study elsewhere after one year of graduate study at Caltech, I got several nice offers, a particularly generous one from Northwestern. It was a prize fellowship. I had only a tuition scholarship offer from Harvard, but Ian Campbell, who was here at that time and had recently come from Harvard, leaned on me to go to Harvard rather than Northwestern. Fortunately, I took his advice. Harvard looked like an old, musty place to me, and initially I didn't think I wanted to go there. But when I got there I found it very stimulating intellectually.

BERRY: Good in geology also?

SHARP: Good in geology and a complete change. At that time, Harvard had a first-class geology department. Graduate study at Harvard for me was pure joy and pleasure. Caltech gave

me an educational discipline that made graduate study at Harvard relatively easy. I could do what they wanted me to do in about half of the time that was allotted, so I then had ample time to do other things on my own, intellectually. I had never had any free time at Caltech, but I had free time at Harvard. I'd sit in the Harvard library and take down a noted volume in geology and study something I'd always wanted to read. So I had a ball at Harvard, I really did. When I got there, I found that there were people from schools all over the country in the graduate body. Harvard was very cosmopolitan in terms of the graduate students. They were from all the good schools, and my educational background, for the most part, was the equal of or better than theirs. Most of them had never had to work hard; I'd had to work like a dog at Caltech. It's better to go from Hell to Paradise than from Paradise to Hell. I enjoyed Harvard tremendously, but I think it was better that I go to Caltech first and then go to Harvard than to reverse the sequence. With its undergraduates, Harvard was very permissive, and most of them weren't as good as they should have been. That was because they'd never had a true academic discipline forced on them. The intellectual discipline I got from Caltech was, I realize in hindsight, a very fortunate thing. Owing to it, I could capitalize on the rich intellectual menu available at Harvard. The students who came from other schools were busy developing the discipline that I got as an undergraduate. So I had a marvelous time at Harvard. Socially it was a blank. It was possible to feel that you were a non-person to the third power when I was at Harvard. Nobody cared about me. No social program, nothing.

It took me quite a while to get into an exercise routine. I needed physical exercise; I still need it. Once you get conditioned to exercise, you've got to continue it. I couldn't find any sport at Harvard. I floundered around until I finally discovered sculling on the Charles River, which proved to be marvelous. Sculling is a real workout and fun. I sculled when the river wasn't frozen in the fall and spring and played squash in the wintertime. It took me a year to work out that program, but when I got it worked out, it was just fine. I discovered that the shift from organized team sport to individual sport is an important one, and it took me a while to make that change.

BERRY: Did you specialize in any particular field of geology?

SHARP: I had not, before I got to Harvard, and for the first year or so at Harvard I repeated some

of the subjects I'd had before, intending to fill out generally in all aspects of geology—such as ore deposits, economic geology, structural geology, geomorphology, and so on. Within my first year at Harvard I had to decide on a Ph.D. thesis. I did that by consulting with certain staff members. I wanted to do a field geology problem, and I was inclined toward one in structural geology. R. A. Daly urged me to work along the 49th parallel, where he had worked one time as a young man, but I had gone to the map room and discovered a brand new topographic map of northwestern Nevada, a beautiful-looking thing. I could just look at it and tell that it was nice country. I went around to the structural geologist, Marland Billings, but he wanted me to work somewhere else. However, Kirk Bryan, God bless him, who was from New Mexico and was a geomorphologist, recognized the virtues of this Nevada map area and egged me on, so that's what I finally did. See that end volume on the shelf right behind you, on the Ruby/East Humboldt Range? That's my PhD thesis.

BERRY: That's an enormous piece of work.

SHARP: Not really. I only worked out there two summers by myself. The faculty wanted me to take an assistant, but I didn't want one. I did a lot of things and I had a great time. It was basically a structurally-oriented problem, but I also mapped the glaciation of the range and a lot of geomorphological features. I think it was largely as the result of my experience there—although the focus of this work was structural geology—that oriented me into geomorphology and glacial geology. Those were the things that caught my attention and became my major interests thereafter.

ROBERT P. SHARP

SESSION 2

December 19, 1979

Begin Tape 2, Side 1

SHARP: Going back to the undergraduate situation at Caltech, just briefly. I have already mentioned that Caltech was highly regimented in those early days. We didn't have much culture or social activity, though things got better after student houses came into existence in my sophomore year. Certainly the social program began to look up a little bit at that time. Before then, unless a person was socially inclined and knew how to go about interacting socially, Caltech was a social desert, it really was. However, we had one interesting thing in those days which I think was good and I want to comment on it. It was the so-called Monday Assembly. Every Monday at eleven o'clock the entire student body had to assemble in Culbertson Hall, which was where South Mudd is now. It was an hour-long affair, usually presided over by Clinton Judy [chairman of the Humanities Division]. The first part of the assembly was devoted to student communications. You could make announcements, you could urge guys to come out for the football squad, or this and that. Afterward there usually would be a talk, a presentation of some sort or other, done by somebody on the campus, or often from off campus. Millikan would frequently appear at those assemblies and talk to us for a few minutes. When Einstein was visiting on the campus, Millikan brought him over, and Einstein gave us about a fifteen-minute talk in German, which none of us understood, except about three guys in the audience. Einstein would tell a joke and those three guys would laugh, so all the rest of us would laugh, realizing that Einstein had said something funny. I remember that when [A. A.] Michelson died, Millikan came and gave us about a thirty-minute memorial lecture on Michelson.

Attendance was required [at the assemblies]; you had to go. I think we got one unit credit for being there. You signed a little slip—we had an honor system then, just as we have now—indicating how many assemblies you had missed during the term. Your grade depended upon how many you had missed, up to the point where you could flunk. Then you had to do something special to make it up. The point is that once a week the entire student body of Caltech got together and there was communication on student-body things. When elections were being

held, the candidates for the various political offices had a chance to stand up and make a statement. I think it was a unifying influence in the student body, which unfortunately is lacking now.

BERRY: Was that at the undergraduate level?

SHARP: That was at the undergraduate level. Graduates were not involved but they could attend voluntarily. Of course, at that time there were more undergraduates than graduate students. My guess would have been about three to one, undergraduate to graduate. So that was a little different than at present.

There are a couple of other items. This gets back to relationships with faculty and important personages around Caltech. As freshmen, each of us was invited, and indeed usually went, in small groups to have breakfast on a Sunday morning with Arthur Fleming, in his fine old home on Orange Grove Avenue. John MacArthur, the dean of freshmen, would arrange these affairs, and on a selected Sunday about ten of us would go down and breakfast with Fleming. He was, of course, a very important figure in the early history of the institute. We also, all of us, in small groups again, had dinner with the Millikans during our freshman year, usually on a Sunday evening. John MacArthur saw to it that all of us got down to the Huntington Library—again, in small, conducted groups. On Saturdays, he would conduct a select group through the Huntington Library. We didn't have much of a relationship with the trustees, but I did see quite a bit of Harry Bauer. Not as a freshman but later in my undergraduate career at Caltech, I got to know Harry Bauer fairly well. We served on committees together.

BERRY: He was a trustee?

SHARP: He was a trustee at that time and president of Southern California Edison Company. He had an estate down in San Marino. As I remember, on our graduation day or class day he had a nice reception for the senior class at his home, in the garden. It was lovely, and a very nice thing for him to do.

BERRY: Was he an alumnus?

SHARP: No, he was not an alumnus of Caltech. Caltech didn't have so many alumni at that time, and probably few as old as Bauer. We saw Millikan quite a lot. He had high visibility. He was around the campus much of the time. He was a very handsome man, and even in his older years he appeared to be in very good physical condition. Considering the amount of time and energy he put in on administrative details, it was amazing that he always looked as fresh and vital as he did. He would come to our football banquets frequently and give us a little lecture about the desirability of physical well-being. Before I left Caltech, I think Millikan knew who I was. I happened to be a student body officer in my last couple of years here.

BERRY: What office was that?

SHARP: First of all, I was representative-at-large, which made me a member of the board of control for the honor system, and then I was vice-president of the student body—Johnny Pearne was president. There were institute affairs at which I had to interface with Millikan. I also sat on a student-faculty-trustee committee which was studying student life, way back then. How to improve it. We'd meet at night in the Athenaeum. It didn't do any good, but it was in that connection that I got to know Bauer the best. He was a trustee member of the committee. Of course, we all stood in great awe of Millikan. Bauer was the only man I ever saw who told Millikan off, and it tickled the students on the committee. Bauer would turn and look at him and he'd say, "Millikan, you're wrong, you don't understand people." It was an eye-opener to us, because we had never heard anybody challenge Millikan before.

BERRY: Millikan apparently related well with the students?

SHARP: Yes, in a somewhat distant way. Most of us never got close to Millikan. I never felt warm, buddy-buddy with him, but he clearly was interested in student life, student welfare, student education, and was not just a person sitting behind a desk over in Throop Hall. He was out moving around the campus, and nearly everybody had some contact with him, in one way or the other. You saw him. He was highly visible, and respected—deeply respected—but most students didn't get to know him very well. He was a presence on the campus, no question about

that.

There were a lot of notable faculty, of course—Linus Pauling and Charlie Lauritsen, for example. Incidentally, my freshman roommate had Charlie Lauritsen as his physics instructor. I never was that lucky. I always had graduate teaching assistants.

BERRY: You had no choice of the teacher?

SHARP: No, no, no. We were assigned a section, and then somebody was assigned to handle that particular section. Sections A and B were the honors sections, A particularly. I think Earnest Watson would handle Section A in physics. I never was that good. On the other hand, in terms of general lectures, we got to hear some of the first-rank people. Pauling's lectures were great. Very imaginative, and Pauling is a ham, as you probably know if you watch him on TV. Knowing the man, you can just see him do things that you know darn well are pure ham. He pretends that he can't find the end of a certain chain that he wants to show as an example of a long, complex molecule, but you know doggone well that he knows where the end of that chain is. He keeps fumbling around trying to pretend to find it. He'd use a slide rule and multiply 2 x 2 and get 3.9999967 or something like that.

BERRY: You mentioned Ian Campbell.

SHARP: Ian Campbell was a new young assistant professor when I first came in contact with him, which would have been in my junior year. I had a course from him when he had been at Caltech only about a year, having come freshly from Harvard. I knew him moderately well for the next year or two. On my way east, after my first graduate year at Caltech, I spent some time in Montana in the field with Ian, then subsequently when I came back to Caltech in '47 he was a colleague, and we were very close. He was a very fine person who did a tremendous lot for Caltech. He was devoted to students. Initially he was a very good instructor. I would say that in his later years he wasn't as effective, because he didn't do research in the forefront of his field. He was so busy doing other things. When I first knew him he was great, just great.

Let's go back to the tail end of my graduate work at Harvard and PhD work in Nevada. Basically, I finished up at Harvard in 1938. The year 1937-38 was a sort of a climactic year for

me in many ways. I spent the summer of ’37 in northeastern Nevada in the Ruby / East Humboldt Range, doing my second year of field work on my PhD thesis. Late in the summer, I got a letter from Ian Campbell asking if I might be available to go on a Caltech-Carnegie Grand Canyon boat expedition that he was organizing, to go down the Colorado River from Lee’s Ferry to Pierce’s Ferry through the Grand Canyon in the fall, October-November. We’d be in the canyon about two months, completing a study of the Archean rocks in the inner gorges of the canyon that he and John Maxson had been conducting for a number of years with support from the Carnegie Institution. The problem was that these old rocks are down in the bottom of the canyon, and the only way you could get to them was to go way down into the canyon at one place and then climb out; you couldn’t move along the canyon floor for more than about 100 yards. This wasn’t very efficient, so they got the idea of making a boat expedition through the Grand Canyon. They had been organizing for some time when suddenly I began getting these letters from Campbell wanting to know if I would be interested in going. I sure would! It was an opportunity of a lifetime, so I wrote to Harvard University, to the dean of arts and sciences and to a couple of my professors. Harvard had given me a very nice fellowship for my last year there, the Woodworth Fellowship. I explained about this opportunity to go on the Grand Canyon scientific field expedition, but if I went, it meant I couldn’t get back to Harvard until Christmastime. I’d miss the first part of the fall term; I would really in effect be there only for the second semester. So I asked if they would grant me a leave of absence from my graduate work at Harvard for the first semester, and, secondly, could I have my fellowship for the second semester? I didn’t want them to take it away from me for the whole year. Pretty soon I got a letter back from Harvard which was utterly amazing to me. It was from the dean, but I knew there had been some discussions with Professors Kirk Bryan and Don McLaughlin, who was later president of Homestake Mining Company and still lives in San Francisco. McLaughlin was regent of the University of California for a long time, but at that time he was chairman of the Harvard geology department. These gentlemen had persuaded the dean that this was a very useful thing for me to do. Harvard said yes, I could have the leave of absence to go on this expedition and they would make my fellowship a traveling fellowship for the first semester. So Harvard University gave me a fellowship to go down the Grand Canyon on the Caltech-Carnegie expedition. It amounted to $400 or something like this, but I thought that was just terrific. So I stayed in Nevada until early October and then came down to Pasadena and got together with Ian

and eventually the rest of the party at Lee's Ferry. There were seven of us all told, three boatmen and four geologists. I was the kid of the outfit; I was the gunbearer, so to speak. That was the greatest trip I've ever had. Two months on the Colorado River doing geology, and having the excitement of running the canyon.

Fig. 1. Breakfast scene in the Grand Canyon, Carnegie Institution expedition, fall 1937. Bob Sharp sits at the table (right rear). Other Caltech geologists were Ian Campbell (seated at table, far left) and John H. Maxson (not shown, probably the photographer). A fourth geologist, Jack Stark from Northwestern, and three boatmen completed the party. Caltech Archives.

BERRY: Were the geologists all Caltech, do you remember?

SHARP: No, Maxson and Campbell were from Caltech; the third geologist was a fellow named Jack Stark from Northwestern University, who was a great friend of Ian Campbell's. I was the fourth geologist, so to speak. We had three boats. We got out of the canyon just a little before Christmas. I hadn't been home for Christmas for quite a long time, so I spent Christmas at home

and then took off back to Harvard.

Now I was faced with a tough job. I had to write my thesis and defend it, all within the short period between January and May. I did it.

You asked where I had met Jean, my wife, who at that time was Jean Todd. She was a graduate student at Radcliffe that year, and I knew about her before I ever got back there, because I had had dinner with Dick and Frances Jahns in Pasadena. Dick Jahns was a Caltech graduate who was on the Caltech faculty for a long time. He and I were undergraduates together. He was a year behind me. He had shifted into geology from chemistry in his senior year, and had an awful lot of geology to make up. I was working on a master's thesis out on the Soledad Basin area north of the San Gabriel Mountains at that time, so Dick was given to me as a sort of field assistant instead of being put into the regular field course. His professors figured he'd learn more working one-on-one with me than he would in the field course. So he and I worked together in the Soledad Basin. I knew Dick and his wife, Frances, very well. They had known Jean Todd when Dick went to Northwestern as a graduate student for a couple of years. She had been a graduate student there. At the supper in Pasadena they said, "Oh, gee, when you get back to Harvard, you'll find this wonderful girl, Jean Todd. You must look her up and get acquainted with her." So I knew about Jean before I ever saw her. After I got back to Harvard, I got somebody to point out Jean to me, and I invited her out to supper. We were married in the late summer of 1938, which was the year I finished my graduate work at Harvard. She was a native of Illinois, and we were married at her home in Aurora, which is about forty miles west of Chicago.

BERRY: What did she study?

SHARP: She's a geologist. She was an undergraduate at Carleton College in Minnesota, where the head of the geology department was a fellow named Gould, Larry Gould—Laurence McKinley Gould, to be exact. Jean was his first geology major. Gould later became president of Carleton College and served in that capacity for many years. He is now retired, and right at this moment he's in Antarctica. Larry Gould had been the second in command on Byrd's second Antarctic expedition. He's an Antarctic expert and he's been back a number of times. They're having a big fifty-year celebration of Byrd's flight over the pole this year. Gould is about eighty-

three or eighty-four. He and I went fishing together last summer in Jackson Hole. He is a marvelous fellow.

Anyhow, Jean was his first geology major, and then she went to Northwestern for two years and got a master's degree. That's where she met Dick Jahns. Then she went on to Radcliffe, presumably working for a PhD. In those days, Harvard and Radcliffe were not like they are now. She took courses at Harvard, but she usually had to sit in another room. She could hear the professor, look through the door and see the blackboard, but she couldn't be in the classroom.

BERRY: Really?

SHARP: Yes. I was not taking courses by that time, so we were never in a course together. Anyhow, we got acquainted and decided we'd like to get married, so we did on September 7th, 1938, in her home in Aurora. That summer I had worked for Stanford University in their summer field camp out here in Ventura County and up in the Mono Basin. When the field camp was done, I went back to Illinois. By then I had gotten a job at the University of Illinois. Now we're making a transition into discussion of my professional career.

After we were married, we settled into a little apartment in Champaign. I worked at the University of Illinois for about five years, 1938 to1943. My set-up at the University of Illinois was pretty rough. This was still the tail end of the Depression. The only teaching job I heard of was the one at the University of Illinois. I thought I'd like to have a teaching job in a university, and I got that one, largely because of a fellow whom I had known at Harvard, where he had been one of my instructors, and who was then at Illinois. I think he had a lot to do with my getting the job. Jobs, especially teaching jobs, were very hard to come by, and I was thankful to get one.

What had happened was that the dean of the Liberal Arts and Sciences College at Illinois had decided the geology department had too many graduate teaching assistants. He wanted fewer of them and more professional instructors. So they took four of their graduate teaching assistants and rolled them together and made two instructorships out of them. I got one of the instructorships, so basically I was just two graduate teaching assistants rolled into one. That's the way they treated me. They paid the magnificent sum of $1,800 a year. There was another guy in exactly the same situation from the University of Chicago, who arrived at the same time

as I; he had the other instructorship. So he and I were the two new young instructors in the outfit.

I never worked harder in my life than I did the five years I was at the University of Illinois. At that time, it was not as good a university as it is now, nor was the geology department as good as it is now. I was at the very bottom of the totem pole. I wanted to get out of there. The only way I could get out was to work like a son-of-a-gun, do good work, and, hopefully, make a name for myself that eventually would attract enough attention to merit a job somewhere else.

On the positive side, I've never lived in a community where social relationships were better. I finally decided that this was partly because the countryside around is so uninteresting. Nothing but flat land, cornfields, and hog pens. There is nothing in that country comparable to what we have here, where we can go to the beach, or to the mountains, or you can go skiing, fishing or hunting. You can do 10,000 things. In Urbana-Champaign, what can you do except take an interest in other people? I felt the people there were socially polite, adjusted, sensitive to and thoughtful of others. I've never known a group of people where I had better, honest-to-goodness, social relationships.

I finally got into a group at the University of Illinois which was very good. It was known as Oldfather's Group. [William A.] Oldfather was a professor of romance languages, and every Saturday afternoon throughout the whole year—rain, shine, snow, no matter what—the group gathered right after lunch, got in a bunch of cars and drove off into the countryside, which was easy to do there, to some cow lot or meadow, and played softball. Guys seventy years old and younger people. After the game, we would go to a little wooded patch somewhere, build a fire, and have a steak fry, getting home by six or seven in the evening.

BERRY: Even in bad weather they would do it?

SHARP: Under all conditions. During times of snow, a batter would hit a little pop-up, which might land in front of home plate, within fair territory. Nobody could find the ball, because it was buried in the snow, and the guy would get a home run. We had a nice system; everybody had to rotate through all positions, so you didn't get a fast-ball pitcher who dominated the game. He could pitch one inning, but then he had to rotate. It was fun. Everybody from the provost at

the university down to me at the bottom of the ladder. All departments were represented. That was the thing that really bailed Illinois out for me—this Saturday group. I made friendships through the whole university that I would never have made otherwise. Essentially from top to bottom, and that was great.

The thing that finally got me out of the University of Illinois was World War II. I went into the Army Air Forces in 1943 and spent three years in the Arctic-Desert-Tropic Information Center, which was basically an intelligence outfit. The principal character in the Arctic section that I was in was Larry Gould. I got into that unit primarily because back in 1941 I had a fortunate experience. One day in the mail came a letter—in a big envelope, as a matter of fact, with a lot of pictures—from a fellow named Walter A. Wood. Wood was director of the Department of Field Exploration at the American Geographical Society in New York City. It's a very professional geographical society. For years Wood had been running a series of expeditions along the Alaska-Yukon border. One objective was climbing mountains. Wood had been educated in Switzerland and was a good geodesist. He was a wealthy man, and my only captive millionaire. He would take these expeditions into the St. Elias Range right along the Alaska-Yukon border, and he'd do a lot of surveying to make base maps, as well as climb mountains. He finally decided to have a geologist on one of the expeditions. So he asked around in a few places, including Columbia University, because the American Geographical Society offices are practically across the street from that university. He got my name. I don't know who gave it to him, but God bless them, they did. Wood was to be in Chicago to give a lecture and asked if I could come up to Chicago to talk to him about the possibility of joining his expedition for the next summer. So I went up and it all worked out.

So in 1941—this was just before the war—I went off to the Yukon for the whole summer with the Wood Yukon Expedition. I had a marvelous time. We had an officer from the Army General Corps attached to us, because the army was beginning to get uneasy about wartime business and the possible need for mountain troops. So we had a lot of help and support from the armed services. We had air drops from an airplane that was flown up from Georgia to drop stuff to us in the mountains.

I mapped geology that whole summer, about three months, in a valley far back in the St. Elias Range. I had a marvelous time. Next to the Grand Canyon, that's the second best trip I ever had. As a result of that Yukon experience, relationships were established which eventually

got me into the Arctic-Desert-Tropic Information Center of the air force. I was in the Arctic section because I had had this relationship with people who worked in that area, though I don't think I was much of an Arctic expert in any sense. I spent three years in the air force, part of the time in Alaska and out in the Aleutians. I was very glad to get out of the armed forces. I didn't like the armed forces set-up much.

BERRY: Was this geological work?

SHARP: In a sense it was related to geology, but not strictly. I worked on airline route guides a great deal, and then on confidential material. The last thing I was working on was a big report on Siberia. It was mostly, in that case, a literature search; I couldn't go to Siberia. It's clear, and I didn't quite realize it at the time, that there were plenty of people in our armed services, and perhaps higher in the government, who knew that when the war was over, Russia would be an antagonist rather than an ally. So they were building up as much information as they possibly could about Russian territory. I did a lot of different things in the air force, including survival procedures in the Arctic.

I got out of the air force in '46. Here I should note that for a short period of time back in 1943 our Arctic section had been stationed at the University of Minnesota. We had occupied offices in the big Northrup Auditorium at the University of Minnesota, and I had gotten well acquainted with the Minnesota geology staff at that time. Their geomorphologist died of meningitis before the end of the war, so they were looking for a geomorphologist. They remembered me and offered me a job before I got out of the air force. I resigned my leave from the University of Illinois and took the job at Minnesota. It was a better job, a better set-up, and I liked the country better. So when the war was over, I ended up at the University of Minnesota.

BERRY: As associate professor, is that it?

SHARP: I think I went as an associate professor. I had gotten up to assistant professor at Illinois before I left there. In the early postwar years, there was a lot of what I would call musical-chairs activity going on. College staffs had been decimated. Professors had gone off to war, many had not returned, and few new ones had been produced. Students now flocked back. Colleges were

looking for people to do the teaching. A lot of their own guys were gone, so there were a lot of jobs and I kept getting a lot of inquiries and offers. I had been at Minnesota only about a year and a half when I received offers from Caltech and Stanford.

Now, I am a Westerner. I was born and raised in the West and I've always had a yen to live in the West. It didn't have to be California, but I preferred to live in the West. I had been, by this time, twelve or thirteen years in the eastern part of the country. Having finally gotten back to the east bank of the Mississippi River, I figured that was about as far west as I was going to get. I was really settled down and was happy at Minnesota. Minnesota was a well-run, big state university. The geology department was good, and I liked my associates and colleagues there. I liked the people of Minnesota; they're fine people, Scandinavian stock. I was doing research work in northern Minnesota as part of the Geological Survey, in the summertime. It was a very nice set-up and I was happy. In my second year at the University of Minnesota, I had three invitations. The University of Illinois asked me to come back and head up their geology department. I went down and spent a day talking to them. I didn't think I wanted that job, but nonetheless I went and talked to them. Stanford University also offered me a job about the same time. If you had told me earlier in my career that I'd ever turn down a job at Stanford University, I'd have said, "You're nuts! I would never turn down a job at Stanford, because it's one of those wonderful academic places." They'd always had a good geology department, the environment was good, and it would be great to be there. Stanford was just about top of my list. About the same time Caltech offered me a job. This was in the first year of [Caltech president Lee A.] DuBridge's regime at Caltech, so things were stirring and in ferment here. I looked at these three possibilities. Back to Illinois as chairman of the department, which was not very attractive—but the idea of going to Stanford was very attractive. I finally chose Caltech, even though I had some serious reservations. I'd been an undergraduate student here. Although I had been away for essentially thirteen years, I didn't like the idea of coming back to my own alma mater; that was a negative point. Other considerations were positive. The Caltech offer was financially better than Stanford's. Stanford at that particular time had a feud going on in the geology department between the chairman and another very vigorous person, whom I won't identify. I was a protégé of this other person, so I thought it was not going to be a very comfortable spot for me, although the chairman had apparently acquiesced and said he'd go along with my appointment. The chairman, Al [Arville I.] Levorsen, was a Minnesota graduate,

and I knew him casually. I'd met him, I'd talked to him, I liked him, but I thought I was going to arrive under a cloud at Stanford, so I came to Caltech. In hindsight it was a fortunate decision, but at the time I wasn't quite so sure.

So having spent three years at Harvard, five years at Illinois, three in the Army Air Forces, and two at Minnesota, I arrived back at Caltech as a faculty member. This was very early in Lee DuBridge's career here. He came in '46, I think, and I arrived in the fall of '47. John Buwalda, who had been for over twenty years chairman of the geology division, had just stepped down from the chairmanship, and Chester Stock occupied the head spot. This office was Stock's; all these bookcases and everything else belonged to Chester Stock. Chester was just the opposite of John Buwalda. Buwalda ran a tight ship. Chester operated with a very light touch. As a matter of fact, Chester was really not well adapted for this kind of job, though he was good by contrast. He died within three years, of a cerebral stroke. Then Ian Campbell became acting chairman for a couple of years in the interim, and finally I succeeded Campbell. I became division chairman in '52, five years after my arrival in 1947.

You have a question in your outline about Millikan's interest in geology. That's a good place to start, as it gets us into the history of the development of Caltech's geology division. If you read R. A. Millikan's autobiography, you'll discover that during the First World War he was in Washington, where he was instrumental, with other people, in organizing the National Research Council. These people were concerned with applying science to the war effort, particularly for submarine detection and other such things. One of the men in that group was John Campbell Merriam, a University of California professor. He was a vertebrate paleontologist. He was also a very able, politically adept, ambitious man, and subsequent to his Washington service he became president of the Carnegie Institution. This was a very prestigious position, which he occupied until he retired. Millikan decided along about the mid-twenties, I'd guess 1925 or '26, that Caltech should have a geology department. Whether somebody put this idea in his mind or not, I don't know. I wouldn't be surprised if somebody did; there were a lot of mining men involved with the institute—Arms and Mudd, for example—and they were probably leaning on Millikan a little bit to do something about resources. Anyway, he concluded that Caltech should have a geology department. My guess is that Millikan didn't know many geologists, but he did know John Campbell Merriam, with whom he had served on the National Research Council in Washington. So he turned to Merriam for advice and guidance. Merriam

sold him on two of his own fair-haired students from the University of California, John Peter Buwalda and Chester Stock. Buwalda was a professor at Berkeley at that time and Stock maybe had just finished his graduate work, I'm not sure of his status.

Begin Tape 2, Side 2

SHARP: Geology has a lot of practicality in it. In some ways in those days it was an applied science. So you can see why Millikan might go in that direction. As far as I know, that covers Millikan's interest in geology.

You wanted to ask about the condition of relationships between geology and the other divisions when I first arrived here in '47. In '47, I'd say physics was still the dominant division on the campus. Since that time biology has become important and grown large. There was a time when mathematics might have expanded to equal rank, but that seems not to have occurred. I think geology was accepted and respected in 1947. I can remember [Richard Chace] Tolman, a physics professor—my office in those days was on the third floor at the east end of the Arms building [Charles Arms Laboratory of the Geological Sciences] near West Bridge [Norman Bridge Laboratory of Physics], and once in a while Tolman would appear at the door of my office. This distinguished gentleman would come in and chat for fifteen or twenty minutes in a very cordial, open way, with seeming respect for geology.

I mentioned that I had been hesitant about coming back to Caltech because I had been an undergraduate here and feared that people would still think of me as an undergraduate student. However, I got none of that. I was welcomed warmly as a full-fledged faculty member. Of course there had been a lot of changes, but there were a lot of people still here that I had known as an undergraduate. Some of them had been my instructors. They accepted me as a faculty member, so I had no problems on that score.

BERRY: Did Tolman discuss geology with you? Your work?

SHARP: A little bit, but mostly our conversations were general, almost social in nature. He said he hadn't been looking for me but for somebody else, but he saw me, so he stopped in and talked. The physicists didn't regard us as a bunch of bums, and chemistry, possibly because of Pauling's interest in mineralogy, was especially cooperative. Caltech is a tough place in which

to be a faculty member. I don't mean to say it isn't a good place. It's an exciting place to be, but it's a rough game, for several reasons. Your students are very demanding, and most of them are smarter than you are. I feel that. You have to work very hard to do justice to the students, and you have to be ready to say, "I don't know." It is very demanding in that sense, and the atmosphere is very competitive. Maybe "competitive" is not quite the right word, but the tradition is that you are expected to do first-rate research like that being done by your peers, most of whom are pretty darn good. You work like a son-of-a-gun to keep up with the pace. Caltech is not the most comfortable educational institution in the United States at which to be a professional staff member, but it's surely one of the most exciting.

I haven't really answered your question as to relationships with other divisions. By and large they are pretty good, better than in most other educational institutions I have known. Best of all, they have gotten better through the years. Currently, we work well with parts of physics, astronomy, chemistry, engineering, and occasionally biology. Our relationships with JPL [Jet Propulsion Laboratory] are among the best on the campus.

I hadn't been here but about two days in 1947 before John Buwalda walked into my office and said, "I have been teaching the elementary physical geology course in this department for over twenty years. I don't want to teach it anymore. I'm tired of it. Here are my notes, you do it." Now, he wasn't the chairman of the division, so it really wasn't his prerogative to tell me what to do. But he was a very senior member, and I said, "Yes, sir." So suddenly, in just a matter of a couple of weeks, I found I had inherited a big elementary physical geology course, which at that time was required of all students at Caltech. So I really had to turn to and do that task with a vengeance. I taught the course for about twenty-five years after Buwalda unloaded it on me. I was glad to do it, because I think that's probably the most important course taught in the whole division. What we had was three lectures, plus a laboratory and recitation periods and field work. The recitation sections were handled by graduate teaching assistants, as were the laboratories, although I always took a laboratory and a recitation section to know what went on in the front-line trenches. There were five or so graduate teaching assistants who worked with me to handle the other sections. We had as many as ten sections, something like that. In addition, I taught courses in my specialties—in general geomorphology, arid-region geomorphology, glaciology, regional geology, and so on.

Teaching loads at Caltech were about half of what I had been carrying at the University

of Minnesota, so I had a lot more time to do research. In 1952, I was made division chairman, more or less over my own dead body. DuBridge had leaned on me very hard about a year before. I had declined, but he finally came to me and said, "You've got no alternative. You've got to be a good soldier and put your shoulder to the wheel and take this job." I was division chairman for about fifteen years. In those times, division chairmen tended to linger longer than they should have, and I think fifteen years was a little longer than I should have stayed as division chairman. However, it was rather traditional that once a chairman got the job, he stayed there until he retired. Fred Lindvall [chairman of the Division of Engineering and Applied Science] and Hallett Smith [chairman of the Humanities Division] both went well over twenty years. Buwalda had been chairman for over twenty years. It took at least five years to learn how to do the job. There was no charter. Nobody told you what you could do or what you couldn't do. First thing I did was to call up [George] Beadle [chairman of the Biology Division] and [Robert] Bacher [chairman of the Division of Physics, Mathematics, and Astronomy], and I said, "Will you guys come and have lunch with me? I need to ask you some questions." So we had lunch together in the Athenaeum and I kept putting questions to them and they'd look at each other and asking, "Well, how do *you* do it?" Then they'd discuss and often say, "Gee, I don't do it that way." Finally at the end of the luncheon, I think it was Beadle who said, "You know, we ought to get all of the division chairmen together and talk about some of these things." That was the beginning of what used to be called the division chairmen meeting, which has now become the Institute Academic Council, the IAC.

It started first of all with just DuBridge and the six division chairmen getting together informally to talk about common problems, those that I didn't know the answers to; then it got a little more formalized. Eventually the vice-president for business affairs was added, but at that time that was about all. It was a small but very good and effective group. Every month we'd have a meeting.

BERRY: Would you give an example or two of what type of problems you discussed?

SHARP: Yes, and I also will indicate how it worked. This was interesting, because the group never voted. A lot of matters were discussed. Questions concerning promotions and salary changes and institute financing were taken up. Aside from those matters, which are the normal

kind of operational problem, the group dealt mostly with institutional problems, although any division could ask for discussion of a problem of its own. They'd say, "We've got a problem, we need your advice and help on this," and so on. DuBridge would chair the meetings. He'd say, "Well, today we ought to talk about the problem of: Shall we have an increase in tuition? Other institutions have done this." Then we'd have a long discussion on tuition. Or, "What do you think we ought to do about the industrial relations sections?" Bob Gray wasn't there at that particular time, and whoever had been running it was retiring, and so we'd have a long discussion about whether we ought to continue the industrial relations section. These were campus-wide problems mostly. Each division came in with a budget proposal and everybody hacked at it. But the group never voted. DuBridge would make notes, he'd sit there and get a feeling of the general attitude on a subject. Then he'd say, "Well, OK, I guess what we'll do is this." The result was a sort of consensus of the feelings of the group. It actually became, in a sense, the governing body of the institute. It just sort of grew and evolved in this curious way.

BERRY: You didn't do any teaching then, while you were chairman?

SHARP: Oh, yes, I did!

BERRY: Research?

SHARP: Oh, yes, I did both teaching and research. I feel very strongly about that. It's very important that administration figures keep contact with the front-line activities of the institute. My teaching load did not decrease much. I still handled the elementary course but stopped teaching the laboratory and the recitation section. I had graduate teaching assistants do that and also had help from Clarence Allen. I would space my other courses out a little differently. But I still carried very nearly the same teaching load that I had before. My research suffered, but I continued to do research, and I think that's highly desirable. The institute encourages you to do this, and it's a sound policy. At that time in the administrative group, DuBridge did no teaching or research. Bacher was involved in research, but he wasn't doing any teaching, as far as I know. After Bacher became provost, I guess he probably wasn't much involved in research either.

You asked about courses taught; I've touched on that. As to the matter of outstanding

students, I'm a little hesitant to get off on that, because I will forget to mention some. When you mention a few, you do injustice to others. But I had fellows like Ron Shreve, who is now a professor over at UCLA. He was just here, incidentally, as a Fairchild Distinguished Scholar last year. He has done very well. I could remember John Andelin; he was Mike McCormack's right-hand man in Washington for a while. Andelin was not a geology major. He just took elementary geology, and he and I then became friends as a result of that. Johnny was here as a visitor on the campus about a year ago. The YMCA brought him back as one of their distinguished visitors. A lot of former students are here—[Leon] Silver, [Barclay] Kamb, and [Clarence] Allen, for example—and they have obviously done well.

There are a few little high points there that are interesting. We were building our division all through those years, and I can remember one time when we had an opportunity to hire two young assistant professors. We looked all over for people and finally had a line on three that we'd like to hire, but we could place only two of them. Those three people were Leon Silver, Clarence Allen, and Gerry Wasserburg. We had already gotten Frank Press here. That's an interesting sidelight I should talk to you about. Harrison Brown was also here. Press and Brown were both highly entrepreneurial and good operators. Thanks largely to them and their imagination, and using some money that I had managed to raise independently, we ended up hiring all three of those young men—Silver, Wasserburg, and Allen—all in one year. They have all subsequently been elected to the National Academy of Sciences. That was just one of those things, which in retrospect you realize involved a lot of blind luck. But it was blind luck that worked out for us. It is from those kinds of things, of course, that an administrative figure must derive his satisfactions. He glories in the accomplishments of the group he helped build. He has to learn to take quiet satisfaction in the fact that somewhere along the line, somehow, things fell into place and worked—maybe because he jiggled them a little bit. Such things are never written down anywhere. Nobody ever comes up and says, "By God, that was a brilliant move—you guys hired all three of those fellows in one year." But you yourself know it happened and that it has worked out well for the institute and for your own department, so you are happy.

I want to talk just a little bit about the geology division in historical perspective. Let me go back and touch on some past history. This is another one of those things that doesn't get written down, and I think it's important from the standpoint of our department that some aspects be recorded. You asked me why I thought it was a good idea to go east for graduate study. One

of the reasons was that at the time I was here as an undergraduate, Caltech had a small and modest geology department, which in hindsight was surely provincial. It gave me a good education, but its horizons were limited, and I didn't realize that until I got away into a completely different environment. Then I found, to begin with, that these people whom I thought were great figures in geology had never been heard of in other places. They were not regarded as great figures in geology; so, you know, your perspectives get changed. John Buwalda built a very sound operation based on discipline, toughness, and a devotion to field work. As I say, he recognized the opportunity in seismology. The Caltech geology department was established on a classical base into which Buwalda integrated seismological work, the earthquake business. Initially, the Seismological Laboratory belonged more to the Carnegie Institution than to Caltech and the first people in the laboratory were Carnegie Institution people—Harry Wood, and of course Hugo Benioff, who had been an astronomer at Mt. Wilson. Benioff didn't like to stay up all night, so he turned to seismology. Buwalda kept capitalizing on every opportunity for integrating the laboratory more closely with Caltech. He brought [Beno] Gutenberg over from Germany. Gutenberg was a Caltech appointment, not a Carnegie man. Eventually Caltech wound up with a whole seismological laboratory in its pocket, largely thanks to Buwalda. The fact that the operation prospered was not solely due to Buwalda, but he was an important influence. Thus we had a classical geology department, with seismology attached. This made us different, and I think established right away a fortunate frame of mind. Our Seismological Laboratory is world famous and has been for years and years and years.

Then, when Stock died in 1950, we had an extended introspection within the division. I was just one of the young guys involved. Ian Campbell was the acting chairman, and in some ways the guiding light. We elected to go into the field of geochemistry. Subsequently, we gave up vertebrate paleontology, which had been Stock's field. We didn't do it precipitously, however; we actually struggled with the problem for seven years, but in the end elected to phase out vertebrate paleontology. To get started in geochemistry, we obtained Harrison Brown from Chicago, and Harrison brought Clair Patterson and Sam Epstein with him. At that time this was quite an innovation. I would go to national geological meetings and geologists would come up and hiss in my face, "How's the department of geochemistry at Caltech?" With great scorn, I used to say, "Just be patient and give us time." It took about twenty years, but our geochemical operation has proved to be a huge success. We were lucky. We were lucky in that we got into

the isotope phase of geochemistry. Isotope geochemistry is now widely practiced in a great many other places, but it really started at the University of Chicago, which should have capitalized on the opportunity but somehow didn't have the flexibility, the imagination, or something else that I don't know. So we finally ended up as the wellspring for this sort of work, and that was very fortunate.

As geochemistry developed, a curious thing happened, and I hope it will happen again. We had on our staff here perhaps three people who would have been called geochemists anywhere. Slowly, other staff members became involved in the geochemistry program, and although we never called them geochemists, other people would have, because of the nature of the work they were doing. Hugh Taylor, Lee Silver, and Heinz Lowenstam are examples. What I'm saying is that you get a new discipline, a new opportunity started, and then people will come and work in that from different parts of classical geology. I'm a classical geologist, a geomorphologist. Eventually I did a lot of work in geochemistry. Sam Epstein and I wrote a good many papers, based on isotopic techniques in the study of snow, ice, and glaciers. At that time this was pioneering work. When I said it was fortunate that I came to Caltech and didn't go to Stanford, that's partly what I had in mind. I would never have had these opportunities at Stanford that I had at Caltech to do work in geochemistry in cooperation with the geochemists. That's the great thing about this place: There is the opportunity to work in cooperation with experts in other fields, where you bring something, they bring something, and together you make a kind of music you can't make individually.

This happens throughout this whole division. Just because we went into geochemistry didn't mean that we crossed out seismology. Not so, we just added a new aspect to our operation at that time. The Seismological Laboratory staff had been, when it matured, Gutenberg, Benioff, and [Charles] Richter. When I first got into the chairmanship, they were inadequately housed out in the San Rafael Hills laboratory. I finally got a grant from the Kresge Foundation that made it possible for us to buy the Thorsen building, near our original lab. It became the Dornelley Laboratory, thanks to a gift from Mrs. Pardee Erdman, made in honor of her father. One day Frank Press came to me and said, "We've got to have still more room." We had all been unhappy that the seismological group was so separated from us on the campus. So I said to Frank, "I can't get you more room out there, but if you will come to the campus, I will try to get you a building here." He finally bought that idea, and that's how we got them down there into

South Mudd.

Much earlier, I had gone to the administration and said, "Look, Gutenberg is old, he's got heart trouble; Richter isn't in much better shape, and Benioff is not a healthy fellow. We could find ourselves nowhere in seismology overnight if we are not careful. How about letting us go find the best young geophysicist in the whole country and bring him here as a young fellow." They said, "Go ahead." There's another nice thing about Caltech: If you get an idea that has merit you can often operate quickly on it. So we zeroed in on Press, made a proposal to him, and he agreed to come, but not for two years. We actually had appointed him to our staff two years before anybody knew he was to come here. Frank proved to be great. As you know, he is now science advisor to the President. We had lost him to MIT several years before that, which was too bad. We've lost some very good men out of this department: Frank Press, Al [Albert] Engel, Dick Jahns.

BERRY: Campbell, also?

SHARP: Campbell, yes. Though in Campbell's case, there were other considerations. Ian had gotten so deeply into administrative functions that he was not doing science anymore. When he left Caltech he became chief of the California Division of Mines. That was really a pinnacle to his career. We lost a great deal in terms of personal relationships when Campbell left, but in terms of science and teaching we didn't lose that much. That's neither here nor there; he was a great guy. In no way do I mean to say he wasn't.

This leads to a third thought. Our geochemistry operation was maturing, so we began looking around and said, "What do we do next?" Press, who was still with us at the time, wanted us to go into ocean-floor geophysics, which is a great field. The trouble with ocean-floor geophysics is you have to have ships. Owning a ship is like marrying a harem: You've got problems. When the ships are tied up at the dock, they still cost you so much money it's incredible.

We debated this for a long time and finally said, "No, let's not go that way. Let's go into space science, because the bus, in a sense, is right at our back door. Through JPL, we have an opportunity to participate." Now, either decision would have been good. Ocean-floor geophysics has been just terrific, except there we would have entered at the bottom of the totem

pole behind a long string of other successful operations—Woods Hole, Scripps, Texas A & M all had big operations and we had nothing. We had a greater chance for uniqueness by allying with the space program, and it worked out very well. So we now have a planetary science group in our division. Again, this has been one of the great blessings of the set-up at Caltech. Most geology departments, at the time we went into geochemistry, would not have been able to encompass the geochemists and they would never have encompassed the planetary scientists. Duane Muhleman is a radio astronomer, and it's a major job to make a Duane Muhleman comfortable in a geology department, or to make the geology department comfortable with him. I think it's now a comfortable relationship, but you have to work at those things; they just don't happen automatically. The atmosphere of Caltech, God bless it, is the thing that makes that possible. At ninety percent of the other schools in the country, it isn't possible. Astronomy was very helpful, incidentally, in our drive into planetary science.

For example, I sat on the visiting committee of Harvard's geology department. As a matter of fact, I inherited the chairmanship [of that committee] from Ian Campbell. When Ian died, I found he had stacked things so I ended up with the chairmanship. I served one year, wrote a rather tough report, and resigned. Harvard's geology department is not in very good shape. In discussing this with them, you find that the big hold-up is a curious image of what constitutes geology. You say, "Look, you need to get into some new stuff," and they say, "It's not geology." We were lucky at Caltech that this view did not prevail.

The big problem that faces the Caltech division at the present time is, What do we do next? Our planetary science is now matured and is a recognized, successful operation. We have about five guys over there and they are doing great, a lot of students, and so on. What do we do next? That's where the division is right now. We went from geology to seismology to geochemistry to planetary science. When we got into the planetary field, I ended up doing planetary science, and I'm a geomorphologist and an old-line geologist. We need to find a new field so that sort of thing can happen again.

BERRY: Did you do the moon and Mars?

SHARP: I didn't do the moon, but I did Mars. [Eugene] Shoemaker did the moon. He's also primarily a classical geologist, but he's better than I am. It was the opportunity to participate and

make contributions to these marvelously exciting modern programs from your base of classical geology that attracted us into planetary science. I had a ball doing planetary science, and I think I was able to make a contribution. I got into the game in a peculiar way, and it wouldn't have happened to me at Stanford.

What happened—going clear back to Mariner IV to Mars—was this. Gerry Neugebauer was still at JPL at that time, and he knew that a proposal being made to NASA to do a TV experiment was no good. So he came down to Bob Leighton [chairman of the Division of Physics, Mathematics, and Astronomy 1970-1975], and said, "Bob, as a duty to our community, you've got to make a proposal on the TV experiment to Mars. This other one is terrible." So Leighton did. He knew how to put the instruments together, but he needed some help in terms of interpreting the results, so he enlisted Bruce Murray, and Bruce Murray enlisted me. So the TV-investigative team on Mariner IV was Leighton, Murray, and Sharp. I was the tail-end of that operation, but from there we went on to Mariners VI and VII, and Mariner IX. These are exhausting operations; you can stand only about three or four, so except for Murray, we got out after Mariner IX. Murray did Mariner X to Mercury.

I would never have had the opportunity at Stanford that I had at Caltech. It reflects the richness of the place. I'm just an old-line geologist, but I had these wonderful opportunities to work in geochemistry and, to some degree, a little bit in seismology, and more in planetary science—because of the set-up and atmosphere here.

As I said, our geology division right now is faced with, What do we do next? I hope we'll have an operation that will start with three or four guys and will be so exciting that eventually about forty or fifty percent of the rest of the division will be coming on over—even the planetary scientists. We're talking about natural resources. You've probably heard a little bit about this. We planetary scientists could make a big input to this endeavor, in terms of remote sensing. With respect to Earth, it is a primitive art at the moment, very crude, and the planetary scientists are experts in remote sensing. They have been at it for 300 years, in a sense, with respect to other planets.

That's a hasty historical perspective on the division. I think we were just plain lucky to be able to move through those various stages without destroying the whole operation. But the division is now faced with finding a new focus of interest, an area where the action is going to be at a high level in the future. We've got to be willing to give up some old things in order to move

into something new. It's what I call the file problem. It's a principle I try to practice myself, but I used to have trouble getting the rest of the staff to do so. They'd say, "I need another file cabinet." So I'd say, "Look, just go through that cabinet you now have, and throw out half of what's in there. Then you'll have plenty of room, and you don't need another file cabinet." You can't quite do that with the division, particularly with regard to staff and other personnel, but you can get the staff to reorient itself.

BERRY: It seems to be one of the things that Caltech has done so successfully,

SHARP: Yes, that's right.

BERRY: It looks for new directions.

SHARP: New directions, and then the people already here find those new things so exciting that they get caught up in the activity. You can't direct them, they have to shift over of their own free will. I will never forget when I was division chairman and about six German research people came for a visit. It was a delegation; somebody brought them to me. They sat down and then said, "Now please tell us how you direct your research program." I just burst out laughing. I said, "I'm sorry, gentlemen, I don't direct it." "You don't direct it?" I said, "No, no, I don't direct the research program. It just goes the way it's going to go all by itself; I don't direct it. We try to make an atmosphere here that's good for creative work." Basically, that's the way things are at Caltech; it becomes so attractive for somebody to do work in a certain area that he or she goes into it voluntarily. It's exciting, it's fun. That's the motivation. You don't go to someone and say, "Well, you've been doing just fine on all this stuff, but now we want you to stop and become a paleontologist." You know, they'd throw you out of the office in thirty seconds. But if, for example, something was going on in paleontology, and this person saw that he or she could make a contribution and it was exciting, they'd be over there on their own power and welcome. That's one of the great things about Caltech.

ROBERT P. SHARP

SESSION 3

January 9, 1980

Begin Tape 3, Side 1

SHARP: You asked me about backpacking, and how I got started doing it. As a kid I had done some packing in the mountains locally, but it didn't amount to much. We'd tie a horse-collar roll of blankets around our neck, put on a knapsack, and spend a day or two or three out in the woods. But you couldn't carry very much stuff. I had never done any really honest-to-goodness backpacking until 1941, when I went to the Yukon with the Wood expedition. I bought a Trapper Nelson backboard in the Hudson Bay Store in Vancouver. At that time, there was not a lot of backpacking equipment around, but the people in the Northwest wilderness did some backpacking. I don't think you can buy a Trapper Nelson at the present day, but in those days it was regarded as a good packboard. I fell in love with mine. I was amazed how independent you could be, in terms of the supplies and equipment you could pack on your back with a Trapper Nelson. I subsequently used backpacking a lot in my research work in Alaska, Canada, the Trinity Alps, the Sierra Nevada, and various other places.

BERRY: Glacier work?

SHARP: Glacier work and mountain work, and then also I did it for recreation and fun. I had a couple of golden retrievers, and we used to go way back in the Sierra Nevada, just the dogs and I, and we had a wonderful time. You asked me about jogging. I got started doing some jogging at the University of Minnesota back about 1946. When I got to Caltech, Dick Jahns was here, and he was always organizing basketball and touch football games. However, I soon discovered that I was getting too old for football and basketball, so I started running again at Caltech about 1948. I've kept it up since that time. I still jog except when my gimpy old knees get too sore. I run up in Santa Barbara, and I run here when I'm in Pasadena. My most favored sports when I was in the East—I think I have already told you this—were sculling and squash. Jogging is not really a favorite sport, but it's a way of getting exercise. I skip rope. I have a rowing machine, which is a great exerciser. I'm using that more and more as my legs start to give me trouble. I

guess fly fishing for trout is my most favorite activity. When my kids were growing up, I got into the downhill skiing operation. I don't ski very well, but I think it's a great sport. I go every now and then and have a wonderful time.

Let me go back and see if I can't bring my research more sharply into focus. As I progressed through my career in doing various types of research, I came to have a greater and greater interest in what I call today's geology. Geology is in many respects a historical science, dealing with about 4.5 billion years of Earth history. Many of the things geologists deal with involve past events or conditions and materials that are old. Things that happened a long time ago. I've gotten more interested in the things that are going on currently on the earth's surface in the way of geological processes, events, and their products; that's what I mean by "today's geology."

A lot of geology is yesterday's geology—something that happened way back. I found it satisfying and a lot of fun to deal with current processes. I particularly like to work with things you can measure. That's how come, in part, I got into research on glaciers. They're dynamic bodies, and you can make measurements on glaciers—how fast they move, what the velocity distribution is, what their growth and shrinkage is, and so on. To some degree, the work I did with wind is related to the same thing. Wind is an active process, and you can make measurements of its effects. How fast does a sand dune move? By what mechanism does it move? What do sand ripples do? So much of my research work has been focused on current activity, where you can set up observation stations and maintain them for five years, ten years, or sometimes fifteen years—which I have done in the desert—and come up with useful, worthwhile figures and data, because the processes observed go on rapidly enough. I've done a lot of that, particularly after I got out here to Caltech. We have the desert in our backyard, and since I had always been interested in deserts, the opportunity to work directly with arid-region features was welcome. Desert dunes, desert soil, desert landscape features, wind action, desert varnish—the dark weathering coat that develops on stones in the desert—even the stupid skating stones on Racetrack Playa.

BERRY: How did you happen to name some of those rocks out there?

SHARP: That was just a quirk of humor. The weather service in Miami has for years been

giving names to storms that develop in the South Atlantic: The first storm of the season will always be a name starting with A and then B, C, and so forth. Initially they used girl's names almost exclusively. I think they now have run into a little problem and have to use men's names as well. This practice always amused me, and I thought, "Gee whiz, if the Weather Bureau can name storms, why can't I name stones out here on Racetrack Playa?" We had already given them letters, A, B, C, D, E, F, G, but anybody could do that. The first one I named happened to be for the wife of a graduate student, Jim Lawrence, who was working with me on the project—Mary Ann. Mary Ann was a very fine stone, and she behaved beautifully. I named one for my mother, one for my great-aunt Grace. Also for a lot of the technicians and secretaries here in the geology division. I had no trouble finding plenty of names. Some of the girls were quite pleased, but I had to be a little careful. There was one very large rock out there that had moved and made a track. We labeled it, but it didn't move during the time we were observing it. It weighed about 700 pounds and I had to be careful—if I named that for some fairly robust lady, she would say, "You think I'm a great big old tank." So I very carefully named it for a very small, petite Chinese girl who was working here in the division at the time. We called that one Karen, as I remember.

Much of our scientific writing tends to be too stiff and too staid. I thought naming these stones would spice things up a little bit. I got away with it then, but I recently had a paper dealing with the Algodones Dunes, down in the Imperial Valley. I was studying the big intra-dune flats. These are very curious things—great big flats that expose the substrate underneath the dunes. They are dynamic features that are moving through the dune chain. Initially I labeled them A, B, C, D, but I also gave them feminine names. But the editors made me take those out before the paper was published. Perhaps because I didn't name a flat for the girl editor processing the papers. I didn't get away with naming flats in the published paper, but I had a lot of fun doing it in my field notes.

I mentioned earlier that Caltech was a great place to be because of the opportunities of working with geochemists and planetary scientists, particularly the Martian project. Currently I'm cleaning up odds and ends of unfinished research tasks and working with some of my graduate students. In the last year or two, I've been to Hawaii several times, working with a couple of our former students on some interesting projects. Since vulcanology is an active process there, I'm still true to my basic philosophy. We don't measure much, but we deal with

something that isn't very old, 100 or 200 years at the most.

BERRY: Is that on the island of Hawaii?

SHARP: Exclusively on Hawaii. We worked in the Kau Desert, where nearly always steam and volcanic gases are drifting about and you are walking on a lava flow of 1971 or something even younger.

One thing you asked about was the presidential search committee. That's getting into administrative things again. This was a faculty committee appointed with the consent of, and at the wish of, the trustees. The trustees, after all, have the absolute word as to who the president of the institute will be, and they don't have to consult with anybody. The trustees own the institute, and they hire the president to run it. They don't have to ask the faculty whether the faculty would like a certain person or not. I don't know whether Bob Bacher had talked to Arnold Beckman, who was then chairman of the Board of Trustees, suggesting a faculty committee or whether Arnold, all on his own, had recognized that whoever came in as president would have an easier time if the faculty was happy about the person and felt they had had a hand in determining who he might be. Anyway, Arnold requested Jesse Greenstein, who was then chairman of the faculty, to appoint a committee. Jesse came and talked to me about it, and we drew up a list of possible members, being sure we got campus-wide representation. I guess the committee was actually appointed by Greenstein, with Arnold Beckman's blessings. The committee consisted of about a dozen people—very good people—from around the faculty. I don't quite know how I ended up as chairman of the committee, but I did. Greenstein couldn't be chairman, since he was the person who appointed it, but he became a very active and constructive member of the committee when he was no longer chairman of the faculty. We worked very, very closely with the trustees. As a matter of fact, the trustees used us as their search and evaluation arm.

BERRY: This was the first time it was done at Caltech?

SHARP: That way, yes, I think. When Lee DuBridge was appointed, it was right at the end of World War II and things were somewhat disorganized. There may have been individual faculty consultations. I don't know, because I wasn't here. Jim Page was chairman of the trustees, and

Jim didn't talk a great deal to the faculty. I think he just up and announced to the faculty, "We have gotten Lee DuBridge." There certainly must have been some faculty input, but it was probably informal and on a personal basis. The search for DuBridge's successor was the first time that Caltech had had a faculty committee of this type, so there was no precedent as to how we would work. The trustees had a small committee of their own: Arnold Beckman, Norman Chandler, and Tom Watson. Our whole committee, as such, never met with their committee, although as chairman of our committee I worked closely with Arnold Beckman and met occasionally with other trustees and with the trustee committee. There was good communication. If the trustees had a thought about a candidate, it was relayed to us. They didn't plow ahead independently but circulated thoughts to us for evaluation. We, on the other hand, also worked completely independently in soliciting names and investigating people. We did a lot of work by telephone. I had discovered way back that you could do a much better job of getting somebody to tell you how they really felt about a person on the telephone than in writing. As soon as they start writing, they get very cautious, but they would discuss things openly and frankly on the telephone. We did a tremendous amount of telephoning. We met frequently over a period of about a year and a half and developed an extensive list of candidates.

BERRY: Who did you telephone to, professors at other institutions?

SHARP: Yes. A lot of people in academic institutions, but also people in foundations who dealt with academic institutions, and people in business and government. We made no secret that we were looking, and we didn't beat around the bush. We would have liked to have been able to do a little more circulating ourselves on the outside, talking directly with possible candidates, but Arnold Beckman was hesitant to have us do that. We did a little direct contacting. For instance, toward the end of the search, Norm Brooks, Bob Christy, and I made a trip to the East Coast to talk to two or three people. Arnold Beckman did some direct contacting himself. We'd say, "Arnold, we need to know something about this fellow," and Arnold would say, "I'll go talk to him."

BERRY: Did you compile a series of biographies?

SHARP: Goodness yes, we made large files on many candidates. They have all been destroyed now, except for one or two crucial files, which rest in the archives in Millikan Library under a sealed set-up. It is interesting to note that the JPL directorship problem required more work over a longer time and was harder than finding a president for Caltech.

BERRY: Was it the same committee?

SHARP: No, not the same committee, but I was chairman of that committee, too. We had a small committee—two JPL people, three from the campus, and one trustee, Bill Zisch. That was a tough job. We used to deliver dossiers to Harold Brown [Caltech president 1969-1977] about candidates. We would try to find out everything we could about the person. I remember the first one we ever delivered to Harold. He looked at it and said, "Boy, I'd like to see the dossier you guys have on me." I said, "Well, Harold, that's one thing you can't see!" [Laughter]

We developed very extensive files for both projects. The presidential committee was a good committee. As a matter of fact, it got to be a little funny. We met so often, usually at my house, that some nights when we had no meeting, I'd be sitting at home and the telephone would ring. It would be the wife of some fellow who was on the committee saying, "Have you got my husband?" I'd say, "No." She'd say, "Well, he hasn't been home for supper; I thought you probably had him." Occasionally people would show up at my house when there wasn't any meeting scheduled there. They came to my home automatically instead of going to their own home. They got so in the habit of assembling that they said, "Well, it's Monday night, we must be meeting," and off they'd go. On the whole, I think it was a very educational experience for all of us.

One of the most unusual aspects of the whole procedure was bringing Harold Brown to the campus before he was anointed as an acceptable candidate. Tom Watson was the one who had first spotted Harold and recognized his potential. Brown was secretary of the air force at that time. That was the era of student disruptions. You couldn't have brought the secretary of the air force onto the campus of most educational institutions then without having a confrontation. You could do it here, but even then it was a sensitive issue. We simply said to the trustees that Brown could not be a viable candidate unless he came and exposed himself to the community so we could see what kind of person he was. Harold came out and spent the better part of three days,

meeting with small groups of the faculty and a fair-sized group of students in head-to-head sessions. Our people treated him pretty roughly. They just bored in on him.

BERRY: It wasn't a cocktail thing?

SHARP: Oh no, there was nothing easy, casual, or social about this. They asked him the meanest, nastiest questions they could think of. He'd go from one to another of these sessions without a break. He's a very durable guy. I didn't sit in on all of these sessions, but I was in on some of them, and of course I was stewarding the whole thing. It was a Saturday afternoon when he left here, and he and I ended up back here in this office for an hour or so before he left. Colene [Mrs. Harold Brown] was to pick him up down on the street in an automobile. We chatted, and finally after I had taken him down and Colene had picked him up, I came back to my office. I was completely worn out. I sat down and said, "Well, there's a real rugged guy." He never lost his cool once during the whole time that people really badgered him. He's very quick on his feet and was highly intelligent in his replies.

BERRY: What type of things—without getting into that secret file—would they ask him?

SHARP: They asked him about his attitude toward American education. Why was he interested in being president of Caltech? They couldn't see anything in his background that would suggest to them that he would have a genuine interest in running Caltech. "What would you do if you came to Caltech? What concepts of education do you have? How do you think you would change things around here? Are you going to come in here and line us up like a bunch of air force cadets?"

BERRY: Weren't there two other candidates who came?

SHARP: We had a couple of others. Yes.

BERRY: The head of NASA was one?

SHARP: Yes, Jim Fletcher. Fletcher, Brown, and we also had more limited exercises with a couple of other people, partly to give broader exposure and not to make too much of a point of just Brown and Fletcher. Why should we do this just to Brown and not do it to others?

BERRY: He was the most favorably thought of at that time?

SHARP: Not at that time, but it finally came down to the point where Fletcher and Brown were the two most favored candidates, and we had had them both here. These on-campus visitations were extremely valuable. I can't remember all of the people who came here, but we felt that the visitation procedure was a good thing. I remember talking to George Hammond, who was on our committee and was a very good member. I said to him, "What else should we do with Brown while he is here? What other groups should he see?" George said that one of the smartest things we could do was get about ten of our most critical and youngest faculty members to eat dinner with Brown. We did this, and it worked out well. I had each one of these young people write me a critique. These were the feisty young guys around the institute. I don't know if they were necessarily mean to him, but they were really aggressive and tough in the written critiques. I remember that particular evening well. After a long day, after he went to dinner with these guys, Brown ended up at my house about eight o'clock in the evening with the whole faculty committee. So he got a real thorough going-over here.

In the final stretch, it was either Fletcher or Brown. Remember that the ultimate decision was in the hands of the trustees, but our committee came to favor Brown over Fletcher. Not by a wide margin—both were good. I think we would have been happy with either. The trustees favored Brown, too. The trustees didn't tell us that Brown was number one: We told the trustees that in our opinion Brown was the better one, which happened to coincide with their own opinion.

In something like this, people on the committee develop a close identification with each other. Like being in the front-line trenches together. It took a lot of time and a lot of work. I can remember Arnold Beckman was in the hospital at one time during these goings-on. I was home one evening when the phone rang. It was Arnold; he'd entered the hospital just that day for an operation. I said, "My gosh, Arnold, you are supposed to be sick." He said, "Yes, and I've got to get out of this place, the pills I'm taking cost a dollar apiece." [Laughter] He wanted

to talk about this presidential business. I had spent a lot of time going to Fullerton, where his offices were at that time, to talk with Arnold. We maintained close contact. Tom Watson was very helpful. One reason IBM is a great institution is that Tom Watson keeps scouting talent all the time. Wherever he goes, he's always looking for good people, people with administrative ability. He marks them, and at the right time he will try to get them into the IBM organization. Brown was obviously a person he'd had his eyes on for some time. He'd worked for Brown on an air force committee and had been impressed with Harold's flexibility, pragmatism, and the fact that he just wasn't a hard-nosed bureaucrat, he was a smart guy. Watson was very effective, and Arnold Beckman certainly worked devotedly on the presidential problem. We had Harold Brown for eight years, and I think, considering the climate of the academic community at the time Brown was here, he was a good person to have. He left this place shipshape. A lot of academic institutions didn't come through that period in very good shape.

So much for the presidential committee. Fred Anson and his associates did the most recent job for Murph [Marvin L.] Goldberger [Caltech president 1978-1987].

BERRY: They used the same procedure?

SHARP: They did some things differently. They worked under a somewhat more liberal charter than we had had, and they had the benefit of our experiences. I didn't write a report of procedures, but Anson had complete access to all the material I had at the time, and we talked together frequently. I refused to be on the committee—I had been on too many committees at Caltech having to do with the selection of administrative people—director of JPL, president, and others on down along the line.

BERRY: You had more experience than anyone.

SHARP: OK, but no one person should have that much input as to who should be chosen to lead the institute. So I said, "Look, I'll be glad to respond to any requests that you make of me, but I will not be on the committee."

Now as to other matters, the Grand Canyon trip was one of the things that was a little unusual on your list of items. The endeavor was basically a brainchild of Lee Silver and Gene

Shoemaker. You see, our geology department had been the only division on the campus that didn't have an endowed named professorship. Shoemaker and Silver were grounded on an airplane in Texas somewhere—Fort Worth, Dallas, or one of the big airports there—and they got to talking. I think in the back of Lee's mind this sort of idea had lain for quite some time. They came up with the wild idea of running a geologically guided boat trip through the Grand Canyon and charging enormous sums to people for doing it. The aim was to raise money for an endowed professorship in geology. People would basically be making a charitable gift to the institute for that purpose. This would be our way of saying thank you. That's about what it really was.

BERRY: Wasn't it something like $50,000?

SHARP: Yes indeed, $50,000 for an individual and $75,000 for a couple. When Shoemaker and Silver came back, they tried it out on Barclay [Kamb], and then Barclay called me over and explained the situation. You will recall that I had been through the canyon way back in 1937 on a Caltech-Carnegie boat trip. That was probably about the last of the real honest-to-goodness exploration trips. There were no tourist trips at that time. I knew a bit about boating in the canyon. I thought the proposal was great, so I sat down and wrote a brochure about it, and Barclay presented it to the administration—particularly to Harold Brown and Bill Corcoran [vice president for institute relations]. To the everlasting credit of the Caltech administration, they didn't say, "Crazy, forget it, and go on back and tend to your rocks and don't bother us with wild ideas like this!" They gave us their blessing and we got the program rolling. The first year [1976], fortunately, we got a lot of good support from the trustees. The trustees were crucial. As a matter of fact, Stan [Stanton] Avery and his wife Ernie Avery were among the first to sign up for the trip. We also had a lot of constructive help from the development office. Gene Wilson, Sue Walker, and many other people over there helped us a great deal, as did Bill Corcoran. But crucial was the trustee reaction, and Stan Avery, who was chairman of the trustees, said, "We'll go." He leaned on other people, some trustees and some of the Caltech Associates, and so we got off to a running start the first year. There were some trustees who liked the idea so much that they contributed even though they couldn't go, for some reason. Earle Jorgensen would be an example—for two years running, as a matter of fact. Dean McGee couldn't go, but sent his daughter and son-in-law. So we had some *in absentia* supporters. Cecil Green, who may be an

Associate but not a trustee, gave us a $50,000 challenge grant, at Barclay Kamb's urging. We were shooting for a million dollars. The first year we got half a million dollars, but we had to run the trip on two more successive years to complete the fund. The last time, Stan Rawn picked up the remainder of the tab, so we made it a family trip—the Rawn family and friends of his. Jim Glanville, a trustee, was unusual in his support of the project. He went himself, with one of his boys, the first year; the next year he sent his wife and another boy; and the third year he sent another one of his boys. So we had a lot of support from the trustees. It was a most unusual idea, and you've got to credit Silver and Shoemaker. One of our trustees [Jim Robinson] said, "I was going to go to Europe this summer, but your proposal is the most unusual solicitation for funds I've ever had, so I'm going to go with you." He and his wife went and had a marvelous time.

BERRY: You went on the trip, and then Shoemaker?

SHARP: There were four of us as geology guides on the trip: Shoemaker, Silver, Kamb, and myself. I missed the intermediate trip. I had a very bad cold just at the wrong time. I did much of the organization for these trips.

BERRY: Did you camp out at night?

SHARP: Oh yes, we camped out at night along the river. A lot of these people had never really camped out before. But we made a deluxe operation out of it—the first trip particularly. We had some very sophisticated ladies, and the fact that they could come back and say to other ladies, "It was great! Go! Don't worry about things, it's comfortable, fun, and exciting!" was helpful to us in getting others to go. Stan Avery must have been sixty-nine when he went, so we had some older people. Stan was in good shape and had a great time. We didn't let age be a barrier if we could possibly help it.

BERRY: Did you start out near the confluence of the Green and the Colorado?

SHARP: No, you'd have to come down through Lake Powell to do that, and that would be a slow

journey. We started right below the Glen Canyon Dam at Lee's Ferry, which is the classical shove-off point. We were on the river only for a week, and traveled about 190 miles. We went from Lee's Ferry down to Lava Falls and then were evacuated by helicopter. It's a well-set-up operation. You can do this commercially. We just hired a commercial outfit, Western Rivers Expedition, to handle our party, but we deluxed the operation beyond what they would normally provide. We said, "We want your most deluxe operation to begin with and we want a couple of extra attendants, to make things easier." Then we added a lot of things ourselves.

BERRY: A lot of descriptions on the way?

SHARP: Oh yes, all the way down. We started initially somewhat formally, but eventually shifted more into a one-on-one sort of presentation thing. We called it a trip of a lifetime. People came to me after the end of the first trip, either just before we were evacuated from the canyon or after we got back to Las Vegas, and said, "You were right, it was a trip of a lifetime. No question about it." Then the last trip, of course, we had Murph Goldberger aboard. There was one place, I think it was the last night in the canyon, Murph stood up on the sandbar and said, "This is the greatest trip I've ever had in my whole life." He was supposed to have gone to Japan or perhaps somewhere else in the Orient, but we got him into the canyon. It was one of those happy ideas that worked out just fine.

A number of other universities are now trying to figure out how they could do something similar to raise funds for professorships. I had a letter just a couple of weeks ago from a professor at Princeton saying, "We've been trying to figure out how we could do something like what you guys did." He wanted the background information. The University of Miami, I gather, has approached Lee Silver with the same query, and so it goes. I wish we could do something similar again sometime. Unfortunately, the Grand Canyon is hard to top. It was a natural; it's a thrilling experience.

BERRY: You haven't talked much about your space adventures.

SHARP: I'll talk about myself in connection with the exploration of Mars, but the planetary exploration operation has been important for our whole division. We have already talked a little

about that. Mariner I and II went to Venus; Mariner III and IV were to go to Mars. Bob Leighton, who had done a lot of planetary photography using the Mt. Wilson telescopes, rather reluctantly, I think, put together a television experiment for Mariner III and Mariner IV. Mariner III crashed on takeoff. There was something wrong with the shield, so it just never got into orbit. It never went to Mars, but Mariner IV did. We always used to launch two spacecrafts in those days, and it's a good thing we did. When the pictures started coming in—we only got about twelve or fourteen pictures all told—they came in very slowly. It took eight and a half hours or something like that just to get one picture back. Furthermore, they didn't look like anything to begin with, because JPL had not yet developed its processing and enhancement techniques to any degree. Anyhow, as it eventually turned out, there were craters on Mars, and JPL learned how to process the pictures to reveal details of the terrain. Eventually six or seven of the Mariner IV TV pictures of Mars turned out to be pretty good.

Mariner VI and Mariner VII, still fly-by operations but with a much expanded program, came next, and eventually Mariner IX gave us a continuing orbit. The earlier flights were fly-bys. In effect, you just lean out the window and take a bunch of pictures as you go by. But when you start orbiting, that's a different ball game, and that was Mariner IX. By the time Mariner IX was finished, Leighton was exhausted. He had had it and I'd also had about all I wanted. [Bruce] Murray continued on into Mariner X, which went to Mercury, but none of us was in on the spectacular Viking operations, although we watched from the sidelines with great interest. These space missions are very demanding things: The pressure on experimenters is heavy.

BERRY: A good part of it got into the news.

SHARP: Oh yes, the media just drive you absolutely nuts. I can remember a television personality grabbing me one day up there at JPL. I think it was during the Mariner VI and VII operation. He rushed me to a microphone in front of a TV camera, and said, "Now, tell us what this means to the guy drinking beer down in Torrance." So there you are, suddenly, bingo, you get a question like that.

We published a number of scientific papers on the Martian flights. It was an exciting scientific adventure, and we got a lot of useful information. Your interest certainly gets whetted.

As a matter of fact, I have at home in Santa Barbara right now some integrated and enlarged mosaics taken by the Viking landers that one of my associates sent to me. We're looking at them carefully to see what kind of geological interpretations we can make beyond those already made by the regular Viking crew. The regular crew has, properly so, priority to these pictures at the beginning, but that privilege has a time limit on it. They have to work fast. Subsequently other people can have access to the data and see if they can wring anything else out of it.

BERRY: You're doing that now?

SHARP: Yes, I'm working a little bit on it now. The work we've been doing in Hawaii is a related effort, designed to determine patterns and parameters that might be recorded on remote sensing instruments orbiting other planetary surfaces. We use Hawaii as a model for other planetary surfaces, because of the recent volcanic terrains. So I'm not out of the space game. But I never did any work on the moon. Many people in our division, however, did a great deal of lunar research: Shoemaker, Silver, Epstein, Wasserburg, [Dorothy S.] Woolum, [Donald S.] Burnett, [Arden] Albee and others have been involved in the lunar program.

BERRY: Have we discussed the field trips for the geology staff people?

SHARP: As I expressed to you earlier, it's long been my feeling that the non-academic people at Caltech don't get the recognition they deserve. Any educational institution is a hierarchical operation. The faculty considers itself to be pretty well up toward the top of this hierarchy, while the non-academic staff feels it is at or near the bottom.

Begin Tape 3, Side 2

SHARP: We were starting to talk about relationships with non-academics. George Beadle was a master at this. I was walking back from the Athenaeum one day with Beadle—this was when the carpenter shop was where the new student houses have been built. The old physical plant buildings used to be there. George said, "I've got to peel off here. I've got to go thank one of the carpenters for something he did for us." So off he goes into the carpenter shop. I was pretty impressed—the chairman of the biology department stopping off at the carpenter shop to thank a

guy for something he'd done. George was very good at this, and I tried to take a leaf from his book. I was running some research in Alaska very shortly after I got here, and there was always a big rush getting equipment and supplies shipped at the beginning of the season—as well as buying them in the first place and getting them up to Seattle. Kermit Jacobson used to help me a great deal; he was purchasing agent at the time. I remember having gotten onto the upper Seward Glacier when I was caught in a blizzard. There I was lying on my belly in a sleeping bag in a little tent out in the middle of this huge ice field—oh, it was a grand place! Mt. Logan, almost 20,000 feet, just a few miles to the northwest. Since it was a miserable day, I was writing letters. So I wrote Kermit a little note saying, "Jake, gee if it weren't for you, we just wouldn't have been able to get this stuff up here." It wasn't more than a few lines, and I told him there was a terrible blizzard going on and that he might never get this letter—but he did get it.

BERRY: Were you by yourself?

SHARP: I was by myself at that time in the camp. The blizzard made it impossible for the expedition plane to bring anybody else in. There were some people in a little quonset hut about three or four miles from me, but our research camp was down on the ice field, and I was by myself. I could have gotten up and walked to the hut, if I had wanted to. Anyway, Jake never forgot that note. He became a warm friend, just because I wrote him a simple little note. I was very impressed with that. So having learned that lesson, I try to go out of my way to express appreciation to non-academic people who help us in various different ways.

In our own division, I always felt that the non-academic people—the lab technicians and the secretaries—must have felt left out when they saw our faculty and students going on field trips. We do a lot of field tripping. The faculty and students come back all excited and all enthused about what they've seen and done on such trips, but the technicians and secretaries never got to go. It must have been tantalizing to them. "What are those people doing out there? They have such a marvelous time." So we started a non-academic field trip. Initially, I ran very simple one-day trips, such as over the San Gabriel Mountains and back by way of Mint Canyon. Eventually, we ran a two-day trip. The first two-day trip we ever ran was up to the Owens Valley and back by way of Panamint Valley. We stayed overnight at and near Lone Pine. Some of us wanted to camp out, but most wanted to stay in motels. So we had to find a place for those

who wanted to camp out, while the others could stay in a motel. We had a bus that had to take those of us who were going to sleep out in the Alabama Hills, after we had eaten supper in town, out to the place where we were going to camp. I think this was in May. So after supper we went to the motel and dropped all the people staying there, and then I climbed back on the bus with the people who were going to go camp out in the hills. What was left on the bus, besides me? Seven girls, nobody else! So I had seven girls camping with me in the Alabama foothills that night. We had a marvelous time. I had brought some firewood and some popcorn, and we really enjoyed a great evening in the Alabama Hills. The aim in making the trips is mostly to express appreciation to these people. The last time we went was to Death Valley. It was a longer trip, three days.

BERRY: How many have you had, do you remember?

SHARP: I think maybe the last one was the seventh. We had Phyllis Brewster on the last trip; she wrote an article for the campus non-academic publication, "On Campus." I'm now beginning to run out of places I can take them. But we may run one or two more. They're very enjoyable and relaxed.

I notice one of your questions is, How did I learn so much about the geology of Southern California? There are people who know more about the geology of Southern California than I do. One just accumulates information slowly over the years. Particularly if you prepare guidebooks, then you pick up lots of information, necessarily. You've got to remember that I was an undergraduate student at Caltech, and John Buwalda did a lot of field tripping with us. Buwalda was very good at this sort of thing himself, so I got indoctrinated way back in the early thirties. Then I've been back here since 1947, so I've been more than thirty years in Southern California as a professional, so to speak. You just accumulate the knowledge. There are a lot of places I don't know in Southern California. I cover my ignorance; if you listen to me talk and you think I know a lot, well, I'm just not telling you what I don't know.

BERRY: There are two of those field guides, aren't there?

SHARP: Yes, there are two. The idea to write them came from a publisher in Iowa, William C.

Brown. A young fellow wandered in here one day. He sat down and started talking about these field guides that Brown wanted to develop. I had always thought it was a good idea, and I said, "Gee, people have been trying to get me to write books for years and years, and I've always resisted, but that's attractive to me and I will consider it." The first one, which you reviewed so nicely for me, I did in a very short period of time, about eight months. Much of the information needed for a guidebook is available in the published geological literature. A lot of it I already knew, but a lot of it could also be looked up. So if you know geological literature and how to handle it, anybody could do a guide. The only difference between my field guides and those done by other people is that mine are written expressly for the layman, not for the professional, and secondly, I've tried very hard to make them focus on what you see while traveling. So many field guides say, "Drive to Victorville and then at Victorville you'll see so-and-so. Now drive to Barstow and you'll see so-and-so." In other words, they take you from spot to spot. I tried to give a continuous running account of features between Victorville and Barstow, which is difficult but worth the effort in my opinion.

BERRY: Are you planning another one?

SHARP: No, not at the moment. The last one, on the south coastal region, was a difficult chore. I found it very hard to do a guidebook for freeways. Not only was it hard to do but it's hard to use on freeways. But that's what we have. I felt if we were going to try to reach the lay public, we had to stick to freeways; they're not going to turn off and follow some back road just because you've written a geological guide about it. I won't say I'll never do another guidebook, but I'm not planning another one at present. If I were to do another book for laymen on Southern California geology, I would do something like this: "Vignette of Southern California Geology." In it I would write a little story about Cajon Pass, for example, through which thousands of people travel every day. Why is it there? What were the geological influences that brought it about? Things that you can see as you drive through Cajon Pass. Or a story about the San Gabriel Valley, a little like the one we wrote for the Pasadena *Star- News*, which the sons of guns never used. I wish they would send back my photograph. They've lost it, obviously.

BERRY: You mean that big beautiful photograph that you had?

SHARP: Yes. They claimed that they never published the article because they lost the photograph.

People obviously liked the Southern California guidebook. They come to me and say, "Why don't you tell us more about the geology right here in Pasadena?" To do that, you have to write a little pamphlet about Pasadena. Nobody will publish a little pamphlet just about Pasadena—too limited in appeal. But if you included the geology of the San Gabriel Valley as one of the vignettes in a book that had twenty vignettes in it, a publisher might be interested, and he could take individual chapters or vignettes and issue them as separate pamphlets if he wanted to. [W. H.] Freeman Company has been talking to me about such a plan a little bit, but at the moment I have no firm plans to do such a book.

BERRY: Are you writing?

SHARP: I'm cleaning up, as I say.

BERRY: Scientific papers and things like that?

SHARP: Oh, yes, things that have backlogged. I want to clean out the accumulated trash, so to speak, before I start something else. It's like cleaning out my office here. I've been giving away most of my library. I've been throwing away lantern slides and specimens. It's a very traumatic experience, because you save things for thirty years which are really pretty precious to you, and then comes that moment of truth when you just have to heave them out.

Now that brings us to the matter of retirement plans and activities. I've been concerned for some time about the problem of older people in educational institutions. In most educational institutions, the students ought to be somewhat older and the faculty ought to be a lot younger. Our current systems don't do either of these things very well. I instituted my own plan at about age sixty-two; I went to three-quarters time. I said to the institute administration, "I want to go to three-quarters time." They said, "All right." Pretty soon I found I was working full-time for three-quarters pay. So when I got to be sixty-five I went to half-time and moved out of town. I still taught a full schedule, of course, but they were all field courses, so I didn't have to be on the

campus all the time. That worked better, and now I'm fully retired and supposed to be living in Santa Barbara. I still illegally teach a course each spring semester. As an emeritus professor, I cannot carry sole responsibility for a course. I can teach in somebody else's course. There is a traveling field-trip course in the geology of the Southwest which I have run for many years. It's listed in the catalog under Clarence Allen's and Kerry Sieh's names. It's a pass-fail course. I've told the administration all about this arrangement. I teach the course, make up the grade list at the end of the quarter, and I'll take it to Allen or Sieh and say, "Here, sign this, please." So, I continue to teach a course, and I'll probably teach in some other courses as requested from time to time. I wanted to phase out gradually and I also wanted to make room for young people. It's been advantageous for Barclay Kamb, as division chairman, to be able to say, "Look, Bob Sharp has already gone onto three-quarters time, and in another couple of years he's going to be down to half-time and then he's going to be gone. I've got to start looking for young guys." Which is what he needs to do in order to maintain vitality within the division. The administration says, "Yes, we can see that. OK, start looking for young people."

Young people are very important in this operation. In order to make room for them, we need to phase out the older people gently, gradually, and gracefully. I found it not traumatic to do it that way. It's a little easier now, with the division chairmen rotating more rapidly than they did before. At the time I quit, a division chairman usually stayed in the job until he retired. Hallett Smith and Fred Lindvall had been in over twenty years, and I was in for fifteen. To have ex-division chairmen around who have been in the saddle that long can be a pain in the neck. But I was lucky, in that when I moved out of the chairmanship there were a whole lot of other things I could do on campus that had nothing to do with our geology division, like the presidential search committee. Then after Harold Brown got here, I spent much time working closely with Brown. Before he came here, I said, "I'd like to see you come to Caltech. I will help you in any way that I can if you come." He cashed that check, and I spent a lot of time working ad hoc for Brown. Which was a good thing; it took me out of divisional politics and put me into campus-wide problems. I had no constituency, I wasn't suspected of trying to promote something for the benefit of geology. It worked well.

BERRY: Was it sort of helping him get acquainted with Caltech and what went on here?

SHARP: Oh, yes. But Brown learns very, very quickly. People have unkindly said that I told him how to run Caltech. That is not true. He didn't have to be told.

I still continue to do things at Caltech and will continue to do things at Caltech and will continue to participate upon request. It's a satisfying relationship, and I will do this as long as people at Caltech feel there is some benefit to the community from such activities. I'm basically a simple-minded guy. I like to go skiing, I like to go fishing, I would like to have more time to grow vegetables in a garden, some fruit trees, work at home, work with my hands, build things, and fix my car. I want to be a little more independent of other people for things that I ought to be able to do myself. Gradually, I'm doing that. My most satisfying retirement activity, which I started a couple of summers ago, is building a small cabin in Montana, just off the northeast corner of Yellowstone Park. It's not fully completed, but it's fully functional, it's dry, it's warm, it's comfortable, and I spent most of the summer up there last year. I don't think I ever had more fun in my whole life than I've had with my cabin.

BERRY: You didn't do this by yourself?

SHARP: No, I had help. In building the main structure, I had help from a wonderful guy, Don Ellis of Livingston, Montana. It was a lot of fun. The cabin itself is nothing unusual, but the country around is just beyond description. I'm really about 100 miles from any good supply center. The logistics are complicated. I was lucky to find a person like Don Ellis, who had lived in Cook City, which is a little old mining town about two or three miles from my cabin. He and his son basically built the main structure, although I made the foundation (poles and beams) up to the joists for the floor, which is where they started. I worked like a dog on the project. The cabin is a simple little structure, but to me it's satisfying, and I'm proud of it. I really have a marvelous time up there. The country around is beautiful. It's one of the best trout fishing places in the whole country, and fly fishing is one of my passions. Some bad weather occurs up there, of course, but I've got my cabin, with a woodstove and essential services.

BERRY: You can drive to it, apparently.

SHARP: About two days from here. I can drive right to it. Yes. It's only about 300 feet off a

paved road, though you can't see the cabin from the road. So that's been one of my retirement activities. It's been very satisfying and has brought me much joy.

BERRY: Do you find time hanging heavy on your hands?

SHARP: No. I've got lots to do. I do have a home in Santa Barbara, and there is always a list of chores to be done there, whenever I'm home. I did a lot of the initial work on our place in Santa Barbara, and I still do a lot of maintenance and continue construction there. We have an acre of ground, not all of which is cultivable, but I've got some fruit trees and a vegetable garden which take time and attention. I have all sorts of projects, like surf fishing, that I can't get to because I don't have enough time. When I come to Pasadena, there are a lot of things to do here, and a lot of people stacked up to see me. So I have no problem in keeping busy.

I jotted down here a sort of summation. This is very personal, in some ways. Luck plays a very large role in what happens to us. I was lucky in simply being at the right places in many instances, at the right time. I was fortunate in having a grandfather who made it possible for me to get a really good education. I don't know anything that has made more difference to me than the education I was able to get at Caltech and Harvard. If I hadn't come to Caltech, if I had just quit at the end of high school, I could be like hundreds or thousands of other guys who are in some little town working in a drugstore, a jewelry store, or a service station. Which is not to say that's bad, but my life has certainly been, to me, a lot more exciting and interesting and satisfying because I had wider opportunities afforded by a good education. It's been a satisfying professional career. Caltech has been a fortunate experience for me for reasons that I have already told you—the opportunities to do things around here, sometimes almost over your own dead body. Like when Bruce Murray drafted me, pretty reluctantly, into this seemingly far-fetched project on Mars with him and Leighton, which then evolved into what was ultimately a very satisfying activity in the space program. Often you get dragged into something, sometimes unwillingly, that turns out to be rather fortunate for you.

Let's see, the one thing I haven't talked about which you had on your list were various honors that have come my way. Let me just run down through some of those you touched on to get them into perspective. For example, this business of the *Life* magazine award as one of the ten best teachers in the United States. Well, that's a very capricious selection—to pick out only

ten guys in the whole country and say these are the ten best teachers. You have to have certain basic qualifications, perhaps, but after that other things control the selection. *Life* wanted somebody from a small technical school, because they had people from Harvard University and other big and liberal arts places. Still, they wanted a prestigious school, and Caltech would qualify. Take the *Sports Illustrated* award, the so-called 25th anniversary All-American. Again, that's basically a publicity stunt on the part of *Sports Illustrated*.

One of the two most meaningful honors, from my standpoint, was the naming of the endowed professorship for me here at Caltech. I was utterly flabbergasted. I worked very hard on that project and was more than a little embarrassed that the professorship ended up bearing my name. That was the Grand Canyon operation.

The other honor, which is a highly specialized thing, is the Penrose Medal of the Geological Society of America. That's a very humbling sort of award, because if you look at the names of former Penrose medalists, they have been the great names in geology in this country as well as abroad. You have to wonder if you belong on that list. Again, I have a feeling that the choice is a capricious thing. It just happened that at a particular time under certain particular circumstances, my name got onto a list and finally came out at the top.

BERRY: You're an amazingly modest man.

SHARP: Well, you know, there is an interesting guy down at Scripps Institute, Harmon Craig—he's a wild man—but he said something one day that I like very much: "Medals are for soldiers." That's not bad. The Penrose medal is actually a very nice thing, though it's so specialized that most people would say, "Oh, a Penrose medal, what's that?" Probably, in the United States, of all the medals a geologist is eligible to win, the Penrose is close to tops. The applied geologists, petroleum geologists for example, have a medal which means a great deal to them, and the Penrose is the academic equivalent. The National Medal of Science would top it, but that goes to a scientist in any field.

BERRY: What about the National Academy election?

SHARP: As to the National Academy—I'm going to give it to you straight. My humble opinion

is that I was elected to the National Academy of Sciences not because I did unusual geological research or anything like that, which is supposed to be the basis, but because the Caltech geology department was recognized as a real gung-ho operation. That's why I was elected to the National Academy. People gave me, probably incorrectly, more credit than I deserved for the fact that we have a great geology department at Caltech. I probably got the Penrose medal for the same reason—recognition of the Caltech geology division.

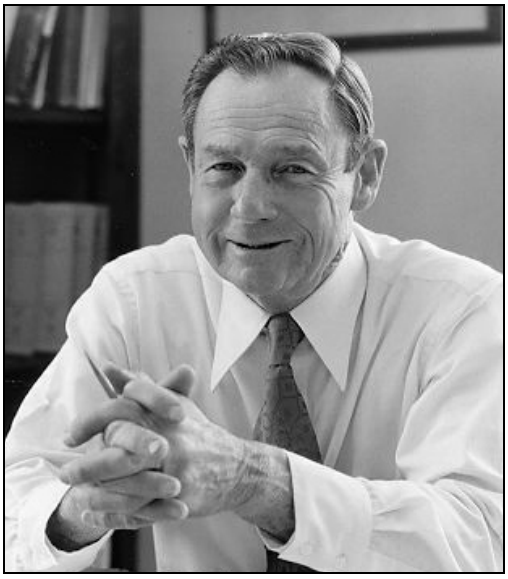
BERRY: You had a lot to do with it.

SHARP: I just happened to be in the driver's seat at the time the division was developing and emerging. I have said to people around here, "You know, I got a medal, but it belongs to you as much as it belongs to me." Incidentally, that medal, which is three ounces of pure gold, is now worth about $1,800. I socked it away in a safe deposit vault. But I'm not ready to melt the medal down. I've been wondering what I should do with it. I thought maybe it should be presented to the division, because I don't think it means that much to anyone in my family. I think the medal really belongs to the division.

We have been one of the top geology/geoscience operations in the country, and we still are. The problem facing the department now is that you have to work doubly hard to maintain such a status. There are two things you can do besides work hard: You get good young people, and you need to have a sense of mission—that is, you have focus. That goes back to what I talked about a little earlier: This geology department became different from many others when John Buwalda identified seismology as a field in which to work. Our ventures into geochemistry and into planetary science, all of which have been forefront activities with a lot of vitality, seemed to give us a vital mission-style orientation. The big problem for our division right now is, What next? Barclay Kamb, I think with good judgment and astuteness, has identified the field of natural resources as an area in which we could become an outstanding contributor. Over the next ten to twenty years in this country, natural resources are going to be a major concern—basic research having to do with the discovery and use of our natural resources is going to be required.

BERRY: Including energy....

SHARP: Energy and materials. A lot of people are so overly concerned with energy that they are forgetting that we are also running low on raw materials. Our division needs to look on down the line, in anticipation of national economic and social needs. It's very important that we have a sense of mission. That's what has maintained the vitality and integrity of this division. We have a lot of very volatile guys around here, and many people wonder how it's been possible to hold them together in a coherent group. They're very critical, very feisty, very itchy, and very ambitious. I think the answer is that we were lucky in having focuses of activity like geochemistry and planetary sciences. Now we aim to find something new that they will find exciting and fun to work in. The future orientation of the division, plus young people, are essential ingredients for the future. We've got to get young people. I think I mentioned to you that some time ago, in one year we hired three young assistant professors—Gerry Wasserburg, Lee Silver, and Clarence Allen—all of whom are now members of the National Academy of Sciences and respected among national and international scientists. You can't be that lucky all the time, but we need to do something similar again.



ROBERT P. SHARP (II)
(1911-2004)

INTERVIEWED BY
SHELLEY ERWIN

May 28 and October 12, 1998

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Abstract
This interview in two sessions in 1998 with Robert P. Sharp, Sharp Professor of Geology emeritus, begins with an account of his institution in 1984 of student field trips to Hawaii to study volcanism up close (Project Pahoehoe), thanks to the financial support of H. Dudley Wright. Recollections of alumni geology field trips that Sharp conducted over the previous two decades to Hawaii, Alaska, Yellowstone, Utah, Death Valley, Pennsylvania, New England, and Iceland, to bring alumni closer to Caltech. Discussion of the field course he has taught at Caltech since his retirement in 1979 (Geology of the Southwestern United States). Discussion of the evolution of the Division of Geological and Planetary Sciences at Caltech: early influence of J. C. Merriam on R. A. Millikan; evaluation of J. P. Buwalda's long chairmanship of the division; recollections of Beno Gutenberg; recollections of Chester Stock. Stock's work in vertebrate paleontology; the decision to phase out vertebrate paleontology after Stock's death in 1950; sale in 1957 of the fossil collections to the Los Angeles Natural History Museum. Recollections of the contributions of Stock's colleagues Eustace Furlong and William Otto. The interview concludes with a discussion of the new field of

geobiology and the interest in ancient DNA and possible role of the division in such investigations.

Administrative information

Access

The interview is unrestricted.

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Robert Sharp at Mesa Verde, Colorado, September 25, 1998. Caltech Archives.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH ROBERT P. SHARP (II)

BY SHELLEY ERWIN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Robert P. Sharp (II) **By Shelley Erwin**

Pasadena, California

Session 1	May 28, 1998
Session 2	October 12, 1998

Begin Tape 1, Side 1

SHARP: This has to do with Project Pahoehoe [in the mid 1980s]—student field trips on the Big Island of Hawaii. I had been working in Hawaii with people at the Hawaiian Volcano Observatory and with two Caltech planetary scientists whose work in Hawaii required geological information. Both had me come to Hawaii several times.

ERWIN: Who were those people?

SHARP: One was Dan Dzurisin and the other was Mike Malin, who has recently been the principal investigator on the orbiter photographs of Mars; he and assistants made the camera that has taken the best pictures ever of parts of Mars. Both Dzurisin and Malin were trained as physicists and came into Caltech's geology division as planetary scientists. Malin knew quite a bit of geology; Dzurisin didn't know as much. I spent a lot of time on Hawaii with them. Dzurisin and I also began leading alumni trips in Hawaii. Every time I came back home I kept saying to myself, "We're not doing this right; we ought to get our students over to Hawaii." It's nice to do things for the alumni, but that's the past and we need to look to the future. On one occasion when I came back from Hawaii I sat down and wrote up a little proposal of what I would like to do in taking a group of students to Hawaii. I took it to Peter Wyllie, who was our division chairman at that time. Wyllie said, "OK, as long as it doesn't cost the division money." I said, "It's not going to cost the division anything. I will raise the funds for this, but I do need some secretarial help from the division." And Wyllie said, "Fine."

So I launched it. I couldn't handle more than, say, fifteen students very well, and a maximum of twenty; consequently I had to have a basis for making a selection. I decided that the people who were finishing up and leaving Caltech within the current academic year would be the most eligible. We would take the graduating seniors in the division who were leaving the institute in good standing. All the graduate students we considered were advanced PhD candidates, completing their theses that year. We aimed for a minimum of fourteen and a maximum of nineteen. We operated in modules of four, because our transportation on the island consisted of rented Aerostar vans with the backseats out to carry camping equipment and personal gear. Each van could thus seat four people, so we operated by units of four. Just one spare student was a headache to me.

ERWIN: Were these all geology majors?

SHARP: This was anybody in our geology division, even a double major or a radio astronomer. I got tripped up every now and then, because I would, for the senior students, ask the registrar's office to give me the list of senior geology majors in good standing. They sometimes listed somebody who was doing a double degree in chemistry and geology, and they'd list a student in chemistry every now and then. This was little—a minor thing.

That was the basis that I chose them on, and it worked out very well. In most years we'd come up with fifteen students. I often had trouble with a surplus of graduate students, because they thought they were going to get their theses done earlier than their advisors did. I always talked to the advisors, both the academic and the thesis advisors. Things got a little tight at times, but we managed to end up with fifteen to nineteen students each year.

I needed only about $8,000 the first year; it's now double that. The first year, I went to Elba Smith, who was in the development office at that time. I explained what I wanted to do. She said, "Well, I think I know some people who might be interested in supporting that activity." In a very short time she called me back and said, "OK, I've got you two people." Howard Smits, a Caltech alumnus, benefactor, PhD—he had actually been my freshman drawing instructor—and a chap named John Navas…. I think Navas may have been an MIT man; I don't think he was a Caltech alumnus. He was interested in benefiting students in one way or another. He had

set up a little award for good teaching at Caltech, and Tom Gold got the first one of those awards.

Anyway, Navas and Smits put up the money we needed the first year [1984]. It was that easy. After that, it began to get harder. I had said I would try to do the trips for one generation of students—that is, for four years. In following years I had to find other benefactors, though Smits helped me a second year. Navas maybe had died by that time; I don't recall. After four years I was eager to continue the trips, they were so successful. By scraping hard, I was able to raise the money for two more years.

About that time, a chap named H. Dudley Wright—I don't know what the H stands for; anyway, he was always known as Dudley—who had established a Feynman fellowship here in physics, contacted me. Wright had been alienated by one of our early development directors, although he was basically favorable to Caltech. He turned Dudley off, didn't handle him right. Dudley was a rough, hard-nosed businessman, but with a heart of gold.

ERWIN: How did he hook up with Caltech in the first place?

SHARP: He had operated in the Pasadena area with a small medical instrument company—Endevco, I think it was called. It had some shops on South Pasadena Avenue—it was quite successful. He was bought out by Becton Dickinson, a big pharmaceutical and instrument company. Apparently, as part of the sales agreement, he agreed not to start another company that would compete with the one he had sold them. That would be only natural. So he went off to Switzerland and established another company in a high-tech field that measured partial gas pressures in commercial fluids. He took up residence in Switzerland, but he maintained close relationships in the US, not so much with Caltech but as a member of the board of directors for the Harvey Mudd engineering college.

I was in Hawaii, on a Pahoehoe trip, when he phoned my home in Santa Barbara. He had been having meetings of the board of directors of his Switzerland company in various parts of the world. He said, "Would you run some field trips for the members of my board if I held a meeting in Hawaii?" And I said, "Yes, I'd be glad to do that, if you will meet in Hilo." And he said, "I'll meet anywhere you want." So we agreed we'd meet in Hilo. The board wasn't big—six to eight members. Richard Feynman had been on the board at one time; Dudley Wright had

great admiration for Richard Feynman. Anyway, board members were to have their wives along, a typical board arrangement. They'd meet in the morning to attend to their business, and in the afternoon they'd relax. In the afternoon I was to take them on field trips.

I had a Hawaiian buddy in Hilo, and in the morning he and I would take the ladies to the orchid farms. After lunch, we'd all go on geological field trips. Dudley said, "I want to pay you an honorarium." I said, "Dudley, I don't want an honorarium, but I'd appreciate a contribution to my Project Pahoehoe fund. I'll tell you about it over there." So when we were done on the last day, he said, "All right, now tell me about Pahoehoe." So I told him. He immediately said two things: "First, you should bring younger faculty members with you who can run these trips after you can't do them anymore." I'm already at that stage, and Lee [Leon] Silver and Jason Saleeby are now running them. Then he said, "Secondly, you should have an endowment. You shouldn't have to be raising funds annually. I'll help you get an endowment." What he did was make a proposal to Henry Mudd, who heads the Mudd Family Foundation. Harvey and Seeley Mudd were both dead by that time. Dudley was on the Harvey Mudd College Board of Trustees. He said, "Henry, you know that I have been interested in getting biology into the Harvey Mudd curriculum," and he named an amount of money that he would give to biology at Harvey Mudd if the Mudd Foundation would make a corresponding donation to Bob Sharp's Pahoehoe.

I thought Henry Mudd couldn't turn this down, but he did. Later I was sitting in my cabin in Montana wondering what to do next when a letter came from Dudley Wright. The letter said, "I have just set up the H. Dudley Wright Foundation, and it will support Project Pahoehoe for as long as the geology division and the institute want to run it."

ERWIN: So he really came through for you.

SHARP: He really came through. But he didn't give me an endowment. He just said, "I will support it annually." So each year I would write to Dudley and say, "Dudley, this year I need $12,000," or whatever it was. He would pen a check. Well, Dudley died. Now, I had two letters from him which stated that the H. Dudley Wright Foundation would support Project Pahoehoe for as long as we wanted to operate it. Fortunately, I knew the president of his company, Orbisphere Laboratories—a fellow named Ion Bals. He was a Yugoslavian. So I wrote to Ion sending copies of these letters. I said, "I don't know who heads the foundation board but I

assume you must be on it." I got a letter back from him saying that the board would honor Dudley's wishes. I simply wrote to him every year thereafter and asked for a check. The expenses had gotten up to about $15,000; the major expenses are airline travel, rental of vans, and fees. There was very little equipment that we had to buy. It's a bare-bones operation. We camp out. We cook our own meals. The kids get to swim just once. If you try to raise money to send students to Hawaii, people tend to be skeptical, thinking it's a vacation trip. I always was very firm that this was an intellectual operation, in the finest place in the whole world to study hotspot volcanism. There's no place better than Hawaii. Each student was to be our local resident authority on some topic; I'd give them a long list of topics and they'd elect one, and they'd make frequent oral presentations during the trip. It was not a vacation. The only time they got to swim was the next-to-the-last day of the trip, after we had taken off from Kilauea to touch base with the other volcanoes on the island. We spent one night on the Kona Coast at Hapuna Beach, a state park where we could camp. There is a good beach there, and they'd get to swim. We have to guard against the idea that Pahoehoe is a great vacation when it is actually a serious intellectual enterprise; Dudley understood that.

So things went along just fine on an annual basis. The '98 trip, this year, [I thought] would have been the last one. But I saved money—a little bit each year, and some alumni have contributed money, so altogether we had enough, after the last check from the Dudley Wright Foundation, to run for one more year. Lee Silver, who is now in charge, says that we have been careful enough so we can run it one more year, so we're going to run it again in 1999. We get some funds in from alumni. We'll probably get some funds from the division, but we can't count on them for major support. Ed Stolper says to me, "I'm not going to let it die. We'll find some way or other."

I am currently trying to raise a half-million dollars to endow Project Pahoehoe. I have just sent out five letters to what looked to me like my best candidates. I made a contribution myself. I have found that if you want people to do something, it helps if you do something yourself first. They are much more likely to say, "This guy is serious."

ERWIN: There's that old expression: "Put your money where your mouth is."

SHARP: That's right. So I've already, by making a pledge, put my money where my mouth is. I'm looking for people to pledge $50,000. I need ten all told, so I've sent out my letters. I was going to send them out back in January, but I came down with viral pneumonia, so I'm way out of phase in time. I've had only one reply to the five letters that have been sent out. It was from Nancy Glanville, who I knew was one of my best candidates. She was just taking off for three weeks in Russia when she got my letter. She wrote me the nicest little note saying, "I'm interested in helping you to continue Project Pahoehoe. I want to sleep on it." At the time, all I was asking for was pledges. I was highly skeptical about whether I could raise a half-million dollars. All I wanted was a pledge that can be paid off over a two- or three-year time interval.

ERWIN: Is she the wife of James Glanville?

SHARP: Jim Glanville was a major supporter of the raft trip, which I described [in the earlier interview, 1979-1980—Ed.]. We raised a million dollars by running raft trips through the Grand Canyon. Jim Glanville not only went but sent Nancy another year. He had four boys. He sent three of them on the trip. Rob, the fourth one, was too young. Have you ever met Nancy?

ERWIN: No.

SHARP: She's a delightful woman. Just delightful. Jim had a car accident driving in Houston. He died within two or three weeks after the accident. They had four boys: John, Charles, Thomas, and then Rob. John, the oldest, worked here at the institute for a while in the physical plant. He has subsequently been active in Caltech Associate affairs.

We are now very much at the crossroads on Project Pahoehoe. It's an eight-day trip, run during the spring break. We assemble the kids in Hawaii, normally on a Thursday, which is before the end of the exam period, but they're all able to come, being mature students. We don't travel weekends, because it's more expensive; we travel on a Thursday. The trip really gets under way Friday. They have to buy supplies; camping in Hawaii is not that easy. There are some county parks that have camping facilities, but often they are in miserable shape. The national park at Kilauea has a good campground with little cabin-type houses that you can rent. That's Namakani Paio. We stay there now about four nights. I won't tell you all of the places

we stay, but we use the county campgrounds extensively. The state also has parks where you can camp; an example is Hapuna Beach, which is on the Kona Coast about thirty miles up from Kailua. The shelters you can rent at Namakani Paio and Hapuna are welcome; it rains a lot in Hawaii. We end up on a Friday. Most of the students come home the same day, although some linger in the islands. And of course I'm always being asked, "Can I bring my wife?" "Can I bring my girlfriend, husband, boyfriend?" We encourage them to get together with members of their family before or after the Pahoehoe trip.

ERWIN: You stay on the one island, though, with your group?

SHARP: Yes, one island. Most of the time is at Kilauea on the Big Island, because that's where the current activity is. We've been lucky, in that after we started this trip, Pu'u O'o, which is a volcanic vent on the east rift of Kilauea—not in the central caldera—became active. It's now in its fifteenth year, the longest continuing activity historically recorded in Hawaii. So we've been able, three-quarters of the time, to get our students right up to the edge of red-hot lava, as close as you and I are together. And they can see the thing moving.

ERWIN: Coming right out of the ground?

SHARP: Right. It's an experience they will never forget. They really have stars in their eyes when they come back from having looked at that red lava. We've been lucky in that regard on about three-quarters of our trips. Frequently we take them at night and they can see flows coming down the pali, with its steep slopes. But getting close to it and seeing it from a distance are two different things.

Since the Dudley Wright trip, I have been taking each year at least one faculty member along who could run a field trip like this. Thus we have built up a core of people who can run the trip. I have taken others who would never do that—because frequently a student will drop out at the last minute and it's been easier to substitute a faculty member. Lots of times I've had as many as three faculty members. Those people contribute and it works very well.

The next-to-the-last day is a relaxed day. We've been at Hapuna, we've backtracked to see some big inclusions in the lava, and then come north to Waimea, right on the edge of Kohala, the northernmost volcano on the Big Island. That's where the big Keck building is.

ERWIN: That's the Keck headquarters?

SHARP: The Keck building houses the staff that services the big telescope on Mauna Kea. We do not usually go to the top of Mauna Kea, but I think next year Jim Westphal [professor of planetary science] is interested in helping us. Westphal owns a house on Hawaii. He retires on the twentieth of June. I just saw him the day before yesterday. He will take our people to the top of Mauna Kea. We go up there for geological reasons as well as astronomical. There we see features that have been glaciated, and moraines. My doctor won't let me go up there, so we haven't generally been going to the top, but Jim and Lee will take them up there, probably next year.

ERWIN: And that will be a first, as part of your trip?

SHARP: With the group, yes. We've never been to the top of Mauna Kea before. We see it a lot of the time. It frequently has snow on it in March when we're there. Mauna Loa is just 100 feet lower—almost as high, but you'd never think so looking at it. It's a great, big, curved shield. [Mauna Kea is] more advanced in its evolution.

Pahoehoe is a trip I wish somebody had [taken me on] when I was a young student—not that I would have become a vulcanologist, but it would have sparked an interest. For example, in 1959 I would probably have tried to get over to see the Kilauea Iki eruptions, which were probably the most spectacular on record, because the lava was being shot out sideways in an arc out over the Kilauea Iki crater. Many people say to me, "Well, I saw the 1959 Kilauea Iki eruption, so I don't have to go to any other. I've seen the best." Of course, we didn't have that privilege. We do go into that area and see the results. We do a lot of other different things. What we do depends a lot on the weather. We always have a top priority of getting to the active lava, if possible. We do that at any time.

We travel in four to five vans. When we have sixteen people, we have four vans; when we have twenty, we have five. We have a nice relationship with Budget. Most car rental companies won't take the backseat out of their vans, so it's a seven-passenger car and we don't have storage for the equipment. Thanks to Keilani Curnan, one of the managers of Budget in Hawaii, we get vans with the backseat out. Keilani would fracture all kinds of rules and regulations. We've been renting from her—first for alumni and then for these trips—for years. I would sometimes have trouble with the local field manager, who would say, "No, we won't take the seats out." I'd say, "Look, here's a letter from Keilani Curnan that says that you will take them out. If you won't take them out, I'm going to get on the phone." We had very nice relationships with the people after that. The Hawaiians are delightful people—very generous and bright. I stayed on the island for a day or two after the trips to store equipment and relax for a day. I loved the trips, but when they were over I always heaved a big sigh of relief, saying, "Well, nobody got hurt." The students come back to Pasadena just bubbling. One can learn a tremendous amount by seeing something in action.

ERWIN: Have you actually made anyone into a vulcanologist that you know of yet?

SHARP: It's hard for me to say, because people don't always get classified that distinctly. I'm sure that all of them now have an abiding interest in vulcanology that they never had before. Vulcanology was kind of a side issue in geology until the space exploration program got under way. Then you had the lunar surfaces and Mars in sight, and suddenly vulcanology grew again in size and interest to a whole lot of people who had never given a thought to it before. That doesn't mean they became vulcanologists, but they're sensitive to the field. Certainly Project Pahoehoe does that. It creates, I'd say, a life-long interest in vulcanological happenings.

ERWIN: It sounds like a fabulous project.

SHARP: It's one of the best things I ever did. It will break my heart if it has to stop. And Ed Stolper says, "I'm not going to let it stop."

ERWIN: Well, good for him.

SHARP: I'm trying to do my best, but I got slowed down. Don't ever have viral pneumonia. It just washes you out. Bacterial pneumonia can be cured with antibiotics, but with viral pneumonia you just have to wear it out. It affects you energy-wise, endurance-wise, strength-wise. I lost a lot of weight and endurance. It affects your mental processes. A nurse said, "You aren't getting enough oxygen in your brain." It's that simple.

Anyhow, that's Project Pahoehoe. The current situation is I'm trying to raise a half-million dollars. I know that I could get several alumni who would make a pool; if I get several pools going, I think we'll make our goal eventually. Most of my prospects are already committed in many ways to charitable giving, including right here at Caltech. Some that I would like to have tapped are simply not available; they have heavy commitments already. So it's tough going.

ERWIN: Well, maybe this leads us into the alumni trips. One question to ask about those would be, are these alumni who give to Caltech?

SHARP: Two of the five letters I first sent out were to alumni who had never made a major gift to Caltech, so we'll see. But the alumni trips have never been oriented toward fund-raising. They aim to make the people feel close to Caltech; indeed, they seem to work that way. Our geology division has received two unsolicited grants from participants on alumni trips.

ERWIN: Is your division the only division that runs trips?

SHARP: No, but nobody else does it on a consistent basis, or runs trips of long duration. The alumni trips got under way when Lee Silver and Gene Shoemaker, who was at that time on our staff, were trapped on an airplane at the Dallas-Fort Worth airport. They had a minor mechanical problem, so the company didn't unload the plane, because they thought it could be fixed promptly. I don't know for sure, but they were probably on the ground for an hour and a half. The hostesses were serving free cocktails to keep everybody happy and relaxed. Gene and Lee—both of whom had worked in the Grand Canyon; Shoemaker, particularly, had rafted the canyon a lot—got to talking. At that time, our division had no endowed named professorship, so

they said, “We’ll get one for ourselves. We’ll run raft trips using big commercial rafts operated by a company.” Silver and Shoemaker came back and suggested the plan to Barclay Kamb, our division chairman at the time….He called me and said, “What do you think about the Silver-Shoemaker idea?” I said, “I think that’s great.” I went back to my office and wrote a proposal for “the trip of a lifetime.” Maybe we can’t take you to the moon, but we’ll do the next best thing; we’ll take you through the Grand Canyon with four geologists aboard.

Stan and Ernie Avery signed up immediately and then persuaded friends of theirs, some of whom were already Caltech benefactors, to go. We had some great trustees aboard. In the first year [1976], we made a half-million dollars; that was our most fruitful year. The next year we didn’t do so well. We ran the trip a third time and sort of finessed it. Stan Rawn had been giving money to the institute without designation, building up a good sum over a number of years. We knew this, so we asked him, “How would you like to sponsor the last trip, and bring your friends? You can bring anybody you want.” He bought the last trip and then brought friends and family. So, we got our million dollars.

We have tried to think of other geological trips that could be run that would have the appeal of the Grand Canyon. It isn’t easy. The astronomers could possibly offer a bang-up experience, but their heads are above the clouds. They could do something unusual with both the Palomar and Keck telescopes. They could possibly raise a lot of money for a new astronomy building; however, they don’t seem interested.

In our division, we have thought of things like taking people onto the Canadian Shield, to the oldest rock known anywhere in the world—about 3.95 billion years old. You have to fly in on a little float plane to this locality, but they could get out and hammer out a hunk of the oldest rock themselves and say, “Now this is the oldest preserved hunk of Earth.” It’s probably only close, because the Earth’s surface initially was so pummeled with all the incoming chunks that the initial surface has been totally destroyed. There’s no record of it. The library is burned.

What you need is somebody with fire in their eyes, who is almost fanatical about some particular thing interesting to a nonprofessional audience. The canyon is terrifically appealing. Day after day after day at the bottom of this magnificent gorge, a constantly changing scene—it’s great. We tried to do something similar in Hawaii on a reduced scale. Howard Keck was going to participate, but he withdrew and we never brought it off. It wasn’t on the scale of the Grand Canyon trip, nor as attractive. Credit that to Lee Silver and Gene Shoemaker.

The later alumni trips in geology [started in 1980, and] got under way because Lee Silver then was on the alumni board of directors. They talked about things they could do for the alumni. He said, "Why don't we run them a field trip? Let's run it to the Grand Canyon Park." So they set up a date in late April. I saw an announcement and I said to myself, "Those two guys will never get their calendars together so that both of them will be able to go on that trip. They're going to come around to me about three weeks before the trip starts and say, 'Can you substitute for one of us on this trip?'"

Begin Tape 1, Side 2

ERWIN: You saw this coming.

SHARP: I saw it coming. I was happy to be involved. I had been in the canyon a lot and I loved it. The idea of going back to the Grand Canyon was great. I'd go any time you gave me the chance. So they came to me, and I said, "OK, I'll help you. What's the schedule?"

Phyllis Jelinek was the first executive director of the Alumni Association. She had been a secretary, hired by Jim Black, who had a genius for picking ladies who were very able, handing them a chore, then letting them alone. Phyllis Jelinek could do almost anything she wanted in cooperation with the alumni board in running the Alumni Association. She set up the logistics for the alumni Grand Canyon trip but unfortunately left the scheduling to Gene Shoemaker. Now, Gene Shoemaker was one of the most wonderful guys who ever lived, but he had no respect for timing or organization. He was going to put the alumni on a bus at the Athenaeum early in the morning and drive all the way to the south rim of the Grand Canyon for supper. I looked at that schedule and said, "Look, if you guys want me to participate in this, we're going to change things. I will not follow that schedule—it's inhuman! You just don't do things that way. We're going to stop and have supper in Williams. There's a good place to eat there. And then we'll go on after supper and get to bed by nine o'clock or so. We're not going to go all the way to the rim before supper." I got off to a real slow start with Phyllis Jelinek, as she had made the arrangements already.

ERWIN: How did it eventually work out?

SHARP: Phyllis had tremendous respect for full professors—I don't know why, but she did. She was late to the meeting we'd arranged to talk about my participating. She said, "Here were two full professors cowering on the floor, and there was this old guy standing word-whipping them." Carolyn Shoemaker was there, watching….The alumni trip plan was this: Drive to the Grand Canyon, stay overnight, walk down the trail to Phantom Ranch, stay in Phantom Ranch two or three nights—I forget how many—explore the region around there, back up the trail, final dinner, drive home. One day over, one day back, and the rest of the week in the canyon. We had a very nice bunch of people, and the trip went very well.

I have a place in Montana. I call it a cabin, but it's more than just a cabin—though it's not a house. I was spending most summers there by this time. Following the Grand Canyon alumni trip, I got an invitation from a couple who planned a summer party for the Grand Canyon alumni. I wasn't going to come down from Montana for a party, but I wrote a letter to Phyllis Jelinek saying that she could take the letter, if she wanted to, and read it to the people there. In it I said, "You know, we did it wrong. Almost anything is anticlimactic after the Grand Canyon. We should have saved that and run some other trips before we did the canyon, unless you want to go to Hawaii." When I came back—I had moved to Santa Barbara by that time, but I stopped here to eat lunch in the Athenaeum. Suddenly, here was Phyllis Jelinek at my shoulder. She didn't say hello or anything else, she just said, "We want to go to Hawaii!" And I said, "Well, I'm going over there in a couple of weeks to work with some fellows who need geological help and I will look around and see what kind of trip could be put together."

And I did that. We ended up running the Hawaiian trip five different years. We've run an Alaska trip five times, and Yellowstone-Big Horn five times. We have run many different trips, many more than once, usually one or two a year with a different group of people. We always have a trip every year—sometimes two. The alumni trips in the Grand Canyon [are] still going on. We have repeats—like we did in Zion Canyon, Cedar Breaks, and the southern Utah area. We did those on successive weekends in the fall, when the fall colors were great. We sometimes ran the same trip twice in a year. Other times we'd run two different trips in a single year. We've been to Iceland, because we had complaints from people on the East Coast that all our trips were western-oriented.

We ran a trip in Pennsylvania. It was a long weekend, Friday through Sunday, but it went very well. People gathered in Hershey and we got down to Gettysburg. A lot of those people then got interested in going on our other trips. They would cross the continent for that purpose.

ERWIN: Just briefly, what did you look at in Pennsylvania—the local geology?

SHARP: Pennsylvania is geologically a very rich state. In the Appalachians, and the Valley and Ridge Province. The valleys of Pennsylvania are just gorgeous. You've got the Amish and the Mennonites giving them flavor. Concerning the geology of the Gettysburg battlefield, the Union Army did have the advantage topographically, in occupying the high country. Lee didn't want to fight there. Actually, he was headed for the capital of Pennsylvania—Harrisburg, on the Susquehanna River. He figured to cut off the railroad going to Washington. He didn't want to fight in Gettysburg, but he got drawn into a battle there. He lost that battle partly because of the geology. Why is the high country on the Union side? What makes it? How did the Union get it? It's too long a story to relate here.

Elsewhere in Pennsylvania, you've got underground caverns—limestone caverns and wonderful structures of the rocks. The oldest mine in the whole US is the Cornwall mine that supplied iron for George Washington's cannons. That's now a state park, and it's a delightful one. Pennsylvania is a very rich state historically. The rural population of Pennsylvania is greater than the urban population; there aren't many states about which you can say that. I had flown back and forth and back and forth across that country many times and always ached to get down into those beautiful valleys. We ran a nice trip there.

Then we ran a New England trip twice. The geology is different and the fall colors terrific. New England geology is worth attention for the interesting story it tells.

ERWIN: That's where I grew up—in Maine.

SHARP: Maine is great. We were in Vermont and New Hampshire, so we didn't get into Maine. Those trips went very well. Janet Davis and I had explored a trip in North Carolina where you have the Great Smokies [Great Smoky Mountains]. That would have been a good trip, but

something happened in the administration of the Alumni Association. They had a committee that was concerned with trips—the program committee—but they never scheduled the North Carolina trip. Also at about that time our vice president of business administration very abruptly removed Janet Davis from the directorship and substituted Judy Amis. We never did do that North Carolina trip—too bad, it would have been a good trip.

We worked up a trip in the Upper Peninsula of Michigan, in the copper country—that big peninsula that projects far out into Lake Superior. There they have native copper. Copper normally comes as a chemical compound, but there it's native copper, which is most unusual. No refining required, you just melt it and it's there. That area has a great history. We were going to do a fall trip up there, when tree colors are nearly as good as those of New England.

The Midwesterners don't travel well, and the centers of population—Minneapolis, Chicago, Detroit—were peripheral to this Michigan copper area, about 300 to 400 miles from those places. We need close to forty people to make such a trip self-supporting.

ERWIN: They are self-supporting, and they don't aim to make any money? That's not the point?

SHARP: They aren't designed to make or lose money. Not enough people signed up, so we had to abort that trip. Too bad. We had it worked up for the right time of fall. The Upper Peninsula is not quite as good as New England in the fall—New England falls are just beyond comparison. I guess New Hampshire and Vermont are even a little better than Maine, but the mixture of conifers and oaks in Maine is terrific. You get very russet-colored red oak, and the green conifers make wonderful company.

We did two Iceland trips, largely for the benefit of the eastern seaboard people. It's an expensive trip, and it's not much fun getting there, because all the flights that go into Iceland leave New York at about eight o'clock in the evening and arrive in Iceland the next morning around six o'clock. The first day is devoted mainly to recovery.

Our best trip of all is the Yellowstone/Big Horn/Beartooth area. It's a six-day trip. The variety of geology you get is just incomparable. You've got Yellowstone and all the hotspots. We never stayed in the park; we always stayed peripheral to it, for comfort and economy. That's a great trip; we ran it five times and we could probably run it again if we advertised it.

ERWIN: You were obviously closely involved in these trips. Who else participated? Lee Silver?

SHARP: Lee Silver has come in; now Lee is basically taking over. I ran most trips myself. I was the only one aboard. Occasionally I would have somebody else, not necessarily from our faculty. We ran three trips up in the St. Helens area. There I got a fellow named John Elliott Allen, from Portland State University, who knew the Columbia River Gorge like the back of his hand. Usually in Hawaii I had Dan Dzurisin, as mentioned. He was at the Hawaiian Volcano Observatory. One year I had Gordon Eaton, the provost at Texas A&M, aboard to help, because Dan couldn't. This man had once been the chief scientist at the Hawaiian Volcano Observatory.

ERWIN: I see. But he wasn't a Caltech alumnus?

SHARP: Yes he was. When I couldn't get Dzurisin, I started to think of others. There were two good possibilities. There was the director of the United States Geological Survey, a Caltech alumnus named Dallas Peck, and Gordon Eaton. I thought Eaton would do a better job than Peck, but both are top-notch. You don't easily pry them out for just a week. I wrote a long letter to Eaton, and my last sentence was "I look forward to your favorable response, and I expect it." His secretary got the letter, opened it, and before she ever passed it on to Eaton she showed it to the president and various other people in the A&M administration. They ganged up on Eaton and said, "You've got to go." So he came, and he was great.

ERWIN: But when you went by yourself with forty people?

SHARP: I nearly always had help. In Pennsylvania I had a professor from Penn State. So about half the time there was somebody to help me.

ERWIN: How did you manage the forty people?

SHARP: We'd travel by bus, and it would have a loudspeaker. We usually didn't do a lot of long hiking, but we did some, and I had a portable speaker. If we had people who weren't able to do a hike, I'd say, "Don't even try this. It would be better if you could amuse yourself looking for

flowers until we come back." At the end of the day, if you were the only leader aboard, you'd had it, because you had been answering questions or talking the whole day long.

ERWIN: Well, that's what I was thinking. Someone was going to be at you the whole time. And you have six days and more of that.

SHARP: Well, yes, you do need help. Lee Silver was on four of the Yellowstone-Beartooth trips and led one without me. He's also been on all five of the Alaska trips and led one without me. Now he's running his own trips to the plateau country in Colorado. He's been running those by himself, without any help. You can do it, but you work hard. Anyhow, the trips have been very well received and successful. The problem on big trips is that they are expensive.

ERWIN: How expensive? Can you give an idea?

SHARP: Well, $1,600 per person or something like that, for a four- or five-day trip. We did Glacier Park three times, once for the Caltech Associates and twice for the alumni. The Glacier trip was probably around $1,500 or something like that. On the Icelandic trip, round-trip airfare alone from New York is around $700. Actually we were servicing roughly the same group of about 200 people. The rapport between those 200 people was incredible. They loved the trips, partly because of the fact that they were yak, yak, yakking to each other and having such a good time. I don't think there's any question that the trips have created a lot of goodwill for the institute, and probably have benefited the solicitation program, but we don't use them to raise funds. We try to bring the alumni into the Caltech family in a major way, and we did Hawaii five times, Yellowstone five, Yosemite twice, Death Valley twice, Zion twice, Iceland twice, Alaska five, Pennsylvania once, Vermont twice—I've never really counted up the total number of trips, but there are a lot of them. They started in 1980 and they are still going.

ERWIN: They've become an institution.

SHARP: Right. Bill Muehlberger ran a trip for us down in the Big Bend area of Texas. Lee has run trips in Colorado, New Mexico, and multiple times in Borrego Springs, California. Another

type of field trip that's been developed here is what I call the nonacademic staff field trips I ran for the division for a good many years….We started with just a one-day trip around the San Gabriel Mountains and another one-day trip up the Malibu coast. Finally we got to do a two-day trip, and all subsequent trips were for at least two days. I think the first one was in 1973 and we ran them through 1996—twenty-three years. Now we have stopped them, because we were not benefiting the people that I wanted most to benefit, the current institute nonacademic employees. We had a group of people coming on these trips who were largely no longer working for Caltech.…

ERWIN: Did the people who went on those trips pay or did the institute?

SHARP: The bus fare was a major cost, and it was paid by Caltech. I nearly always had a small group that camped out and a group that stayed in a motel. For the campers it was a pretty cheap trip, except the meals. The people paid for their own meals and housing.

I've been retired for twenty years. When I retired I phased myself gradually out at Caltech….But I still taught a course after I retired, and I want to talk about that course for a moment. It was a course I set up back when I was on active status. In our department, a high percentage of the students who come here as graduate students have basic training in physics, chemistry, or astronomy. But now they're in the geology division, and we felt they ought to know a little geology. They didn't want to know any geology; they wanted to do their thing in the way they had been doing it. So I set up what I called a traveling field course, in which everybody who came had to be the authority on some subject that was assigned or chosen. We went on weekends—Friday, Saturday, Sunday—over a radius as far east as Nevada. I had nine or ten trips; they'd run in a three-a-year succession. Some students would take the course three times, so they'd get all nine trips. There were a lot of places to go: Death Valley, Owens Valley, Yosemite, the Mojave Desert, the Imperial Valley, the South Coast, the North Coast. I could handle only fifteen students. I tried taking everybody one time, but that was no good. Students had to give talks, and we couldn't get to all of them in three days. So I imposed a fifteen-student limitation. It was a pass-fail course.

ERWIN: Did this course have a name?

SHARP: Geology 136, The Geology of the Southwestern United States. It's still in the catalog. I declared a moratorium for this year—fortunately, because I wouldn't have been able to do it. I always ran this partly through the winter term and partly through the spring term, because there was so much competition for field equipment and transportation. A lot of the competing courses are required. This was not a required course, but even our radio astronomers took it. I did it especially for these people. I'd say, "Look, I know you don't want to take a course in mineralogy, but let me take you out in the field and show you some geology." Well, the word got around, so I had to limit the participants, and I had to have [a system of giving] priority. If one had never [taken such] a course, that was high priority. However, during the last two or three years of the 1990s, in competition with the required courses, I'd start with fifteen people and end up with only about five still registered in the course. I said, "We can't afford to run it this way," so I declared a moratorium for this year, which was just blind luck. It turned out that I couldn't have done it, because of my right leg.

Here I want to talk about the virtues of a small institution. When I retired, an emeritus professor could not teach his or her own course; you could teach, but only in somebody else's course. So I wrote a letter to the provost, Bob Christy. I said, "Bob, here's what I'm going to do." I explained about this special course, which I had built up over the years, and that it would still be in the catalog over the name of two other professors, who weren't going to teach it. I didn't make any bones about it. I continued, "It's a pass-fail course, so I'll take the grade card around to one or the other of these people, have them sign it, and send it in. But I'm going to teach it. If I don't hear from you, I assume that this is OK." So then he wouldn't have to write me a memo telling me to break the rules. I never heard from him, so I taught it for twenty years. To this day, Christy denies ever having gotten that memo.

ERWIN: Of course!

SHARP: Actually, I think, it could have been intercepted by Neal [Cornelius J.] Pings, who was the associate provost at that time. Neal could have decided that Bob doesn't need to see this.

But now the rules have been changed to conform with the laws of the country. Now an emeritus professor can continue to teach his own course.

ERWIN: So you've never stopped teaching?

SHARP: Yes, but I've taught only the one course.

ERWIN: Every year?

SHARP: Yes, until I voluntarily stopped at twenty years.

I was going to tell you what I've been doing since I've retired, besides teaching. I've been writing books that are mostly an attempt to describe geology to lay people. You see, scientists do not generally help people understand their science; they just say, "Give me *x* amount of dollars, because I'm going to do this, and it's too complicated to explain to you," or something like that.

ERWIN: You're a shining example of someone who has written many books for the public.

SHARP: Maybe. Anyhow, I've been writing books that are designed for lay people. I'm working on my latest book right now.

I also want to comment on Jim Westphal, Peter Goldreich [DuBridge Professor of Astrophysics and Planetary Physics] and guys like that—about the kind of current leadership in the division, which I think is superb. And about biogeology. I'm optimistic about the future of our division and about the future of the institute. We needed David Baltimore [Caltech's president 1997-present] very much. The chairman of the faculty [search] committee [Kip Thorne, Feynman Professor of Theoretical Physics], came and talked to me, and I thought he was going to solve this matter like you solve problems in physics: You get all the parameters, put them in a formula, turn the crank, and the answer comes out. So when he [was ready to leave] I said, "Let your intuition play a part in your decision. It's very important that you kind of relax all those parameters and let your intuition play its part." When the announcement came about Baltimore, I was really surprised. I didn't think the committee would be that daring. I wrote the chairman a postcard with one word on it—"Bravo!"

I'm optimistic. I think Baltimore is still operating at a gentle throttle, finding his way. He hasn't really opened up yet, but he will, I think, sooner or later. I haven't met him; I will eventually. When I do, I'm going to say, "David, Caltech, you know, had over forty years of superb leadership and direction and inspiration from Millikan and DuBridge. They were great. It's time we had another great president. I believe his name will be David Baltimore." [Tape ends]

ROBERT P. SHARP

SESSION 2

October 12, 1998

Begin Tape 2, Side 1

SHARP: When Millikan turned to somebody for advice on setting up a geology division, you would have thought he would have turned to his former colleagues at Chicago. He didn't. He turned to John Campbell Merriam, who was a vertebrate paleontologist formerly from the University of California.

ERWIN: How do you think he knew Merriam?

SHARP: During World War I, John Campbell Merriam had somehow gotten himself to Washington with a group of physicists including Millikan, who were dealing with the problem of the German submarines sinking Allied shipping. Now, what Merriam could bring to that outfit I don't know, except political acumen, I think. He, apparently voluntarily—now, I don't know this—went to Washington trying to find where he could be useful in the war effort. Somehow he got together with these people. I don't think he really knew Washington that well, but he was a man who could have made his way in Washington very easily, and that may have been what they wanted him for.

ERWIN: Was he a Californian or a Westerner?

SHARP: I don't know what John Campbell Merriam's antecedents were. He was at Berkeley for a long time and was very well known, famous, and a very dominant character, who did not get along with Andy Lawson [Andrew C. Lawson], who headed up the geology group. To this day there is a little wariness between paleontology and geology; they're something of two separate operations, I think, even now. But during the time when Merriam and Lawson were in charge they just didn't get along. Both were powerful men.

Millikan knew John Campbell Merriam from their wartime association. Instead of turning to the people at Chicago, he turned to Merriam for advice. I can only guess as to why

Millikan wanted to have a geology department. One reason he probably didn't go to Chicago was that he didn't have anything to do with the geologists at Chicago and didn't want to have anything to do with them now. He was more comfortable with John Campbell Merriam.

Now, Merriam was very glad to advise him and proposed that he hire two of his protégés at Berkeley, [John P.] Buwalda and [Chester] Stock Stock was a vertebrate paleontologist. Buwalda was primarily a field geologist and a structural geologist. He was somebody who could go between John Campbell Merriam and Andy Lawson; as a matter of fact, I always felt, in my relationship with Buwalda, that he was more of a Lawson man than a Merriam man.

So John Campbell Merriam suggested the two of them to Millikan, and he hired both of them, but with the understanding, apparently, and probably on Merriam's recommendation, that Buwalda be the head of the department. That was the right decision. Stock would not have been a very good department chairman at that stage. So Buwalda arrived in Pasadena with a blueprint of the Berkeley department in mind. He got his PhD there and came back as a young professor on their staff not very long after his graduation. He set things up here very much in the image of Berkeley, with even greater emphasis on seismology, which of course was the right thing to do in Southern California. He brought the idea from Berkeley. Berkeley had seismology, and he thought we ought to have seismology, too, because we've got just as many, perhaps even more, earthquakes.

ERWIN: Of course, Pasadena had a seismology station at that time.

SHARP: That was created by the Carnegie Institution. Harry O. Wood of their staff had already made a start. To begin with, the Carnegie was into seismology more than Caltech was. Eventually—I don't quite know how he managed to do it—Buwalda gently and slowly phased out the Carnegie Institution. One way he did it was to bring [Beno] Gutenberg from Germany, an outstanding world seismologist whose reputation was way above that of people here.

ERWIN: You must have been here when Gutenberg came [1930].

SHARP: It was just about the same time.

ERWIN: But you would have been an undergrad.

SHARP: I took sophomore geology, and my remembrance is that Gutenberg lectured once to us in sophomore geology. I'm not positive. By the time I was a senior, I knew Gutenberg modestly.

ERWIN: What was Gutenberg like as a teacher and a lecturer? I don't think we have many people who have been able to tell us that.

SHARP: As a scientist Gutenberg was superb. Of the people in our geology department at that time, Gutenberg was the most outstanding, nationally and internationally. He was a small man, very polite. It was always a joke that you couldn't get through a door after Gutenberg; you had to go first. He was extremely polite.

ERWIN: This is a European trait?

SHARP: No, a European professor usually doesn't defer to students. Just the reverse.

ERWIN: But Gutenberg did?

SHARP: Oh, yes! It didn't matter who you were. you went through that door before Gutenberg. His wife was a very gracious lady. She was also small.

Gutenberg taught a course but—like all of the seismologists, in particular [Hugo] Benioff—he didn't take his teaching too seriously. I would meet him in the hallway during an exam period and he'd say, "I've got all my exams corrected." "How could you possibly do that? You only gave the examination this morning, and here it is noon and you've got them all corrected." Gutenberg gave true-or-false exams, which is not a very good way to teach in an advanced course like his. The lecture he gave in the sophomore physical geology course was really mostly over the tops of the heads of all of us. As I got older and learned more, then I began to understand what he had been talking about. It was important, but at the time and in the way he presented the subject, it was difficult to understand. So the seismology staff—and

especially Benioff, who just refused to do any teaching—contributed little to the educational program. Benioff regarded himself as solely a research professor, a species we didn't have. The Gutenbergs were socially part of the department and entertained at their house very nicely.

If you were doing something with Gutenberg, he would cut it short when he felt that you understood. Somehow he'd make it clear that he had something else to do, and he would go do it. He'd say, "Well, I guess we don't have anything more to talk about." There's no question, as I've already said, that the reputation of the Caltech geology department rested nationally, and to some degree internationally, on the Seismology Laboratory. Buwalda didn't publish. Stock did; I'll come back to Stock. But it wasn't a distinguished staff, and it wasn't a distinguished department.

ERWIN: When you say that it was built along the lines of the department at Berkeley, did that make it any different from any other university, particularly in this country, at that time?

SHARP: No, I'd say that the Berkeley department was pretty representative of the whole bunch, although the emphasis on seismology probably gave it some distinction, and they had their own publication series. That was not unique at that time, though it wasn't general.

ERWIN: Buwalda wanted to establish that here too, didn't he?

SHARP: Buwalda certainly wanted his own publication media, although Buwalda himself never published very much. He also set up a museum. What is now our division office was once a big room that was a museum. That was one of the first things I changed. I said, "We're not in the museum business."

ERWIN: Now, was that in the Arms building [Charles Arms Laboratory of the Geological Sciences]?

SHARP: Yes. You'd come in the north door and walk straight ahead through big iron doors, where it was all museum.

ERWIN: What was the museum designed for? Was it designed to lure the public in?

SHARP: [Laughter] I don't know that anybody ever asked the question. If you were going to be a real geology department, you had to have a museum, because that's where you'd exhibit specimens, and it would be good for the students. It was, like so many things, a custom, so to speak. You did it because it was customary to do it, without asking, "Do we really want to do this?" I don't know whether Buwalda ever asked himself questions like that. What he ended up with was very much like the Berkeley department in terms of seismology, field geology, a publication series, and a museum….

It's changed a lot now. Geology gets a lot more respect among physicists than it did back in those days. Physics was riding high and handsome, and they were arrogant. Now it's biology that's the top science. Anyhow, our own geophysicists really introduced physics into our department. I sat down one day with our faculty of about thirty members and made a hasty survey identifying at least ten whose initial and primary science training was in physics. Not all physicists fit well into a geology department, but when you get a physicist who fits, that's gold in the bank. Physicists bring so much to the field of geology; a lot of our faculty are geophysicists.

Andy Lawson at Berkeley was a very strong department chairman and tended, I think, to override contrary opinions without making a fuss about it. He made independent decisions without a lot of consultation. I think Buwalda modeled himself somewhat on Lawson. We lost a very good man early in the game—Wendell Woodring, a paleontologist who went on to a notable career. He was on the faculty before I got here. He was a good friend of Chester Stock's. I asked Chester one day about Wendell Woodring. I said, "I thought he was one of the best invertebrate paleontologists in the whole country. How come he didn't stay?" Stock said, "He came to me and said, 'I can't stand Old Iron Face any more. I'm leaving.'" He didn't feel that he was consulted on matters in his field. We lost a good man there. My remembrance as a student is that Buwalda was the dominant person in the department, in terms of operations. I don't know how much he consulted with anybody, because as a student you don't know these kinds of things.

ERWIN: Was he out in the field?

SHARP: Only on student trips and consulting jobs. He was good in the field. We always had a field trip during the spring vacation. Everybody in the department—all the students—were expected to go. You didn't get any credit for it, but they were great. As a matter of fact, Buwalda led many good trips. One of the things I got the most out of as an undergraduate here were the spring field trips and the other trips that Buwalda led. He was good at it.

ERWIN: He was chairman of the division for a long time.

SHARP: He was chairman for over twenty years. He taught the elementary physical geology course and courses in structural geology and engineering geology. I regarded him as a good teacher. He tried to make you think about things, as well as just listen to what he thought about them. When I got away from Caltech and found out how the rest of the country viewed it, I realized that I had an exaggerated opinion of how good Buwalda really was. He was not known in the rest of the country.

ERWIN: That was largely, I suppose, because he didn't publish.

SHARP: Yes. Or give presentations at national meetings.

ERWIN: He didn't attend meetings?

SHARP: He gave short papers at local meetings but not at the big annual gatherings. Early on, he worked for the United States Geological Survey for at least a year in Washington, and I think he had an appointment as an instructor at Yale for a year or two. I'm hazy on that. He'd been there, but not for very long. He went to the University of Washington as an undergraduate [Buwalda matriculated at the University of Washington but spent his three last undergraduate years and got his degree at the University of California, Berkeley.—Ed.] He was a big, husky man, and rowed on the [University of] Washington crew, which was one of the outstanding national crews at the time. He was totally western in his orientation. He was a deliberate sort of person. When he talked, it was deliberate. He was careful with what he said. I think I rated

him, when I was here, much more highly than he deserved, but I didn't know any better. One of the best things that I ever did was to go from Caltech to Harvard. Take a map of the United States—it's hard to get two institutions that are geographically farther apart than Harvard and Caltech. Buwalda had four children. He had a tall wife, who was ambitious and politically oriented [Imra Wann Buwalda]. She was active in California and national politics.

ERWIN: The Archives has some of her papers as well. She seems to have made her mark in some way.

SHARP: Do you have a copy of her book [a history of Caltech]? It never got published. It basically wasn't publishable.

ERWIN: I understand she was a policewoman?

SHARP: Yes, she was very interested in the penal system, particularly the women's penal set-up. She was a recognized, influential person in California politics. She talked about "Pretty Boy Reagan." [Laughter] The Buwaldas lived in a rather nice stucco house on the eastern part of San Pasqual Street. Imra Buwalda was never around the department much until John Buwalda died and she got to working on her book. The children we didn't see very much of, either. I never remember being in the Buwalda house as a student. He didn't hold seminars there; all his seminars were on the campus. The family didn't mix in much.

ERWIN: You never had a sense that he was unhappy here or anything like that?

SHARP: No, I don't think John Buwalda was unhappy here at all. He was riding the crest of a wave. He was the head man. There was no question but that he was top dog in some off-campus endeavors, such as Yosemite Park and the Colorado River Aqueduct.

ERWIN: And getting our division going.

SHARP: Yes, right. And by that time, he was deeply into consulting activities that addressed problems in Southern California.

ERWIN: Millikan supported that?

SHARP: I don't know whether Millikan supported Buwalda specifically or not. There definitely was a time when engineering was dominant at Caltech. I think I got here in a transition period.

ERWIN: Yes, I would have thought so. Only engineering dominated in the twenties—not much longer than that.

SHARP: Anyway, the engineers regarded consulting as their god-given right.

ERWIN: That's true.

SHARP: The story is—and it's a story that probably has no real substance—that Buwalda got into consulting because his big family needed more money and Mrs. Buwalda said, "Go out and earn more money," and he did what she told him to do. I don't know whether that's right or not. That's the kind of thing that somebody can make up. Buwalda was on the advisory board for Yosemite Valley. He was very proud of that relationship and kept telling us about experiences he had with regard to building roads in Yosemite or doing this or doing that. He regarded that as a plum, obviously. I think his motivation was sincere. He wanted to preserve it in as near its pristine condition as possible and still make it available to the public. If they built a road, they consulted with him to make it as inconspicuous as possible and still a sound highway. He got very heavily into what we now call engineering geology. He taught a course in engineering geology. The field was not as well developed as it is now. There weren't many engineering geologists at that time, and Buwalda was basically an engineering geologist in many ways.

ERWIN: What do they do?

SHARP: They deal with the impact that geological relationships have on engineering activities.

ERWIN: Buildings?

SHARP: Dams, aqueducts, and that sort of thing. He worked, along with [Frederic L.] Ransome on the metropolitan water aqueduct: "You're crossing a major fault here; you've got to worry about that." They did a lot of work on such problems. The Owens Valley aqueduct was already in, but the metropolitan wasn't. Buwalda and Ransome both worked on that.

ERWIN: Those would have been big public works projects?

SHARP: Big public works projects indeed! Buwalda did a lot of work for the Pasadena water department on the geology of Pasadena, which was never published. But I have, or had, a copy of Buwalda's report, which he gave me at one time. It's a really good analysis of the groundwater basins in the Pasadena area and how they were controlled by faults. It's a useful document.

ERWIN: So there's no question that he was really competent?

SHARP: No. His time and effort, aside from his teaching and administration, was all in applying geological knowledge to practical problems. There's no question about that. And he also paid attention to administration. As a young man, he had written a couple of reasonably good papers. A lot of his engineering geology work, of course, was, I would expect, confidential to the sponsor. But water supply, dams, aqueducts, highways, bridge foundations, and almost anything like that would come under public domain.

ERWIN: So you've been gone from Caltech for only about ten or twelve years.

SHARP: I think I was gone twelve years, all told. I stayed one year as a graduate student [1934-1935]. I didn't really want to do that, but Caltech was not well known, and we had a grading system based on numbers—2, 3, 4—that nobody could understand, although it was the same

thing, practically, as an A, B, C, D. As I said, the staff wasn't distinguished. I applied to several colleges, mostly in the East—Princeton, Harvard, Yale.

Here is a little story. I went to our dean of students, [Frederic W.] Hinrichs, who was a West Point alumnus and a very straightforward man, and asked him for a letter of recommendation. He knew me fairly well. He called his secretary in and dictated that letter right in front of me. The last sentence was "And this boy was captain of our football team." People told me subsequently at Harvard that, to them, "this boy" had to be a great big, dumb oaf and they didn't want him. And that, I think, helped sink me the first year I applied for graduate studies elsewhere.

ERWIN: The reference to the football team?

SHARP: Yes. One person told me—at Harvard at least—that the comment sank me the first year. It didn't sink me the second year, because they didn't have that letter before them; I got other letters the second year. So I spent one year as a graduate student at Caltech; I really didn't intend to or want to do that, but this was the one place that provided significant support. We didn't get any money, but we were housed in the Athenaeum and fed there, and we didn't have to pay any tuition. When I went out on my field projects from Caltech, I'd go to the Athenaeum kitchen and ask for a half-pound of bacon, a half-dozen eggs and some bread and fruit. So although we didn't get to see any money, we were cared for in style. The other students were a great bunch of guys: Willy Fowler, [Simon] Ramo, [Dean] Wooldridge. As [graduate] students living in the Athenaeum, we'd eat together, but also with interesting visitors. It was a very good environment.

But then, the next year, I got a splendid offer from Northwestern University. I didn't want to go to Northwestern University. Harvard gave me only a tuition scholarship, which I guess was about the lowest of all, but I decided to go to Harvard anyway, and I'm glad I did. But then I had to make my way there. The nicest thing at Harvard was an Austin graduate teaching fellowship. My first-year roommate, from Washington University in St. Louis, had an Austin teaching fellowship, which was a nice deal. I kept wondering, how did he get an Austin teaching fellowship, when all I could get was a tuition scholarship? I figured I was better trained and just as smart as he was. I ultimately learned that old school ties did it. There was a

professor at Washington University who was highly admired at Harvard, and nobody out here at Caltech was highly regarded by Harvard except possibly Ian Campbell. But after two years at Harvard, I had a top fellowship.

ERWIN: You've talked about this elsewhere. I guess you had to be in the army for a while. And then you went to the Midwest.

SHARP: Well, the first job I could get after graduating was at the University of Illinois…and I was there for five years. Then I took a leave of absence to go into the Army Air Forces, as it was called at that time….My outfit was stationed for three months at the University of Minnesota….I got acquainted with the people at the University of Minnesota in geology at that time. Their geomorphologist—someone interested in surface processes—died of meningitis, and they offered me the job while I was still in the army. Well, I preferred being at Minnesota more than at Illinois, so I resigned my leave of absence at Illinois and went to Minnesota when the war was over.….Suddenly, both Stanford and Caltech out of the blue offered me a job….[Lee A.] DuBridge had just come to Caltech [as president] the year before [1946] and was doing great things here. He moved in very rapidly and took the faculty from a nine-month to a twelve-month salary scale, for example.

ERWIN: How did you know about this? You had enough contacts that you knew what was going on at Caltech?

SHARP: How I knew some particular thing I don't remember, but I was impressed. He wiped out meteorology, which he had to do because Irving Krick was operating a business in meteorology out of the Caltech campus, and Caltech couldn't permit that and still have a tax exemption status. He didn't throw out all the meteorologists, but he told them to start looking for other jobs; when they found one, they would leave. So he wiped out meteorology. But he later on would have liked to re-establish it. At one time he asked me, "Would you people like to get back into meteorology?" I demurred and said that I thought we had other things to do that were more important. We got into planetary science, because both DuBridge and Bob Bacher, who by that time was provost, sent up a smoke signal saying, "Our astronomers are interested in the far-out

things, like galaxies and black holes, and meanwhile they're ignoring the solar system. Would you people be interested in something within the solar system, let's say planetary science?" He didn't say, "Do it." He said, "Maybe we can help you." He didn't promise me anything. But it was a signal. And so on the basis of that, we got started in planetary science, which is now one of our strongest arms.

ERWIN: Well, backing up a little bit to when you came back to Caltech as a faculty member. That was 1947. Was Stock chairman of the geology division?

SHARP: Stock was chairman, but only within the year.

ERWIN: You had of course known Stock before.

SHARP: Yes. I knew Chester as an undergraduate, because he gave us a lecture in elementary geology. Then in my senior year here I had a course in historical geology for which he was responsible, but he didn't do much of the teaching. It wasn't a very good course. I knew him a little bit at that time. Then when I came back I knew him better [laughter] than I had before, for a curious reason. One summer my wife and I were traveling back to Illinois across Wyoming. We ran into Chester Stock and his [second] wife in a Wyoming town. at that time. We'll come back and talk about his wife later. He had run out of funds and wanted to know if he could borrow $20.

ERWIN: Do you mean literally? He didn't even have anything in his pocket? You just met him on the road?

SHARP: [Laughter] It was in a town—in a service station or a motel or restaurant, and we recognized each other. Then with embarrassment he asked me if I had enough money to loan him $20. Of course, I did.

And before I came here as a professor, there was considerable correspondence back and forth between us. When I was at Minnesota I remember him saying in a letter, "If you can get your ear muffs off, I want to talk to you about the possibility of coming out here."

As a young professor and scientist, Stock was very intent on his professional work, almost to the exclusion of everything else. He wanted to make a national reputation. He didn't lecture well nor did he teach well. When I first came to Caltech, a major tragedy in his life occurred: His first wife died—they had two children, I believe. That was a terrible shock to him. He later told me, "I used to wander the streets of Pasadena wondering what will happen to me," while he should have been wondering what was going to happen to his kids. But no, it was "What's going to happen to me?" Not too long after she died, within a year or two, he married a much younger girl, who worked in the athletic department, in an office that was in the basement of Throop Hall. She raised the children. I'm pretty hazy about this. She and Chester eventually started a child of their own, but one of the other Stock children had a childhood disease and the second Mrs. Stock got it and aborted the fetus. That made things complicated for Chester.

As he got older, he became recognized as an outstanding vertebrate paleontologist. Nowadays there aren't very many, so the competition isn't very great. But he was good—no question about it. He got elected to the National Academy of Sciences, and I think he was once president of the Geological Society of America. The bigwigs took him aboard, so to speak. As a result, Chester changed quite a lot. He had a good sense of humor, and it came out as his confidence grew. When I [joined the Caltech faculty], our relationship was very cordial. Soon, however, Chester died [1950], after a cerebral hemorrhage or some cerebral stroke. Once, when I had been here only a couple of years, he came into my office and said, "What are you working on?" I told him. He said, "Work hard on it and finish it up. You don't have too much time." I didn't know what he meant. He knew he was going to die and I was most likely to succeed him as division chairman.

ERWIN: You're quite certain of this?

SHARP: I think he must have been [thinking] that I was the most likely successor and it was going to cut into my time. He never explained; he just said, "You don't have a lot of time. Work hard and get your work out."

ERWIN: The implication is that he must have had some warning.

SHARP: Yes, I think maybe the doctors told him that this was incurable and that it was going to get him sooner or later. He died in his sleep, as I recall.

ERWIN: How old was he?

SHARP: I'd say he was in his sixties, but he wasn't way up there [Chester Stock was fifty-eight at the time of his death—Ed.]. He was a very pleasant person, jovial and relaxed in his later years. Before that he was too intense. He was so focused on his work. Chester did two really monumental studies. One was on the Rancho La Brea fossils—an incredibly rich thing to work with, and he did that very well. A second was the Sespe Formation. Caltech used to own that collection. The Sespe is a formation about 7,000 feet thick of land-laid deposits in the midst of some 60,000 feet of marine sedimentary beds that occupy the Los Angeles and Ventura basins. It's an anomaly. But it had vertebrate fossils in it. Chester tended to focus on big fossils, not rodents, chipmunks, or anything. Big—horses, ground sloths. The Sespe had a lot of big fossils—like hippopotamuses and similar animals. He made himself *the* expert on the Sespe Formation, and he did a good job.

Those were the two big things he worked on. He worked on other faunas too, but he focused on those particularly, and the work was monumental. He published his papers, unlike Buwalda. Actually, the Carnegie Institution published a lot of them. He almost had, in a sense, a private organ for publication that many other people didn't have, because John Campbell Merriam, who controlled the purse strings at the Carnegie Institution in Washington, had supported him.

I don't know if I can tell you any more about Chester Stock. I was very fond of Chester [when I was] a faculty member. As a student, I felt that he was not much interested in education. It was the wrong time of life for him; his personal life had just gone to pot.

There's no question that as he got recognition, he felt he had it made. Outstanding figures in geology began almost patronizing him, you might say.

ERWIN: Caltech has some portion of his papers, but not by any means all of them. I think a lot of them are at the Page Museum. [Tape ends]

Begin Tape 2, Side 2

ERWIN: Did you ever go to the Rancho La Brea or the Sespe site and look at what was going on there? Did you see the digging?

SHARP: Stock took us once, when I was an undergraduate or graduate. As a graduate student, I took a course in vertebrate paleontology that Chester Stock was supposed to teach, but his teaching assistant taught almost a hundred percent of it. Stock would come in once in a while. He did take us to Rancho La Brea and described and pointed out some special things. I know I was over there with Stock; whether it was as a graduate student or as an undergraduate I don't remember.

ERWIN: Did you actually get down in the ground and dig things out?

SHARP: No. We looked at some of the fossil pits, but we didn't do any digging.

ERWIN: Well, I've seen somewhere—perhaps this was at the Hancock Foundation down at USC—pictures of the Rancho La Brea dig. I know there was a lot of volunteer work done there. In fact, one gets the impression that people were just pulling bones out of the ground any old way and sort of dropping them in heaps. Perhaps I'm under a misconception, but in any case, how was the digging done? Would it have been very different from the way people dig up bones today? I mean, you have to worry about whether you're dislocating something.

SHARP: Well, I think today they are much more careful, partly because of the archaeologists, who save everything. The San Bernardino Museum excavations [at Calico, California,] are striking. I don't know whether you've ever been to any of them or not. If somebody is working there as a volunteer, they have a square yard of ground, and they're responsible for everything that comes out of that square yard. Every bone or trace of bone or artifact is separated and classified. They are very careful. They've got this deep shaft that they've dug down about thirty or forty feet. In profile it looks like a miniature skyscraper, because different people are working

at different [levels]. I think that was never done at Rancho La Brea; I think you're probably right that a considerable amount of bones was wasted, but I had no knowledge of that.

ERWIN: In some sense, it wouldn't matter so much?

SHARP: It wouldn't matter so much because there was so much fossil material. But at Calico, in the desert, everything mattered. The archaeologists are now coming around slowly to the idea that most geologists have always felt anyway—that man has been in North America considerably longer than most archaeologists think. They are terribly conservative. But that's a separate subject. Chester was not into archaeology; that's a different business.

ERWIN: Was he out in the field a lot himself?

SHARP: When he was young, certainly. He did his PhD work in the John Day fossil beds in Oregon. From there comes one interesting story. This is a dry, desert kind of country, and Chester was wandering back and forth, looking for fossil remains. He had a little chant he chanted repeatedly: "Dear Lord, please grant your servant just one little bone, just one little chip, just one little fragment. I'd be ever so grateful if you would…."

As I say, I think Chester always had a sense of humor. It didn't really come out full force until he relaxed and he could say, "I've got it made and now I can afford to enjoy things." Chester's devotion to his first wife, whom I never knew—she died before I got to know Chester—I think must have been very deep and very sincere. His relationship with his second wife was probably much more tenuous. I'm not surprised, because I don't think his second wife had much in common with Chester.

ERWIN: We left Chester praying for a bone up at the John Day fossil beds. But as a young man, you were saying, he was very much in the field?

SHARP: Oh yes. He collected in the field. This was before he got to be a professor at Caltech.

ERWIN: We have, in his collection here, field notebooks. And you know there has been discussion about the field notebooks and whether they belong with the bones or not. The fact is the field notebooks we have are all from his early period. They go up to 1920 only. What I'm asking is, did he keep field notebooks, do you think, after he came to Caltech?

SHARP: On that I haven't the faintest idea. I don't think he spent a lot of time later in the field. You see, now he could afford professional collectors. He had—I wouldn't say a crew—but he had experienced people who collected for him.

ERWIN: So that's how it worked? He sent people out into the field?

SHARP: Yes. He directed the work of collecting and probably didn't do much himself. When he was a graduate student at Berkeley, he was doing all his own fieldwork and all of his own collecting, even his own preparations. But once he got financial support and could hire people to do that type of thing, he directed, identified, and published the results.

ERWIN: And he was comfortable doing that?

SHARP: Of course, it was normal in that profession.

ERWIN: Who was Eustace Furlong? He was connected with Stock.

SHARP: That's right. In his later years, Eustace Furlong was Chester Stock's righthand man. He was the principal field collector. The other person who was very important was Bill [William J. P.] Otto. You've seen the *Smilodon*—the saber-toothed tiger over in [the geology division]? Sitting alongside that gigantic skeleton is this wooden *Smilodon* that was sculptured by Bill Otto, who was basically a frustrated sculptor. He's still alive, and served for years as Stock's principal preparator. He's about ninety-four now and lives over in New Mexico. Normally I would have been able to give you an address for Bill, except about three weeks ago someone stole a valise out of the car I was driving which had my address book in it, among a lot of other things. It's like suddenly not having a knife or a spoon to eat with. I'm slowly building it up again. The

only virtue is that you get rid of a lot of phone numbers you didn't need. I've lost Bill Otto's, but I can recover that. I have a source, I think.

ERWIN: Well, that's amazing. He's been around for a long time. Did he do the preparation of the fossils?

SHARP: He did the preparation. He did sculptures too, so he knew how they ought to go together. He was very careful. Then when Chester died, we took Bill onto our division staff as a handyman to do any kind of work. He was Teutonic in his outlook, but he had spent time in South America. He was always drinking maté.

ERWIN: Oh, I don't even know what that is.

SHARP: Well, it's like tea—hot water and dried leaves. I have contact with Bill once in a while, not very often, though.

ERWIN: The things that were collected by Furlong and prepared by Bill Otto—these were not a part of the museum?

SHARP: No, they mostly weren't museum items; they were science collections. That was what the Los Angeles Natural History Museum bought.

ERWIN: Were they collected using the special funds that Chester Stock had?

SHARP: Otto may have had some support from the Carnegie Institution. I don't know that. I wouldn't be surprised but that he did have.

ERWIN: So these collections started from the time that Otto came?

SHARP: I doubt that early. Preparators not known to me may have had an earlier hand in them.

ERWIN: And when they were sold, the item number was about 55,000 specimens.

SHARP: I don't know that. We got only about $100,000 for it. I remember that's how we started the construction of geochemical facilities: a curious switch—we went from vertebrate paleontology to geochemistry. We had to drill holes in concrete floors; we had to convert a building that had never had a chemistry laboratory or anything in it into a building that has a number of chemistry laboratories, and that involved drilling holes in concrete floors and walls and everything else for years. And a lot of the money went to that purpose.

ERWIN: Whose idea was it finally to sell Chester Stock's vertebrate fossils?

SHARP: I think it was the Los Angeles County Museum that wanted to buy them. I was division chairman at that time. We had spent about seven years looking for somebody to succeed Stock. That had not been my choice, but I had to placate Heinz Lowenstam, who had come from Chicago. Harrison Brown, whose name you must know, had written two unauthorized letters to Heinz Lowenstam promising him the moon, practically, to get him to come here.

ERWIN: To be your paleontologist? Is that essentially what it was?

SHARP: Paleoecologist.

ERWIN: I know he did have that special title—ecologist. But in a sense, he would replace Chester Stock. Is that what you're saying?

SHARP: No, Lowenstam did not replace Stock. We gave up vertebrate paleontology. Stock was not replaced. We had had invertebrate paleontologists here, going back to Wendell Woodring, who would have been great, but Buwalda turned him off after hiring him. We'd had two other younger people—Frank Stehli and David M. Raup, both of whom left of their own volition. They were invertebrate paleontologists, and we had wanted to replace the invertebrate paleontologists. Instead, we got Heinz Lowenstam, which was great. He was in paleoecology and that suited us well. It's a different way of getting at paleontology.

Stock was always by himself. When Stock died, we made an honest effort to find somebody else in vertebrate paleontology. I think it's just as well we didn't succeed, because we then got into geochemistry instead, which has been a very successful endeavor for us.

The Los Angeles County Museum realized that we weren't going to find a vertebrate paleontologist. Vertebrate paleontologists throughout the country used to come and stand in front of me and curse me from top to bottom. There were so few centers for vertebrate paleontology, and I was wiping out one of them. Oh, boy! Some of those people had been my good friends, up to that point. But I thought it was the thing to do. I was not solely responsible, but I was a lightning rod for them to strike at.

ERWIN: You must have had some support from your division to do this.

SHARP: Yes, of course. Seldom does any division chairman do something single-handed.

We made an honest effort for seven years to try to find a vertebrate paleontologist. Heinz Lowenstam was the principal one who kept beating on me that I was not doing things the way he wanted. I finally said, "We've spent seven years futilely. We're never going to find the right person. We can take the money that the museum is now offering us." It is clear that Caltech is not a good place for the field; even biologists don't give a hoot for vertebrate paleontology. I used to say to George Beadle, who was chairman of the biology division, "George, I'm going to get myself a jackrabbit. I'm going to grab him by the ears and I'm going to go around in your department and shove him into the faces of all your professors and say, 'Now what is this?' and they won't know." He said, "I don't want them to know. I want them to know modern molecular biology."

ERWIN: Oh, how funny!

SHARP: Well, they were doing what they should be doing. They were moving into molecular biology, which is where they belonged. It's where they should be. Caltech was not really a good place for Chester Stock to have come to. The Southern California location was fine, but in terms of colleagues and people to interface with, no.

ERWIN: He certainly had strong ties with the Natural History Museum.

SHARP: Yes, indeed. They had vertebrate paleontologists over there.

ERWIN: My impression was that he spent a good portion of his time there, and with their fossil collections.

SHARP: Yes, that's where he had people he could talk to who understood. That's exactly right. That is not really Caltech's area, you see. He was a loner. He was respected on campus, and liked by a few people that knew him well enough to understand what kind of a guy he was. He was certainly treated richly by Millikan. But if you stood back and said, "What is Caltech doing in vertebrate paleontology? Why do they have a vertebrate paleontologist?"—it was totally foreign to the things we are doing, except perhaps for the fact that we have several field geologists. Stock did attract a few students, and several have become outstanding vertebrate paleontologists, but he didn't have many. One of them was Paul Henshaw. In the early days we had a requirement: You had to have a major and a minor in your PhD work. Paul Henshaw, who majored in vertebrate paleontology and minored in economic geology—mining geology—couldn't get a job in vertebrate paleontology. But he was hired by Homestake Mining Company and later became their president. Other students fascinated by vertebrate paleontology were less fortunate. Jobs were scarce.

ERWIN: So the collection was sold [1957], and then you were able to take this money and build labs. I have looked at the inventory, which isn't much of an inventory, exactly. It doesn't list things bone by bone. But there is a note in the trustee minutes from 1957 saying that there were 55,000 specimens…

SHARP: I haven't the faintest idea.

ERWIN: …of which 50,000 were unidentified. So only 5,000 were identified. You know, this was the basis of the problem that we subsequently had with the museum. They felt that the records that would have identified those 50,000 specimens were somehow still at Caltech.

SHARP: To the best of my knowledge, there is no clear evidence that the bits and pieces were ever identified. A lot of that material is little slivers of bone that were maybe thought to be the shin bone of a horse or something like that, but they don't really know. So they are unidentified. I don't put much stock in that 50,000 figure. I don't know where it came from.

ERWIN: It certainly could be erroneous, but it is listed that way in the trustee minutes. You know, later the museum came back to Caltech and asked to have documentation for the collection.

SHARP: They wanted field notebooks.

ERWIN: Field notebooks—of which we were never able to find any.

SHARP: That's true, because they probably never existed.

ERWIN: I think that was the theory that eventually evolved.

SHARP: Chester operated independently. As a matter of fact, after he left Berkeley, I bet that Chester never made any real serious field notes.

ERWIN: Do you think someone like Eustace Furlong might have had all sorts of field notes and they have just stayed with him?

SHARP: I doubt that. We're talking about field notes and not about some laboratory data. You know, I was division chairman during the time the sale was consummated. I don't remember any details about the affair except that we couldn't find a trace of books of any kind.

ERWIN: OK. Well, you know, today with the push into—I guess we'll call it geobiology for the moment, although I'm not entirely sure what that means—the current chairman of the geology division [Edward M. Stolper] has referred to the Natural History Museum as a sister institution,

and there's this feeling that there's going to be a big rapprochement between Caltech and the museum now. Is this something really new, or are we just sort of, in a sense, going back?

SHARP: "Geobiology" is a general term for any mixture of geological and biological endeavors. The museum has been, up to now, more geobiologically oriented than Caltech. What the future holds, I don't know. I hope our latest hire, Dianne Newman, is our new thrust into geobiology. Joe Kirschvink is about the only one in our division that could be classified as a geobiologist. His geobiology has been to tell you whether whales have magnetite in their brains and so on.

ERWIN: He was a student of Lowenstam, right?

SHARP: As an undergraduate, yes. He was a protégé of Lowenstam's, more than a student. But he was a great admirer of Heinz. Lowenstam got into the biological origin of minerals, like magnetite. Joe is in that same area. He and his wife are trying to solve [those kinds of problems]. How can honeybees go out and do what they do and come back to the right place? And whales and so forth. Magnetite in the brain may have a lot to do with that. Of course, we've got magnetite in *our* brains, too.

We just hired another microbiologist—that's Dianne Newman. Microbiology is getting to be very important. There's a big hassle between companies that want to use the hot springs in Yellowstone Park. There are all kinds of microorganisms in those hot-spring environments, some of which are very important to these companies. The environment in which they are living is a geological environment, but it consists of more than hot water; there's a lot of geochemistry involved. Can you artificially create the environment that exists in natural hot springs like Agua Caliente and others in Yellowstone and build these things so that you don't have to go to the park?

ERWIN: To harvest [these organisms]?

SHARP: Yes. It's tricky in a national park, when people want to exploit it commercially. If somebody could say, "Look, I know what you need to make a hot spring all of your own

somewhere"—that's a geological-chemical problem. You can plant your microbes in it and that's a microbiology spring!

One of the things the biologists and others are very interested in is ancient DNA. When I first started talking about this, biologists said the oldest known [DNA] was about 35,000 years: "Anything beyond that has been destroyed by heat; it doesn't exist." The date since then has been pushed back and back; we finally got it back to about 35 million years at a Utah locality. And I said, "Gee, you guys said thirty-five thousand years and now we're back to thirty-five million!" I have even heard of a possible 50 million years. It's becoming basically a geological problem.

Where would I look for really ancient DNA? Well, I have a seemingly absurd candidate. I haven't the faintest idea or hope that it would ever yield anything, but it's a geological approach: It's in the eastern Grand Canyon, where we have protozoic sedimentary rocks that are at least 700 million years old. If they have chert in them—chert nodules, and I don't know that they have—maybe the precursor DNA would be in that chert, which is like amber, very tight. Besides the tightness of the system, we want something that's never been too hot, because heat will ruin DNA. I've seen truly old sedimentary shale beds in the eastern Grand Canyon that are soft and unconsolidated and apparently have never been deeply enough buried to get hot enough to be metamorphosed. I'd get out there and start scouting around, looking for unfractured chert nodules. If there is any unaltered primordial DNA, that's where it might be. Thanks to the stable isotope work that Sam Epstein does on organic materials, we have a capability in our geology department to work with stable isotopes—a capability that doesn't exist anywhere else on the Caltech campus. We've got about nine or ten mass spectrometers, the equipment needed to work with very old samples. We can do a lot with the biologists, if they are interested.

ERWIN: So this is an outgrowth of geochemistry in a sense?

SHARP: Yes. It uses geochemical know-how and equipment. We were lucky we had our geochemistry going when planetary science started and they began returning samples from the moon. That's where our geochemists came in. We got heavily into planetary science partly through geochemistry. I think we can get heavily into biogeology, involving geology and biology, through our geochemistry stable-isotope gang. I think we ought to do it.

You know, it's very interesting. It's much easier to start something new than it is to stop doing something old. I've met Baltimore only very briefly, and I haven't really talked with him. He wants Caltech to diversify. You don't necessarily have to get bigger to diversify; just stop doing something so you can do something new. But it's very hard to stop doing something.

ERWIN: But Caltech has had the reputation of being able to do that. That's the formula.

SHARP: Well, OK. I hope we can do it. I think it's the way to go. We stopped doing vertebrate paleontology to do geochemistry, in a sense. That proved to be the right thing, there's no question about that. Even my most severe critics would say, "Yeah, that was the right thing to do, although we abused you for doing it."

ERWIN: It took some years for them to stop abusing you?

SHARP: They'd come to me and, sneering, they'd say, "How is the department of geochemistry at Caltech?" And I'd say, "Well, give us fifteen to twenty years." In twenty years, they were trying to hire our geochemistry graduate students. [Tape ends]